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1767

THE

PHILOSOPHICAL TRANSACTIONS

(From the Year 1719, to the Year 1733)

ABRIDGED,

AND

Diſpoſed under General HEADS.

In Two VOLUMES.

VIZ.

VOL. VI. Containing	VOL. VII. Containing
PART I. The MATHEMATICAL Papers.	PART III. The ANATOMICAL and MEDICAL Papers.
PART II. The PHYSIOLOGICAL Papers.	PART IV. The PHILOLOGICAL and Miſcellaneous Papers.

By Mr JOHN EAMES, *F. R. S.*

AND

JOHN MARTYN, *F. R. S.* Profeſſor of *Botany*
in the Univerſity of *CAMBRIDGE*.

LONDON:
Printed for J. BROTHERTON, J. HAZARD, W. MEADOWS,
T. COX, W. HINCHLIFFE, W. BICKERTON, T. ASTLEY,
S. AUSTEN, L. GILLIVER, and R. WILLOCK. 1734.

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TO THE

READER.

THE Abridgment of the PHILOSOPHICAL TRANSACTIONS, begun by Mr *Lowthorp*, and continued by Mr *Jones*, has been already ſo well received, that there is no occaſion for me to trouble the Reader with a particular Account of the preſent Deſign. I believe I need only inform, him that I have endeavoured to follow the Method laid down by thoſe Gentlemen, that I might make the whole Work appear as uniform as poſſible. I have ventured to differ from them in this; that whereas they have omitted a conſiderable Number of Papers, I have totally omitted none. For I thought it would be too great a Preſumption in me, publickly to declare any Paper to be uſeleſs, which had been read before the ROYAL SOCIETY, and permitted by them to be publiſhed. I have differed from them alſo in inſerting the *Accounts of Books*, many of which are as entertaining and inſtructive to a great number of Readers, as any other Paper.

I have endeavoured to ſhorten the Papers that came under my Hand, in ſuch a manner as not to hurt their Senſe; and I flatter myſelf that very few (if any) of the Learned and Ingenious Authors of them will accuſe me of having done them any Injury.

The three firſt Chapters of this Abridgment were done by (that Learned and Judicious Member of the ROYAL SOCIETY) Mr *EAMES*; to whom alſo the Reader is obliged for the Communication of the late learned Profeſſor *Gregory*'s Diſcourſe upon Motion, printed at the beginning of the Chapter of Mechanics.

In the Catalogue of Plants from the Botanic Garden at *Chelſey*, I have inſerted fifty more than are contained in the *Philoſophical Tranſactions*, within the Compaſs of this Abridgment. But as theſe fifty Plants had been preſented to the Society; as they belonged to the Year 1731; and as by this means the even number of five hundred was compleated, I thought I ſhould oblige the curious in Botany by inſerting them.

If the Public receive this Work favourably, it will be eſteemed an ample Recompence for many Months aſſiduous Application. And if the Readers meet with that Satisfaction, which I have endeavoured to give them, it will be a very great Pleaſure to their humble Servant,

Chelſey, June 8. 1734.

John Martyn.

It being of ſome uſe to know the Dates of the ſeveral Papers, the following TABLE *will ſhew for what Months each Tranſaction in this Abridgment was publiſhed.*

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THE
Philoſophical Tranſactions
ABRIDG'D.

PART I.

CONTAINING THE

Mathematical PAPERS.

CHAP. I.

ARITHMETIC, ALGEBRA, FLUXIONS, GEOMETRY, *the* QUADRATURE *of* CURVES.

I. THE Uſefulneſs of this Arithmetic conſiſts in this, that it performs all the Operations with more Eaſe and Expedition than the common Affirmative Arithmetic, eſpecially in large Numbers: And it differs from the common Arithmetic chiefly in this, that it admits of Negative Figures promiſcuouſly with the Affirmative.

A Summary of Negativo-Affirmative Arithmetic, by J. Colſon, *F. R. S.* Nº 396. p. 161.

Notation.] Theſe Negative Figures are diſtinguiſh'd from the Affirmative, by the Sign - placed over them.

Thus $3\bar{7}09\bar{2}\bar{8}65\bar{7}3\bar{9}\bar{6}\bar{1}47\bar{2}$ is one of theſe Numbers, whch may be converted into its Equivalent common Number 2308726432039468, in this manner:

$$\begin{array}{rl} & 3009006503000470 \quad \textit{Affirmative Figures.} \\ - & \underline{0700280070961002} \quad \textit{Negative Figures.} \\ & 2308726432039468 \end{array}$$

(1.) Write down all the affirmative Figures by themſelves, putting a Cypher in the Place of every negative Figure. (2.) Write down all the negative Figures by themſelves, putting a Cypher in the Place of every affirmative Figure. (3.) Subſtract the laſt Number from the firſt, and the Remainder will be a common Number, equivalent to the given negativo-affirmative Number. See the Operation above.

Reduction, Caſe I.] But the readieſt practical Way of performing this Reduction in any given Number, will be in this Manner: Begin at the left Hand; and going over all the Figures in order, obſerve theſe Rules. (1.) An affirmative Figure before a Negative muſt be diminiſh'd by an Unit. (2.) A negative Figure before an Affirmative, muſt be changed into its Complement to 10. (3.) A negative Figure before a Negative, muſt be changed into its Complement to 9. All other Figures muſt remain unchanged, and a Cypher is always to be underſtood where there is no ſignificant Figure. The Sign of the Cypher is neglected; but where there is occaſion to conſider it, it is always ſuppos'd the ſame as the Sign of the following Figure. Thus the negativo-affirmative Number $72\bar{9}\bar{5}86\bar{4}\bar{5}\bar{5}98\bar{2}00\bar{1}\bar{7}30$ is immediately reduc'd to 710585545977998330; and ſo of all others.

Reduction, Caſe II.] But on the contrary, common Numbers may be reduced to negativo-affirmative Numbers a great Variety of Ways, by ſubſtituting inſtead of the Figures 1, 2, 3, 4, 5, 6, 7, 8, 9, their reſpective Values $1\bar{9}$, $1\bar{8}$, $1\bar{7}$, $1\bar{6}$, $1\bar{5}$, $1\bar{4}$, $1\bar{3}$, $1\bar{2}$, $1\bar{1}$, in any Places at Pleaſure. But the moſt uſeful Reduction of this Kind is what I call *A Reduction to ſmall Figures*, which conſiſts in throwing out all the large Figures, 9, 8, 7, 6, out of any given Number, and introducing in their Room the equivalent ſmall Figures $1\bar{1}$, $1\bar{2}$, $1\bar{3}$, $1\bar{4}$, reſpectively. Thus 182937462 may be reduc'd to $2\bar{2}3\bar{1}4\bar{3}5\bar{4}2$, conſiſting only of ſmall Figures. But this Reduction may be perform'd more readily by theſe *Rules* following.

(1.) A ſmall Figure before a large Figure muſt be increaſed by an Unit. (2.) A large Figure before a large Figure muſt be changed into its negative Complement to 9. (3.) A large Figure before a ſmall Figure muſt be changed into its negative Complement to 10. Other Figures are not to be changed; and 5 will be ambiguous, being to be eſteemed either large or ſmall, according as the Figure following is either large or ſmall. Some Examples of this Reduction ſhall here follow, both in whole Numbers and Decimal Fractions.

$$37068259764 = 4\bar{3}1\bar{3}\bar{2}3\bar{4}0\bar{2}\bar{4}4$$

$$729528960739957 = 1\bar{3}3\bar{1}53\bar{1}0\bar{4}1\bar{3}400\bar{4}\bar{3}$$

$$9260872395,87294 = 1\bar{1}3\bar{4}1\bar{1}\bar{3}240\bar{4},\bar{1}\bar{3}3\bar{1}4$$

$$\text{Or } (9)\ 926087239587294 = (10)\ 1\bar{1}3\bar{4}1\bar{1}\bar{3}240\bar{4}\bar{1}\bar{3}3\bar{1}4$$

$$(m)\ 387916407953,\&c. = (m)\ 4\bar{1}\bar{2}\bar{1}2\bar{4}41\bar{2}\bar{1}53,\&c.$$

It is to be obſerved, that in this laſt Example the Numbers are what I call *interminate*, or Approximations only; that is, the firſt and moſt valuable Figures are expreſs'd, and all the reſt (whether finite or infinite in Number, whether known or unknown) are omitted as inconſiderable, and inſinuated by the Mark, *&c.* Alſo the Index *m* before the Number ſtands for ſome Integer, expreſſing the Diſtance of the firſt Figure 3 or 4 from the Place of Unites; which Integer is either affirmative or negative, according as the ſaid firſt Figure ſtands in integral or fractional Places. The Example immediately before is a particular Inſtance of this.

And thus much by way of Notation: To proceed therefore to the Operations to be perform'd with theſe Numbers, whether reduced to ſmall Figures or not; and firſt of *Addition.*

Addition.] *Place the Numbers to be added juſt under one another, obſerving the Homogeneity of Places, as in common Numbers. Then beginning at the Right Hand, collect the Figures in the firſt Row or Column, according to their Signs, and place the Reſult underneath:* And ſo ſucceſſively of all the other Columns, as in Example 1.

But if at any Time this Reſult cannot be expreſs'd by a ſingle Figure, it may be writ down with two or more Figures, obſerving the Homogeneity of Places, and then the Sum may be collected over again. But to ſave this Trouble, it will be ſufficient to reſerve the Figure in mind which belongs to the next Column, and collect it with the Figures of that Column; as in Example 2, 3.

If the Numbers to be added are reduced to ſmall Figures, as in *Example* 3, their Addition will be very ſimple, and the Sum may alſo be exhibited in ſmall Figures, by an eaſy Subſtitution of Equivalents, where there is occaſion.

Example (1.)

$$\begin{array}{r} 2\bar{5}7\bar{3}\bar{8}42\bar{6}3 \\ 70\bar{9}\bar{8}2\bar{1}\bar{3}70 \\ 580\bar{7}30\bar{5} \\ \hline 9\bar{5}3\bar{3}\bar{6}\bar{4}21\bar{2} \end{array}$$

Example (2.)

$$\begin{array}{r} 64\bar{7}03\bar{9}\bar{6}\bar{8}\bar{2} \\ 4\bar{9}\bar{8}\bar{2}\bar{7}36\bar{5}1 \\ 81\bar{9}\bar{4}03\bar{7}\bar{6}5 \\ \hline 18\bar{6}\bar{4}\bar{6}\bar{4}\bar{3}\bar{8}\bar{9}4 \end{array}$$

Example (3.)

$$\begin{array}{lr} (m) & 2\bar{1}5\bar{3}\bar{1}40\bar{4}3\bar{1}\bar{2}\bar{1}3,\ \&c. \\ (m) & 50\bar{4}\bar{2}03\bar{1}\bar{4}255\bar{1}\bar{2},\ \&c. \\ (m-1) & 4\bar{3}10\bar{2}\bar{3}10\bar{2}\bar{4}\bar{1}\bar{3},\ \&c. \\ (m-2) & 5\bar{1}3\bar{4}\bar{2}1\bar{1}03\bar{2}1,\ \&c. \\ (m-3) & 2\bar{1}30\bar{4}2\bar{1}03\bar{2},\ \&c. \\ (m-4) & 1\bar{3}\bar{2}0212\bar{2}2\bar{4},\ \&c. \\ (m-5) & 13\bar{2}\bar{2}43\bar{1}5,\ \&c. \\ \hline (m+1) & 1\bar{3}33\bar{3}214\bar{1}\bar{3}\bar{4}3\bar{1}\bar{2},\ \&c. \\ \hline \end{array}$$

Subtraction.] *Subtraction* in this Arithmetic is reduced to Addition, *by changing all the Signs of the Number to be ſubtracted.*

Thus, if from *(n)* $7\bar{2}\bar{9}38\bar{4}2\bar{9}63\bar{7}$, &c. we are to ſubtract *(n-2)* $8\bar{1}07\bar{3}\bar{5}9\bar{2}6$, &c. the Remainder will be found as in Example 4.

Example (4.)

$$\begin{array}{lr}
(n) & 7\bar{2}\bar{9}38\bar{4}2\bar{9}63\bar{7},\ \&c. \\
(n\text{-}2) & \bar{8}10\bar{7}35\bar{9}2\bar{6},\ \&c. \\
\hline
(n) & 7\bar{3}\bar{7}47\bar{1}5\bar{4}\bar{3}4\bar{3},\ \&c. \\
\hline
\end{array}$$

Thus in all Caſes will Addition and Subtraction be eaſily performed: But the chief Uſe of this Method will be, to eaſe the Trouble of prolix *Multiplications.* And here, as well as in Diviſion, the firſt and moſt valuable Figures may be firſt found, and conſequently the Product may be continued to as many Places as ſhall be required, without finding any unneceſſary Figures; which is a Convenience not to be had in the ordinary Way of Multiplication.

Multiplication.] Let it be propoſed to Multiply together the Numbers 8605729398715 and 389175836438, which reduced to ſmall Figures will be $1\bar{1}\bar{4}1\bar{4}\bar{3}3\bar{1}40\bar{1}\bar{3}15$ and $4\bar{1}\bar{1}2\bar{2}\bar{4}\bar{2}4\bar{4}44\bar{2}$. Write down theſe two Numbers one under the other upon a Slip of Paper, with the Figures at equal Diſtances, and then cut them aſunder. Take either of the Numbers for a Multiplier, and place it over the other in an inverted Poſition, ſo as its firſt Figure may be juſt over the firſt Figure of the Multiplicand.

Moveable Multiplier

$4\bar{1}\bar{1}2\bar{2}\bar{4}\bar{2}4\bar{4}44\bar{2}$

Multiplicand

$$\begin{array}{rr}
 & 1\bar{1}\bar{4}1\bar{4}\bar{3}3\bar{1}401315 \\
\hline
 & 4\bar{5}\bar{6}1\bar{7}\bar{9}1825\bar{4}\bar{9}8\bar{6}450\bar{6}\bar{2}\bar{6}0\bar{6}080 \\
 & 11\bar{1}\bar{1}3\ \ 112\bar{4}\ \ 2\bar{6}32\ \ \bar{1}\ \ \bar{2}311\bar{1} \\
\hline
\textit{Product} = & 4\bar{6}\bar{5}0\bar{8}\bar{6}19370\bar{9}\bar{6}0\bar{1}70\bar{7}\bar{2}\bar{0}\bar{2}\bar{3}170
\end{array}$$

Then Multiply theſe two firſt Figures together, and their Product ($4 \times 1 = 4$) place underneath. Then move your Multiplier a Place forward, ſo that two of its firſt Figures may be over two of the firſt Figures of the Multiplicand; and collecting their two Products ($4 \times \bar{1} + \bar{1} \times 1 = \bar{5}$) put their Reſult underneath in the next Place. Move the Multiplier a Place forward; and collecting the three Products ($4 \times \bar{4} + \bar{1} \times \bar{1} + \bar{1} \times 1 = 1\bar{6}$) put the Reſult underneath, as in the Example. Move the Multiplier, and collect the four Products ($4 \times 1 + \bar{1} \times \bar{4} + \bar{1} \times \bar{1} + 2 \times 1 = 11$) which write underneath as before. And ſo proceed by one Stop at a Time, as long as any Figures of the Multiplier can be over any Figures of the Multiplicand. *Laſtly*, Collect the Product into one Line, which being reduced to a common Number will be 3349141936903996927377170.

From this Proceſs it may be obſerved, that at every new Situation of the moveable Multiplier, thoſe Figures only are to be multiply'd together, each by each, as are found over one another. And this Multiplication is to be perform'd, and the ſeveral Products collected, according to the Rules of Specious Multiplication, wherein like Signs will make +, and unlike Signs will make — in the Product. This will always make the Products deſtroy one another, or at leaſt will depreſs and keep them low, and the Figures themſelves being always ſmall, the Reſult will be always ſmall, and often but a *ſingle* Figure, which is the great Compendium of this Method.

When an Approximation only is deſir'd, or when the Product is to be produced to a given Number of Places, the Operation may be continued one Place farther, in order to obtain ſo many Places true as are required. For ſeldom any Correction extends beyond the Place immediately aforegoing, and that is generally corrected but by an Unite, and very often needs no Correction at all; which will be of no ſmall Convenience in the Multiplication of Decimal Fractions.

In this Method we may (if we pleaſe) begin the Proceſs of Multiplication from the loweſt Places, or from the right Hand, as is uſual in common Arithmetic, and then the Correction may be carry'd on continually to the next Place, and ſo the Product may be always comprehended in one Line, without the Uſe of any Superfluous Figures. Of this I ſhall give an Inſtance in the foregoing Example.

Moveable Multiplier

$$\rotatebox{180}{$4\bar{1}\bar{1}2\bar{2}\bar{4}2\bar{4}44\bar{2}$}$$

Multiplicand

$$1\bar{1}\bar{4}1\bar{4}\bar{3}3\bar{1}40\bar{1}\bar{3}15$$

3349141936903996927377170 = *Product*

Place the moveable Multiplier inverted in ſuch a Manner, as that its laſt Figure $\bar{2}$ may be juſt over 5 the laſt Figure of the Multiplicand. Multiply theſe together ($\bar{2} \times 5 = \bar{1}0$) and ſet down the laſt Figure of the Product 0 juſt under, reſerving the firſt Figure $\bar{1}$ for the next Place. Then move the Multiplier a Place forward, ſo that two of its laſt Figures may be over two of the laſt Figures of the Multiplicand, and then multiplying and collecting, you will have $\bar{1} + \bar{2} \times 1 + 4 \times 5 = 17$. Set down 7 in the next Place of the Product, and reſerve 1. At the next remove, you will have $1 + \bar{2} \times \bar{3}$ + 4

$+4\times 1+4\times 5=31$. Set down 1 and carry 3. Then $3+\bar{2}\times\bar{1}+4\times\bar{3}+4\times 1+\bar{4}\times 5=\bar{2}\bar{3}=\bar{3}7$. Set down 7 and carry $\bar{3}$. Then $\bar{3}+\bar{2}\times 0+4\times\bar{1}+4\times\bar{3}+\bar{4}\times 1+4\times 5=\bar{3}=\bar{1}7$. Set down 7 and carry $\bar{1}$. And ſo proceed as long as there can be any Figures over one another, and the Product will be found as before.

This way of Multiplication is ſo eaſy, and may be made ſo familiar by a little Practice, that it will be but little ſhort of *Multiplication by Inſpection*; and will doubtleſs ſeem very ſurprizing to thoſe who are only acquainted with the common tedious Way of Multiplication: eſpecially, if we content our ſelves with a mental Preparation of the Numbers given, or only mark thoſe Figures that are to be changed, which by ſome Practice is eaſily attained.

The firſt of theſe two Ways of Multiplication will be moſt convenient for interminate Numbers. As if we were to multiply (m) 307149741748, *&c.* by (n) 183609712649, *&c.* the Product will be found (m+n) 563956758222, *&c.* as may appear from the Proceſs following.

$$\begin{array}{r} \text{(n) } 2\bar{2}4\bar{4}10\bar{3}13\bar{4}5\bar{1}\text{, \&c.} \\ \text{(m) } 31\bar{3}150\bar{3}42\bar{3}5\bar{2}\text{, \&c.} \\ \hline \text{(m+n) } 6\bar{4}40\bar{5}7\bar{2}\bar{5}83\bar{7}\bar{8}\text{, \&c.} \end{array}$$

Here the Index of the firſt Figure of the Product will be m+n, or the Sum of the Indexes of the given Numbers; but it would have been m+n+1 if there had been any Increaſe from the Product of the two firſt Figures, or if there had been any Correction to have been made to the Cypher, which is underſtood before the firſt Figure of the Product.

When both the Numbers to be multiply'd are interminate, as in the laſt Example, they ought to conſiſt of the ſame Number of Places, or otherwiſe the greater Number muſt be reduced to the leſſer, by cutting off the ſuperfluous Places: and the Product is not to be continued beyond the ſame Number of Places. If but one of the Number is interminate, the other muſt be reduc'd to the Form of an interminate Number, either by cutting off the Exceſs of Places if it has more, or by ſupplying or ſuppoſing Cyphers, if it has fewer Places than the interminate Number. Then the ſame Reſtrictions will take place as before.

Diviſion.] The Method of *Diviſion* in this Arithmetic will not be ſo ſimple or expeditious as Multiplication. After a Tryal of ſeveral Ways, I think this following will be the moſt commodious. Reduce the Dividend and Diviſor to ſmall Figures, and form a Tariffa or Table of all the Multiples of the Diviſor as far as 5. Compare theſe Multiples with the Dividend, and with the ſeveral Remainders after the Multiples have been ſubtracted, by which Means you will diſcover the ſeveral ſmall Figures and their Signs, to be put ſucceſſively in the Quotient.

To form the Table of Multiples, ſet down the Diviſor above, drawing a Line, under which ſet down the Diviſor over again, putting 1 over againſt it. Add theſe two together according to the Rule for Addition in ſmall Figures, and put 2 over againſt their Sum. Add this laſt and the Diviſor together, and put 3 over againſt their Sum. Then add this laſt and the Diviſor together, and put 4 over againſt their Sum. *Laſtly*, Add this and the Diviſor together, puting 5 over againſt their Sum. Thus will you have a Table of all the Multiples of the Diviſor, as far as will be neceſſary.

Thus for Example, if $(m+n)$ 563956758222, &c. $= (m+n+1)$ $1\bar{4}\bar{4}40\bar{4}\bar{3}\bar{2}\bar{4}\bar{2}222$, &c. is to be divided by (n) 183609712649, &c. $= (n)$ $2\bar{2}4\bar{4}10\bar{3}13\bar{4}5\bar{1}$, &c. the Proceſs will be as here follows, by which the Quotient will be found (m) $31\bar{3}150\bar{3}42\bar{3}5\bar{4}$, &c. $= (m)$ 307149741746, &c.

TABLE of Multiples.

$$\begin{array}{r|r}
\multicolumn{2}{r}{(n)\ 2\bar{2}4\bar{4}10\bar{3}13\bar{4}5\bar{1},\ \&c.} \\ \hline
1 & (n)\ 2\bar{2}4\bar{4}10\bar{3}13\bar{4}5\bar{1},\ \&c. \\
2 & (n)\ 4\bar{3}\bar{3}22\bar{1}425302\bar{2},\ \&c. \\
3 & (n)\ 551\bar{2}3\bar{1}14\bar{2}\bar{1}5\bar{3},\ \&c. \\
4 & (n+1)\ 1\bar{3}3444\bar{1}\bar{2}51\bar{4}0\bar{4},\ \&c. \\
5 & (n+1)\ 1\bar{1}2\bar{2}05\bar{1}\bar{4}\bar{4}3245,\ \&c.
\end{array}$$

$$\begin{array}{r}
\textit{Quotient} = (m)\ 31\bar{3}150\bar{3}42\bar{3}5\bar{4},\ \&c. \\ \hline
\textit{Dividend} = (m+n+1)\ 1\bar{4}\bar{4}40\bar{4}\bar{3}\bar{2}\bar{4}\bar{2}222,\ \&c. \\
551\bar{2}3\bar{1}14\bar{2}\bar{1}5\bar{3},\ \&c. \\ \hline
1313\bar{2}\bar{4}203\bar{3}5,\ \&c. \\
2\bar{2}4\bar{4}10\bar{3}13\bar{4}5,\ \&c. \\ \hline
\bar{1}5\bar{2}\bar{3}\bar{3}\bar{4}5\bar{1}010,\ \&c. \\
551\bar{2}3\bar{1}14\bar{2}\bar{1},\ \&c. \\ \hline
3\bar{3}5\bar{1}404\bar{1}\bar{1},\ \&c. \\
2\bar{2}4\bar{4}10\bar{3}13,\ \&c. \\ \hline
1\bar{1}1331\bar{3}\bar{2}\bar{4},\ \&c. \\
1\bar{1}2\bar{2}05\bar{1}\bar{4}\bar{4},\ \&c. \\ \hline
\end{array}$$

1534220,

$$\begin{array}{r}
\bar{1}53\bar{4}\bar{2}20,\ \&c.\\
551\bar{2}3\bar{1},\ \&c.\\
\hline
1\bar{2}\bar{4}\bar{4}5\bar{1},\ \&c.\\
\bar{1}3\bar{3}\bar{4}\bar{4}\bar{4},\ \&c.\\
\hline
3205,\ \&c.\\
\bar{3}33\bar{2},\ \&c.\\
\hline
\bar{1}533,\ \&c.\\
551,\ \&c.\\
\hline
1\bar{2}4,\ \&c.\\
\bar{1}1\bar{2},\ \&c.\\
\hline
\bar{1}2,\ \&c.
\end{array}$$

The Index of the Quotient is found by ſubtracting the Index of the Diviſor from the Index of the Dividend, when the firſt Figure or Figures of the Diviſor are not greater than the like Figures of the Dividend. Thus $\frac{(m+n)\ 56,\ \&c.}{(n)\ 18,\ \&c.} = (m)\ 30$, &c. When they are greater, then an Unite muſt be farther ſubtracted from the Index of the Dividend. Thus $\frac{(m+n+1)\ 1\bar{4}\bar{4},\ \&c.}{(n)\quad 2\bar{2}4,\ \&c.} = (m)\ 31\bar{3}$, &c.

Now a little to illuſtrate this Proceſs, it may be obſerved, that the Dividend $1\bar{4}\bar{4}$, &c. or 56, &c. being compared with the ſeveral Multiples of the Diviſor in the Table, it is eaſily perceived that 55, &c. makes the neareſt Approach to it, and therefore its correſponding Figure 3 muſt be made the firſt Figure of the Quotient, which I place juſt over. Under this I place its reſpective Multiple, changing the Signs and collecting, becauſe it ought to be ſubtracted. Then the Remainder 131, &c. being compared with the Table, I find the neareſt to it (whether in Exceſs or Defect) to be $2\bar{2}$, &c. or 18, &c. belonging to 1. This therefore is made the ſecond Figure of the Quotient, and its Multiple with the Signs changed is placed under, and collected with the laſt Remainder. The Reſult, or new Remainder $\bar{1}5\bar{2}$, &c. that is $-\bar{1}52$, &c. or -52, &c. being compared with the Table, the neareſt Multiple is 55, &c. belonging to 3, which 3 therefore is made the next Figure of the Quotient, but with the Sign - over it, becauſe this Remainder is Negative. And for this Reaſon the Multiple 55, &c. is put down in its Place without changing its Signs. The reſt of the Proceſs will be obvious enough, and if any Scruple ariſes about placing the Numbers, it may eaſily be removed by a little Attention to their reſpective Indexes.

In the Diviſion of *interminate* Numbers, the ſame Reſtrictions are to obtain, as are already mention'd in Multiplication.

And this may ſuffice for a ſhort Summary of *Negativo-Affirmative Arithmetic*, as to the ordinary Operations of *Addition*, *Subtraction*, *Multiplication* and *Diviſion*. What Improvements may be had from hence in the *Extraction of Roots*, whether of pure or affected Equations, I ſhall leave to future Inquiry.

I have contriv'd an Inſtrument, call'd *Abacus*; or, *The Counting Table*, which I hope ſhortly to communicate, whereby all theſe Operations may be eaſily perform'd, and long Calculations very much facilitated.

The Number of impoſſible Roots, by Mr. G. *Campbell.* N°. 404. P. 515.

II. Lemma I.] *In every adfected quadratick Æquation* $a x^2 - B x + A = o$, *whoſe Roots are real, a fourth Part of the Square of the Coefficient of the ſecond Term is greater than the Rectangle under the Coefficient of the firſt Term and the abſolute Number, or* $\frac{1}{4} B^2 > a \times A$; *and vice verſa if* $\frac{1}{4} B^2 > a \times A$, *the Roots of the Æquation* $a x^2 - B x + A = o$, *will be real. But if* $\frac{1}{4} B^2 < a \times A$, *the Roots will be impoſſible.*

This is evident from the Roots of the Æquation being $\dfrac{\frac{1}{2} B + \sqrt{\frac{1}{4} B^2 - a \times A}}{a}$, and $\dfrac{\frac{1}{2} B - \sqrt{\frac{1}{4} B^2 - a \times A}}{a}$.

Lemma II.] *Whatever be the Number of impoſſible Roots in the Æquation* $x^n - B x^{n-1} + C x^{n-2} - D x^{n-3} +$ *&c.* $\pm d x^3 \mp c x^2 \pm b x \mp A = o$, *there are juſt as many in the Æquation* $A x^n - b x^{n-1} + c x^{n-2} - d x^{n-3} +$ *&c.* $\pm D x^3 \mp C x^2 \pm B x \mp 1 = o$. For the Roots of the laſt Æquation are the Reciprocals of thoſe of the firſt, as is evident from common Algebra. Let the Roots of the biquadratick Æquation $x^4 - B x^3 + C x^2 - D x + A = o$ be a, b, c, d, whereof let c, d be impoſſible; then the Roots of the Æquation $A x^4 - D x^3 + C x^2 - B x + 1 = o$ will be $\frac{1}{a}$, $\frac{1}{b}$, $\frac{1}{c}$, $\frac{1}{d}$, and therefore two of them to wit $\frac{1}{c}$, $\frac{1}{d}$, impoſſible.

Lemma III.] *In every Æquation* $x^n - B x^{n-1} + C x^{n-2} - D x^{n-3} + E x^{n-4} -$ *&c.* $\pm e x^4 \mp d x^3 \pm c x^2 \mp b x \pm A = o$, *all whoſe Roots are real, if each Term be multiply'd by the Index of* x *in that Term, and each Product be divided by* x, *the reſulting Æquation* $n x^{n-1} - \overline{n-1} B x^{n-2} + \overline{n-2} C x^{n-3} - \overline{n-3} D x^{n-4} + \overline{n-4} E x^{n-5} -$ *&c.* $\pm 4 e x^3 \mp 3 d x^2 \pm 2 c x \mp b = o$ *ſhall have all its Roots real.* Thus if all the Roots of the Æquation $x^4 - B x^3 + C x^2 - D x + A = o$ be real, then all the Roots of the Æquation $4 x^3 - 3 B x^2 + 2 C x - D = o$ will alſo be real.

This *Lemma* doth not hold converſly, for there is an Infinity of Caſes where all the Roots of the Æquation $n x^{n-1} - \overline{n-1} B x^{n-2} +$

$\overline{n-2}\,Cx^{n-3} - \overline{n-3}\,Dx^{n-4} + \&c. \pm 3dx^2 \mp 2cx \pm b = 0$ are real, at the ſame Time ſome or perhaps all the Roots of the Æquation $x^n - Bx^{n-1} + Cx^{n-2} - Dx^{n-3} + \&c. \pm dx^3 \mp cx^2 \pm bx \mp A = 0$ are impoſſible: But whatever be the Number of impoſſible Roots in the Æquation $nx^{n-1} - \overline{n-1}\,Bx^{n-2} + \overline{n-2}\,Cx^{n-3} - \&c. \pm 2cx \mp b = 0$, there are at leaſt as many in the Æquation $x^n - Bx^{n-1} + Cx^{n-2}\ \&c. \pm cx^2 \mp bx \pm A = 0$. Thus all the Roots of the Æquation $4x^3 - 3Bx^2 + 2Cx - D = 0$ may be real, and yet two or perhaps all the four Roots of the Æquation $x^4 - Bx^3 + Cx^2 - Dx + A = 0$ may be impoſſible; but if two of the Roots of the Æquation $4x^3 - 3Bx^2 + 2Cx - D = 0$ be impoſſible, there muſt be at leaſt two impoſſible Roots in the Æquation $x^4 - Bx^3 + Cx^2 - Dx + A = 0$. All this hath been demonſtrated by Algebraical Writers, particularly by Mr. *Reyneau* in his *Analyſe demontré*, and is eaſily made evident by the Method of the *Maxima* and *Minima*.

Coroll.] Let all the Roots of the Æquation $x^n - Bx^{n-1} + Cx^{n-2} - Dx^{n-3} + Ex^{n-4} - Fx^{n-5} + \&c. \pm fx^5 \mp ex^4 \pm dx^3 \mp cx^2 \pm bx \mp A = 0$ be real, and by this *Lemma* all the Roots of the Æquation $nx^{n-1} - \overline{n-1}\,Bx^{n-2} + \overline{n-2}\,Cx^{n-3} - \overline{n-3}\,Dx^{n-4} + \overline{n-4}\,Ex^{n-5} - \overline{n-5}\,Fx^{n-6} + \&c. \pm 5fx$[illegible]$\mp 4ex^3 \pm 3dx^2 \mp 2cx \pm b = 0$ will be real, and therefore (by the ſame *Lemma*) all the Roots of the Æquation $n \times \overline{n-1}\,x^{n-2} - \overline{n-1} \times \overline{n-2}\,Bx^{n-3} + \overline{n-2} \times \overline{n-3}\,Cx^{n-4} - \overline{n-3} \times \overline{n-4}\,Dx^{n-5} + \overline{n-4} \times \overline{n-5}\,Ex^{n-6} - \overline{n-5} \times \overline{n-6}\,Fx^{n-7} + \&c. \pm 20fx^3 \mp 12ex^2 \pm 6dx \mp 2c = 0$, or (dividing all by 2) of $n \times \frac{\overline{n-1}}{2}x^{n-2} - \overline{n-1} \times \frac{\overline{n-2}}{2}Bx^{n-3} + \overline{n-2} \times \frac{\overline{n-3}}{2}Cx^{n-4} - \&c. \pm 10fx^3 \mp 6ex^2 \pm 3dx \mp c = 0$ will be real. After the ſame Manner all the Roots of the Æquation $n \times \frac{\overline{n-1}}{2} \times \frac{\overline{n-2}}{3}x^{n-3} - \overline{n-1} \times \frac{\overline{n-2}}{2} \times \frac{\overline{n-3}}{3}Bx^{n-4} + \overline{n-2} \times \frac{\overline{n-3}}{2} \times \frac{\overline{n-4}}{3}Cx^{n-5} - \&c. \pm 10fx^2 \mp 4ex \pm d = 0$ will be real; and thus we may deſcend until we arrive at the quadratick Æquation $n \times \frac{\overline{n-1}}{2}x^2 - \overline{n-1}\,Bx + C = 0$. The ſame Æquations do aſcend thus $n \times \frac{\overline{n-1}}{2} x^2 - \overline{n-1}\, Bx + C = 0$, $n \times \frac{\overline{n-1}}{2} \times \frac{\overline{n-2}}{3} x^3 - \overline{n-1} \times \frac{\overline{n-2}}{2} Bx^2 + \overline{n-2}\, Cx - D = 0$, $n \times \frac{\overline{n-1}}{2} \times \frac{\overline{n-2}}{3} \times \frac{\overline{n-3}}{4} x^4 - \overline{n-1} \times \frac{\overline{n-2}}{2} \times \frac{\overline{n-3}}{3} Bx^3 + \overline{n-2} \times \frac{\overline{n-3}}{2} Cx^2 - \overline{n-3}\, Dx + E = 0$, $n \times \frac{\overline{n-1}}{2} \times \frac{\overline{n-2}}{3} \times \frac{\overline{n-3}}{4} \times \frac{\overline{n-4}}{5} x^5 - \overline{n-1} \times \frac{\overline{n-2}}{2} \times \frac{\overline{n-3}}{3} \times \frac{\overline{n-4}}{4} Bx^4 + \overline{n-2} \times \frac{\overline{n-3}}{2} \times \frac{\overline{n-4}}{3} Cx^3 - \overline{n-3} \times \frac{\overline{n-4}}{2} Dx^2 + \overline{n-4}\, Ex - F = 0$, and ſo on. Let *M* repreſent any of the Coefficients of the Æquation $x^n - Bx^{n-1} + Cx^{n-2} - Dx^{n-3} + Ex^{n-4} - \&c. \pm A = 0$, and let *L*, *N* be the adjacent Coefficients, let *M* be the Exponent of the Coefficient *M*: By the Exponent of a Coefficient I mean the Number which expreſſeth the Place which it hath among the Coefficients; thus if *M* repreſent the Coefficient *E* (and therefore $L = D$ and $N = F$) then $m = 4$. It will be eaſy to ſee, that, amongſt the foregoing aſcending Æquations, that which hath its abſolute Number *N* will be $n \times \frac{\overline{n-1}}{2} \times \frac{\overline{n-2}}{3} \times \&c. \frac{\overline{n-m}}{m+1} x^{m+1} - \overline{n-1} \times \frac{\overline{n-2}}{2} \times \&c. \frac{\overline{n-m}}{m} Bx^m + \overline{n-2} \times \&c. \frac{\overline{n-m}}{m-1} Cx^{m-1} - \&c. \pm \overline{n-m+1} \times \frac{\overline{n-m}}{2} Lx^2 \mp \overline{n-m}\, Mx \pm N = 0$, all whoſe Roots are real when all the Roots of the Æquation $x^n - Bx^{n-1} + Cx^{n-2} - \&c. \pm A = 0$ are real. Let $N = F$ and therefore $M = E$, $L = D$ and $m = 4$, then that of the aſcending Æquations whoſe abſolute Number is *F*, will be $n \times \frac{\overline{n-1}}{2} \times \frac{\overline{n-2}}{3} \times \frac{\overline{n-3}}{4} \times \frac{\overline{n-4}}{5} x^5 - \overline{n-1} \times \frac{\overline{n-2}}{2} \times \frac{\overline{n-3}}{3} \times \frac{\overline{n-4}}{4} Bx^4 + \overline{n-2} \times \frac{\overline{n-3}}{2} \times \frac{\overline{n-4}}{3} Cx^3 - \overline{n-3} \times \frac{\overline{n-4}}{2} Dx^2 + \overline{n-4}\, Ex - F = 0$.

Prop. I.] Let $x^n - Bx^{n-1} + Cx^{n-2} - Dx^{n-3} + Ex^{n-4} -$ &c. $\pm ex^4 \mp dx^3 \pm cx^2 \mp bx \pm A = 0$ *be an Æquation of any Dimenſions, all whoſe Roots are real; let M be any Coefficient of this Æquation, L, N the adjacent Coefficients, and m the Exponent of M. Then the Square of any Coefficient M multiply'd by the Fraction* $\frac{m\times\overline{n-m}}{\overline{m+1}\times\overline{n-m+1}}$ *will always exceed the Rectangle under the adjacent Coefficients* $L\times N$. Thus in the Æquation $x^4 - Bx^3 + Cx^2 - Dx + A = 0$, where $n = 4$, making $M = C$ and therefore $L = B$, $N = D$, and $m = 2$, then $\frac{2\times\overline{4-2}}{\overline{2+1}\times\overline{4-2+1}}\times C^2$ or $\frac{4}{9}C^2$ will exceed $B\times D$ providing all the Roots of the Æquation be real.

Becauſe (by *Lem.* 3.) the Roots of the quadratick Æquation $n\times\frac{n-1}{2}x^2 - \overline{n-1}Bx + C = 0$ are real, therefore (by *Lem.* 1.) $\frac{1}{4}\overline{n-1}|^2\times B^2$ muſt be greater than $n\times\frac{n-1}{2}\times C$, and (dividing both by $\overline{n\times\frac{n-1}{2}}$) $\frac{n-1}{2n}\times B^2$ greater than $1\times C$. Therefore in the Æquation $x^n - Bx^{n-1} + Cx^{n-2} - Dx^{n-3} +$ &c. $\pm A = 0$ of the n Degree, all whoſe Roots are real, the Square of B the Coefficient of the ſecond Term, multiply'd by the Fraction $\frac{n-1}{2n}$ is greater than $1\times C$, the Rectangle under the adjacent Coefficients. But (by *Lem.* 2.) all the Roots of the Æquation $Ax^n - bx^{n-1} + cx^{n-2} -$ &c. $\pm Cx^2 \mp Bx \pm 1 = 0$, or (dividing by A) of $x^n - \frac{b}{A}x^{n-1} + \frac{c}{A}x^{n-2} -$ &c. $\pm\frac{C}{A}x^2 \mp \frac{B}{A}x \pm \frac{1}{A} = 0$ are real; therefore (from what hath been juſt now ſaid) $\frac{\overline{n-1}}{2n}\times\frac{b^2}{A^2}$ muſt be greater than $1\times\frac{c}{A}$ and conſequently $\frac{\overline{n-1}}{2n}\times b^2$ greater than $c\times A$. Therefore in an Æquation $x^n - Bx^{n-1} + Cx^{n-2} -$ &c. $\pm cx^2 \mp bx \pm A = 0$, of the n Degree, all whoſe Roots are real, the Square of the Coefficient of x multiply'd by the Fraction $\frac{n-1}{2n}$ is greater than the Rectangle under the Coefficient of x^2 and the abſolute Number: But by *Cor. Lem.* 3. all the Roots of the Æquation $n \times \frac{\overline{n-1}}{2} \times \frac{\overline{n-2}}{3} \times$ &c. $\times \frac{\overline{n-m}}{m+1} x^{m+1} - \overline{n-1} \times \frac{n-2}{2} \times$ &c. $\times \frac{\overline{n-m}}{m} B x^{m} + \overline{n-2} \times$ &c. $\frac{\overline{n-m}}{m-1} C x^{m-1}$ &c. $\pm \overline{n-m+1} \times \frac{\overline{n-m}}{2} \times L x^{2} \mp \overline{n-m}\, M x \pm N = 0$ are real; therefore (ſeeing this Æquation is of the $m+1$ Degree) the Square of $\overline{n-m} \times M$ multiply'd by the Fraction $\frac{\overline{m+1-1}}{2 \times \overline{m+1}}$ will be greater than the Rectangle under $\overline{n-m+1} \times \frac{\overline{n-m}}{2} \times L$ and N, that is, $\frac{m}{2 \times \overline{m+1}} \times \overline{n-m}|^{2} \times M^{2}$ will be greater than $\overline{n-m+1} \times \frac{\overline{n-m}}{2} \times L \times N$, and therefore (dividing both by $\overline{n-m+1} \times \frac{\overline{n-m}}{2}$) $\frac{m \times \overline{n-m}}{\overline{m+1} \times \overline{n-m+1}} \times M^{2}$ greater than $L \times N$.

Coroll.] Make a Series of Fractions $\frac{n}{1}, \frac{n-1}{2}, \frac{n-2}{3}, \frac{n-3}{4}$, &c. unto $\frac{1}{n}$, whoſe Denominators are Numbers going on in the Progreſſion 1, 2, 3, 4, &c. unto the Number n which is the Dimenſions of the Æquation $x^{n} - B x^{n-1} + C x^{n-2} -$ &c. $\pm A = 0$, and whoſe Numerators are the ſame Progreſſion inverted. Divide the ſecond of theſe Fractions by the firſt, the third by the ſecond, the fourth by the third, and ſo on, and place the Fractions which reſult from this Diviſion above the middle Terms of the Æquation, thus $x^{n} - \overset{\frac{n-1}{2n}}{B} x^{n-1} + \overset{\frac{2 \times \overline{n-2}}{3 \times \overline{n-1}}}{C} x^{n-2} - \overset{\frac{3 \times \overline{n-3}}{4 \times \overline{n-2}}}{D} x^{n-3} + \overset{\frac{4 \times \overline{n-4}}{5 \times \overline{n-3}}}{E} x^{n-4} -$ &c. $\pm A = 0$. Then if all the Roots of the Æquation are real, the Square of any Coefficient multiply'd by the Fraction which ſtands above, will be greater than the Rectangle under the adjacent Coefficients. This *Corollary* doth not hold converſly, for there is an Infinity of Æquations in which the Square of each Coefficient multiply'd by the Fraction above it, may be greater than the Rectangle under the adjacent Coefficients, and notwithſtanding ſome or perhaps all of the Roots may be impoſſible. Therefore when the Square of a Coefficient multiply'd by the Fraction above, is greater than the Rectangle under the adjacent Coefficients, from this Circumſtance nothing can be determined as to the Poſſibility or Impoſſibility of the Roots of the Æquation: But when the Square of a Coefficient multiply'd by the Fraction above it, is leſs than the Rectangle under the adjacent Coefficients, it is a certain Indication of two impoſſible Roots. From what hath been ſaid, is immediately deduced the Demonſtration of that Rule which the moſt illuſtrious *Newton* gives for the determining the Number of impoſſible Roots in any given Æquation.

Schol.] Let the Roots of the Æquation $x^n - Bx^{n-1} + Cx^{n-2} - Dx^{n-3} + Ex^{n-4} - Fx^{n-5} + \&c. \pm A = o$ (with their Signs) be repreſented by the Letters $a, b, c, d, e, f, g,$ &c. then (as is commonly known) B will be the Sum of all the Roots or $= a + b + c + d + e + f +$ &c. C the Sum of the Products of all the Pairs of Roots, or $= ab + ac + ad + af + ag +$ &c. D the Sum of the Products of all the Ternaries of Roots, or $= abc + abd + abe + abf + abg +$ &c. $E = abcd + abce + abcf + abcg +$ &c. $F = abcde + abcdf + abcdg + bcdef +$ &c. and ſo on. Let (as in this Propoſition) M repreſent any of theſe Coefficients, L, N the adjacent Coefficients, and m the Exponent of M; let Z repreſent the Sum of the Squares of all the poſſible Differences between the Terms of the Coefficient M, let α be the Sum of all thoſe of the foreſaid Squares whoſe Terms differ by one Letter, β the Sum of all thoſe Squares whoſe Terms differ by two Letters, γ the Sum of thoſe Squares whoſe Terms differ by three Letters, δ the Sum of thoſe Squares whoſe Terms differ by four Letters, and ſo on.

Thus if $M = F = abcde + abcdf + abcdg +$ &c.

$$\text{Then } Z = \overline{abcde - abcdf}\,|^2 + \overline{abcde - abcdg}\,|^2 + \overline{abcde - abcfg}\,|^2 + \overline{bcdef - abfgh}\,|^2 + \&c.$$

$$\alpha = \overline{abcde - abcdf}\,|^2 + \overline{abcde - abcdg}\,|^2 + \overline{abcde - abcdh}\,|^2 + \overline{bcdef - bcdeg}\,|^2 + \&c.$$

$$\beta = \overline{abcde - abcfg}\,|^2 + \overline{abcde - abcfh}\,|^2 + \overline{bcdef - acdfh}\,|^2 + \&c.$$

$$\gamma = \overline{abcde - abfgh}\,|^2 + \overline{abcdf - abegh}\,|^2 + \&c.$$

$$\delta = \overline{abcde - afghk}\,|^2 + \overline{acdfg - abehk}\,|^2 + \&c.$$

This being laid down, I ſay that the Square of any Coefficient M multiply'd by the Fraction $\dfrac{m \times \overline{n - m}}{\overline{m + 1} \times \overline{n - m + 1}}$ exceeds the Rectangle under the adjacent Coefficients $L \times N$, by $\frac{\overline{n+1} \times Z}{\overline{m+1} \times \overline{n-m+1}} - \frac{1}{2}\alpha - \frac{1}{3}\beta - \frac{1}{4}\gamma - \frac{1}{5}\delta -$ &c. The Series $-\frac{1}{2}\alpha - \frac{1}{3}\beta - \frac{1}{4}\gamma -$ &c. muſt conſiſt of m Number of Terms.

Let the Æquation be $x^5 - Bx^4 + Cx^3 - Dx^2 + Ex - A = o$, whoſe Roots let be a, b, c, d, e, in which Caſe $n = 5$. Let $M = B = a + b + c + d + e$, then $L = 1$, $N = G$, $m = 1$, $Z = \overline{a-b}|^2 + \overline{a-c}|^2 + \overline{a-d}|^2 + \overline{a-e}|^2 + \overline{b-c}|^2 +$ &c. $= \alpha$; therefore $\frac{1 \times \overline{5-1}}{\overline{1+1} \times \overline{5-1+1}} \times B^2$ or $\frac{2}{5}B^2$ exceeds $1 \times C$ by $\frac{\overline{5+1} \times Z}{\overline{1+1} \times \overline{5-1+1}} - \frac{1}{2}\alpha = \frac{3}{5}Z - \frac{1}{2}\alpha =$ (becauſe $Z = \alpha$) $\frac{1}{10}Z = \frac{1}{10}\overline{a-b}|^2 + \frac{1}{10}\overline{a-c}|^2 + \frac{1}{10}\overline{a-d}|^2 +$ &c. which is always a poſitive Number when the Roots a, b, c, d, e are real, poſitive or negative Numbers.

Let $M = C = ab + ac + ad + ae + bc +$ &c. then $L = B$, $N = D$, $m = 2$.

$$Z = \overline{ab-ac}|^2 + \overline{ab-ad}|^2 + \overline{ab-cd}|^2 + \overline{ab-de}|^2 + \text{\&c.}$$

$$\alpha = \overline{ab-ac}|^2 + \overline{ab-ad}|^2 + \overline{ab-ae}|^2 + \text{\&c.}$$

$$\beta = \overline{ab-cd}|^2 + \overline{ab-ce}|^2 + \overline{ab-de}|^2 + \text{\&c.}$$

Therefore $\frac{2 \times \overline{5-2}}{\overline{2+1} \times \overline{5-2+1}} \times C^2$ or $\frac{1}{2}C^2$ ſurpaſſeth $B \times D$ by $\frac{\overline{5+1} \times Z}{\overline{2+1} \times \overline{5-2+1}} - \frac{1}{2}\alpha - \frac{1}{3}\beta =$ (becauſe $Z = \alpha + \beta$) $\frac{1}{6}\beta = \frac{1}{6} \times \overline{ab-cd}|^2 + \frac{1}{6} \times \overline{ab-ce}|^2 + \frac{1}{6} \times \overline{ab-de}|^2 +$ &c. which is always a poſitive Number when the Roots a, b, c, d, e are real Numbers, poſitive or negative.

Let $M = D = abc + abd + abe + acd + ace +$ &c. then $L = C$, $N = E$, $m = 3$.

$$Z = \overline{abc-abd}|^2 + \overline{abc-abe}|^2 + \overline{abc-ade}|^2 + \text{\&c.}$$

$$\alpha = \overline{abc-abc}|^2 + \overline{abc-abd}|^2 + \overline{abc-acd}|^2 + \text{\&c.}$$

$\beta = \overline{abc-ade}|^2 + \overline{abc-cde}|^2 + \overline{abc-bde}|^2 + \&c.$

$\gamma = 0.$

Therefore $\frac{3\times\overline{5-3}}{\overline{3+1}\times\overline{5-3+1}}\times D^2$ or $\frac{1}{2}D^2$ exceeds $C\times E$ by $\frac{5+1}{\overline{3+1}\times\overline{5-3+1}}\times Z - \frac{1}{2}\alpha - \frac{1}{3}\beta =$ (becauſe $Z = \alpha + \beta = \frac{1}{6}\times\beta) = \frac{1}{6}\times\overline{abc-ade}|^2 + \frac{1}{6}\times\overline{abc-cde}|^2 + \frac{1}{6}\overline{abc-bde}|^2 + \&c.$ which is a poſitive Number when the Roots are real Numbers.

Let $M = E = abcd + abce + abde + bcde + \&c.$ then $L = D$, $N = A$, $m = 4$, $Z = \overline{abcd-abce}|^2 + \overline{abcd-bcde}|^2 + \overline{abcd-acde}|^2 + \&c. = \alpha$, $\beta = 0 = \gamma = \delta$, therefore $\frac{4\times\overline{5-4}}{\overline{4+1}\times\overline{5-4+1}}\times E^2$ or $\frac{2}{5}E^2$ exceeds $D\times A$ by $\frac{\overline{5+1}}{\overline{4+1}\times\overline{5-4+1}}\times Z - \frac{1}{2}\alpha = \frac{3}{5}Z - \frac{1}{2}\alpha = \frac{1}{10}Z = \frac{1}{10}\times\overline{abcd-abce}|^2 + \frac{1}{10}\times\overline{abcd-bcde}|^2 + \&c.$ which is a poſitive Number when the Roots are real Numbers.

Prop. II. *Let* $x^n - Bx^{n-1} + Cx^{n-2} - Dx^{n-3} + Ex^{n-4} - \&c. \pm A = 0$ *be an Æquation of any Degree, whoſe Roots with their Signs let be expreſſed by the Letters a, b, c, d, e, f, &c. let M repreſent any Coefficient of this Æquation, L, N the Coefficients adjacent to M; K, O the Coefficients adjacent to L, N; I, P thoſe adjacent to K, O; H, Q thoſe adjacent to I, P, and ſo on. Let m repreſent the Exponent of M, and let Z (as in the preceding Propoſition) repreſent the Sum of the Squares of all the poſſible Differences between the Terms of the Coefficient M. Then the Product of the Square of any Coefficient M multiply'd by the Fraction* $\frac{1}{2}\times\overline{1-\frac{1}{n\times\frac{\overline{n-1}}{2}\times\frac{\overline{n-2}}{3}\times\&c.\times\frac{\overline{n-m+1}}{m}}}$ *doth always exceed* $L\times N - K\times O + I\times P - H\times Q + \&c.$ *by* $\frac{\frac{1}{2}Z}{n\times\frac{\overline{n-1}}{2}\times\frac{\overline{n-2}}{3}\times\&c.\times\frac{\overline{n-m+1}}{m}}$ *which is always a poſitive*

Number, when the Roots a, b, c, d, e &c. *are real Numbers poſitive or negative.* Let the Æquation be of the ſeventh Degree or $x^7 - Bx^6 + Cx^5 - Dx^4 + Ex^3 - Fx^2 + Gx - A = 0$, whoſe Roots let be a, b, c, d, e, f, g, in which Caſe $n = 7$. Let $M = E = abcd + abce + abcf + abcg + bcde +$ &c. then $m = 4$, $L = -D$, $N = -F$, $K = C$, $O = G$, $I = -B$, $P = -A$,

$$Z = \overline{abcd - abce}|^2 + \overline{abcd - abcf}|^2 + \overline{abcd - abcg}|^2 + \text{&c.}$$

Therefore $\frac{1}{2} \times \overline{1 - \frac{1}{7 \times \frac{6}{2} \times \frac{5}{3} \times \frac{4}{4}}} \times E^2$ or $\frac{17}{35} E^2$ exceeds

$$D \times F - C \times G + B \times A \text{ by } \frac{\frac{1}{2} Z}{7 \times \frac{6}{2} \times \frac{5}{3} \times \frac{4}{4}} \text{ or } \frac{Z}{70} = \frac{1}{70} \times \overline{abcd - abce}|^2 + \frac{1}{70} \times \overline{abcd - abcf}|^2 + \text{&c.}$$

From this Propoſition, is deduced the following Rule for determining the Number of impoſſible Roots in any given Æquation. *From each of the Unciæ of the middle Terms of that Power of a Binomial, whoſe Index is the Dimenſions of the propoſed Æquation, ſubtract Unity, then divide each Remainder by twice the Correſpondent* Uncia, *and ſet the Fractions which reſult from this Diviſion, above the middle Terms of the given Æquation. And under any of the middle Terms if its Square multiplyed by the Fraction ſtanding above it, be greater than the Rectangle under the immediately adjacent Terms,* Minus *the Rectangle under the next adjacent Terms,* Plus *the Rectangle under the Terms then next adjacent* — &c. *place the Sign* +, *but if it be leſs, place the Sign* —. *And under the firſt and laſt Term place* +. *And there will be at leaſt as many impoſſible Roots, as there are Changes in the Series of the under-written Signs from* + *to* —, *or from* — *to* +. Let it be required to determine the Number of impoſſible Roots in the Æquation $x^7 - 5x^6 + 15x^5 - 23x^4 + 18x^3 + 10x^2 - 28x + 24 = 0$. The *Unciæ* of the middle Terms of the 7th Power of a Binomial are 7, 21, 35, 35, 21, 7, from which ſubtracting Unity, and dividing each of the Remainders by twice the correſpondent *Uncia*, the Quotients will be $\frac{6}{14}, \frac{20}{42}, \frac{34}{70}, \frac{34}{70}, \frac{20}{42}, \frac{6}{14}$ or $\frac{3}{7}, \frac{10}{21}, \frac{17}{35}, \frac{17}{35}, \frac{10}{21}, \frac{3}{7}$ which Fractions

Lemma IV.] *Let the Number of the Quantities* $a, b, c, d, e,$ &c. *be* m, *the Sum of their Squares* A, *and the Sum of the Products made by multiplying any two of them* B. *Then ſhall* $\frac{\overline{m-1}\times A}{2}$ *be always greater than* B.

For by adding together the Squares of the Differences $a-b$, $a-c$, $a-d$, $b-c$, $b-d$, $c-d$, &c. you add a^2 as often to it ſelf as there are Quantities more than a; the ſame is true of b^2, c^2, d^2, e^2, &c. But the Rectangles $-2ab$, $-2ac$, $-2ad$, $-2bc$, $-2bd$, &c. ariſe but once each. Therefore the Sum of all the Squares $\overline{a-b}^2$, $\overline{a-c}^2$, $\overline{b-c}^2$, $\overline{b-d}^2$, &c. $=\overline{m-1}\times a^2+\overline{m-1}\times b^2+\overline{m-1}\times c^2$, &c. $-2ab-2ac-2bc$, &c. $=\overline{m-1}\times A-2B$. But $\overline{a-b}^2+\overline{a-c}^2+\overline{a-d}^2$, &c. is always a poſitive Quantity; therefore $\overline{m-1}\times A-2B$ is Poſitive, and conſequently $\frac{\overline{m-1}\times A}{2}$ greater than B.

Coroll.] It appears from the Demonſtration, that the Exceſs of $\overline{m-1}\times A$ above $2B$ is always equal to the Sum of the Squares of the Differences of the Quantities $a, b, c, d,$ &c. and that when the Quantities $a, b, c, d,$ &c. are all equal, then $\overline{m-1}\times A-2B=0$, and with this Reſtriction the preceding *Lemmata* muſt be underſtood.

It is to be obſerved, that tho' we have ſuppoſed in theſe *Lemmata* the Quantities $a, b, c, d,$ &c. Poſitive, they are *a fortiori* true of Negative Quantities, whoſe Squares are the ſame as if they were Poſitive, while the Sum of their Products is either the ſame, or leſs than it would be, were they all Poſitive.

Prop. I.] *In a Quadratic Æquation that has its Roots real, the Square of the ſecond Term muſt be always greater than the quadruple Product of the third and firſt Terms.*

Let the Roots of the Quadratic Æquation be repreſented by $+a$ and $+b$; and if x be the unknown Quantity, then ſhall

$$\begin{array}{l} x^2-ax+ab=0 \\ \quad\;\; -bx \end{array}$$

Now ſince a^2+b^2 is greater than $2ab$, by *Lemma* 1, therefore a^2+b^2+2ab is greater than $4ab$; therefore $\overline{a+b}^2\times x^2$, the Square of the ſecond Term, will be greater than $4ab\times x^2$ the Quadruple Product of the firſt and third Terms.

Prop. II.] *In any Cubic Æquation, all whoſe Roots are real, the Square of the ſecond Term is always greater than the triple Product of the firſt and third.*

If the Cubic Æquation has all its Roots real, they may be repreſented with their Signs by a, b, c, and the Æquation will be expreſſed thus:

$$\begin{aligned} y^3-ay^2&+aby-abc=0\\ -by^2&+acy\\ -cy^2&+bcy \end{aligned}$$

But by *Lemma* 2, $a^2+b^2+c^2$ is always greater than $ab+ac+bc$; and conſequently adding $2ab+2ac+2bc$ to both ſides, $a^2+b^2+c^2+2ab+2ac+2bc$ $\left(=\overline{a+b+c}^2\right)$ will be greater than $3ab+3ac+3bc$; and therefore $\overline{a+b+c}^2 \times y^4$ muſt be greater than $\overline{3ab+3ac+3bc}\times y^4$, that is, the Square of the ſecond Term muſt be greater than the triple Product of the firſt and third Terms.

Coroll. 1.] In general, it appears from the Demonſtration, that the Square of the Sum of three real Quantities, $\overline{a+b+c}^2$ is always greater than the triple Sum of all the Products, that can be made by multiplying any two of them into one another.

Coroll. 2.] It follows from the Propoſition, that when the Square of the ſecond Term is not greater than the triple Product of the firſt and third Terms, the Roots of the Æquation cannot be all real; but two of them muſt be impoſſible: and this plainly coincides with one Part of Sir *Iſaac Newton*'s Rule for diſcovering when the Roots of Cubic Æquations are impoſſible.

He deſires you may write above the middle Terms of the Æquation, the Fractions $\frac{1}{3}$, $\frac{1}{3}$, as in the Margin; and placing the Sign + under the firſt and laſt Term, he multiplies the Square of the ſecond Term by the Fraction $\frac{1}{3}$ that is above it; and if the Product is greater than the Product of the adjacent Terms, he places + under the ſecond Term; but if that Product is leſs, he places — under the ſecond Term, and ſays, there are as many impoſſible Roots as Changes in the Signs. Now by this Propoſition, if p^2x^4, is not greater than $3qx^4$, or $\frac{1}{3}p^2x^4$ greater than qx^4, the Roots cannot be all real. The ſame Suppoſition makes two Changes in the Signs, whatever Sign you place under the third Term, ſince the Signs under the firſt and laſt are both +; and therefore this propoſition demonſtrates the firſt Part of Sir *Iſaac Newton*'s Rule, as far as it relates to *Cubic Æquations*.

$$\begin{array}{cccc} & \frac{1}{3} & \frac{1}{3} & \\ x^3 & +px^2 & +qx & +r=0 \\ + & - & * & + \end{array}$$

Coroll. 3.] If the ſecond Term is wanting in a *Cubic Æquation*, and the third is Poſitive, two of the Roots of the Æquation muſt be impoſſible: for the Square of the ſecond Term (equal to nothing in this Caſe) will be leſs than the triple Product of the adjacent Terms. But this will better appear from conſidering that, when the ſecond Term vaniſhes in an Æquation, the Poſitive and Negative Roots are equal, and when added together, deſtroy each other. Suppoſe the Roots to be $+a$ and $-b$, $-c$; then in this Caſe $a=+b+c$, and the Coefficient of the third Term will be $-ab-ac+bc=-b^2-2bc-c^2+bc=-b^2-bc-c^2$, and conſequently Negative. Or, if you ſuppoſe two Roots Poſitive and one Negative, let them be $-a$, $+b$, $+c$, then the Coefficient of the third Term will be ſtill $-b^2-bc-c^2$. Therefore when the Roots are real, the Coefficient of the third Term is Negative; and if the Coefficient of the third Term is not affected with a negative Sign, it is a Proof that two of the Roots are Impoſſible.

Prop. III.] *In any Cubic Æquation, all whoſe Roots are real, the Square of the third Term muſt be greater than the triple Product of the ſecond and fourth Terms.*

In the ſame Cubic Æquation, whoſe Roots are a, b, c, the Square of the third Term is $\overline{ab+ac+bc}^2$, the Product of the ſecond and fourth Terms is $a^2bc+ab^2c+abc^2$, as is plain from the Inſpection of the Æquation; and it is obvious that $a^2bc+ab^2c+abc^2$ is the Sum of the Products of any two of the Terms ab, ac, bc; and therefore by *Corol.* 1. *Prop.* 2. the Square of the Sum of theſe Terms, that is, $\overline{ab+ac+bc}^2$ muſt be greater than $3a^2bc+3ab^2c+3ac^2b$. So that $\overline{ab+ac+bc}^2\times y^2$ muſt be greater than $\overline{3a^2bc+3ab^2c+3ac^2b}\times y^2$; that is, the Square of the third Term muſt be greater than the triple Product of the ſecond and fourth Terms.

Coroll. 1.] It follows from the Demonſtration, that $\overline{ab+ac+bc}^2$ is always greater than $3abc\times\overline{a+b+c}$.

Coroll. 2.] If the Square of the third Term is found to be leſs than the triple Product of the ſecond and fourth Terms, then the Roots of the Æquation cannot be all real Quantities; and this concludes with *the ſecond Part of Sir* Iſaac Newton'*s Rule* for finding when the Roots of a *Cubic Æquation* are impoſſible: for this Caſe gives $-$ to be placed under the third Term, and conſequently two Changes of the Signs, whatever Sign is placed under the ſecond Term.

$$\begin{array}{ccccccc} & \frac{1}{3} & & & & \frac{1}{3} & \\ x^3 & + & px^2 & + & qx & + & r=0 \\ + & & * & & - & & + \end{array}$$

Schol.] After the ſame manner, it may be demonſtrated, that in a *Cubic Æquation*, whoſe Roots are all real, if the ſecond Term is wanting, the Cube of the third Part of the third Term taken poſitively, is always greater than the Square of half the laſt Term. Suppoſe that the Roots of the Æquation are $+a$, $-b$, $-c$, or $-a$, $+b$, $+c$, and that $a=b+c$, then the ſecond Term in the Æquation will be wanting, and the other Terms will be expreſſed thus:

$$\begin{array}{lll} y^3 & * & -b^2 y \pm bc \times \overline{b+c} \\ & & -bcy \\ & & -c^2 y \end{array}$$

The Square of $b-c$ is always poſitive, ſince b and c are real Quantities. Suppoſe it, (*viz.* $b^2-2bc+c^2$) equal to D, then $b^2+bc+c^2=D+3bc$, and $\overline{b+c}^2=D+4bc$. Therefore $\frac{\overline{b^2+bc+c^2}^3}{27}=\frac{D^3}{27}+\frac{D^2bc}{3}+Db^2c^2+b^3c^3$, and $b^2c^2\times\frac{\overline{b+c}^2}{4}=\frac{Db^2c^2}{4}+b^3c^3$. Now 'tis obvious that $\frac{D^3}{27}+\frac{D^2bc}{3}+Db^2c^2+b^3c^3$ is greater than $\frac{Db^2c^2}{4}+b^3c^3$, ſince D is poſitive, and bc alſo poſitive, b and c being Roots having the ſame Sign. Therefore the Cube of $\frac{1}{3}$ of the third Term having its Sign changed $\left(=\frac{\overline{b^2+bc+c^2}^3}{27}\right)$ is always greater than the Square of half the laſt Term $\left(=b^2c^2\times\frac{\overline{b+c}^2}{4}\right)$. In the Cubic Æquation $x^3 * +qx+r=0$, if q be poſitive, or if it be negative and $\frac{+q^3}{27}$ be leſs than $\frac{1}{4}r^2$, it appears that two Roots of the Æquation muſt be impoſſible, from this Corollary, and from *Cor.* 3. *Prop.* 2. taken together.

Prop. IV.] *In a Biquadratic Æquation, all whoſe Roots are real Quantities, $\frac{1}{8}$ of the Square of the ſecond Term is always greater than* *the Product of the firſt and third Terms; and $\frac{3}{8}$ of the Square of the fourth Term is always greater than the Product of the third and fifth Terms.*

1. Let the Æquation be $x^4 - px^3 + qx^2 - rx + s = 0$; and ſince the Roots are ſuppoſed to be all real, let them be repreſented by a, b, c, d, then $p = a + b + c + d$, and $q = ab + ac + ad + bc + bd + cd$. But it is plain from *Lemma* 3, that $3a^2 + 3b^2 + 3c^2 + 3d^2$ is greater than $2ab + 2ac + 2ad + 2bc + 2bd + 2cd$; and conſequently by adding $6ab + 6ac + 6ad + 6bc + 6bd + 6cd$ to both, we ſhall find that $3 \times \overline{a + b + c + d}^2$ muſt be greater than $8ab + 8ac + 8ad + 8bc + 8bd + 8cd$; that is, $3p^2$ greater than $8q$; and therefore $\frac{3}{8}p^2x^6$ greater than qx^6.

2. Since $r = abc + abd + acd + bcd$, and $s = abcd$; and ſince qs is equal to $a^2b^2cd + a^2c^2bd + a^2d^2bc + b^2c^2ad + b^2d^2ac + c^2d^2ab$, which are the Products can be made of any two of the Quantities abc, abd, acd, bcd, whoſe Sum is r multiplied by one another; it follows, that $3r^2$ is always greater than $8qs$: So that $\frac{3}{8}$ of either the Square of the ſecond Term, or of the Square of the fourth Term, muſt always be greater than the Product of the Terms adjacent to them.

Coroll.] Multiply either the Square of the ſecond Term, or the Square of the fourth Term of a *Biquadratic Æquation* by $\frac{3}{8}$, and if the Product does not exceed the Product of the adjacent Terms, ſome of the Roots of that Æquation muſt be impoſſible.

Prop. V.] *In an Æquation of any Dimenſion expreſſed by m, the Coefficients of the ſecond, third, laſt, laſt but one, and laſt but two Terms, being reſpectively A, B, E, D, C, if the Roots of the Æquation are all real, then ſhall $\overline{m - 1} \times A^2$ always be greater than $2mB$, and $\overline{m - 1} \times D^2$ greater than $2mCE$.*

1. For ſuppoſing the Roots to be a, b, c, d, e, &c. then by *Lemma* 4, ſhall $\overline{m - 1} \times a^2 + \overline{m - 1} \times b^2 + \overline{m - 1} \times c^2$ &c. be greater than $2ab + 2ac + 2ad$, &c. and adding $\overline{2m - 2} \times ab + \overline{2m - 2} \times ac + \overline{2m - 2} \times ad$, &c. to both, the Sum $\overline{m - 1} \times a^2 + 2\overline{m - 2} \times ab + \overline{m - 1} \times b^2 +$ &c. $(= \overline{m - 1} \times \overline{a + b + c, \&c.}^2$ muſt be greater than $2mab + 2mac + 2mad$, &c. that is, $\overline{m - 1} \times A^2$ muſt be greater than $2mB$.

2. In general, it follows from this Demonſtration, that the Square of the Sum of any Quantities whoſe Number is (m) multiplied by $m - 1$, muſt be greater than the Sum of all the Products can be

made by multiplying any two of them, multiplied by 2 m. But it is eaſy to ſee from the Geneſis of Æquations, that $C E$ is the Sum of the Products can be made by multiplying any two of the Terms whoſe Sum is D: From which it follows, that $\overline{m-1} \times D^2$ muſt be always greater than 2 $m C E$.

S I R,

IV. I now ſend you the Continuation of my Method of demonſtrating Sir *Iſaac Newton*'s *Rule*, &c. deduced from this Principle, that the Squares of the Differences of real Quantities muſt always be poſitive; and with it a ſhort Account of two other Methods on the ſame Subject: I have added ſome Obſervations on Æquations, which I take to be new, and, perhaps, will be more acceptable than what relates to the imaginary Roots themſelves.

The Continuation, with two other Methods on the ſame Subject, and new Obſervations on Æquations, by the ſame, No 408. p. 59.

Beſides Sir *Iſaac Newton*'s *Rule*, there ariſes from the following general Propoſitions, a great Variety of new Rules, different from his, and from any other hitherto publiſhed, for diſcovering when an Æquation has imaginary Roots. I ſhall particularly explain one that is more uſeful for that Purpoſe, than any that has been hitherto publiſhed.

Suppoſe there is an Æquation of (n) Dimenſions of this Form,

$$x^n - Ax^{n-1} + Bx^{n-2} - Cx^{n-3} + Dx^{n-4} - Ex^{n-5} + Fx^{n-6} - Gx^{n-7} + Hx^{n-8} - Ix^{n-9} + Kx^{n-10} \text{ \&c.} = 0.$$

And that the Roots of this Æquation are, a, b, c, d, e, f, g, h, i, k, l, &c. then ſhall $A = a + b + c + d + e + f$ &c. and therefore I call a, b, c, d, e, f, &c. *Parts* or *Terms* of the Coefficient A. For the ſame Reaſon I call ab, ac, ad, ae, bc, bd, cd, Parts or Terms of the Coefficient B; abc, abd, abc, acd, bcd, &c. Parts or Terms of C; $abcd$, $abce$, $abcf$, Parts or Terms of the Coefficient D, and ſo on. By the *Dimenſions* of any Coefficient, I mean the Number of Roots or Factors that are multiplied into each other in its Parts, which is always equal to the Number of Terms in the Æquation that precede that Coefficient. Thus A is a Coefficient of one Dimenſion, B of two, C of three, and ſo of the reſt. I call a Part or Term of a Coefficient C *ſimilar* to a Part or Term of any Coefficient G, when the Part of G involves all the Factors of the Part of C: Thus abc, $abcdefg$ are ſimilar Parts of C and G; after the ſame Manner $abcd$, $abcdef$ are ſimilar Parts of D and F, the Part of F involving all the Factors of the Part of D. Thoſe I call *diſſimilar* Parts that involve no common Root or Factor: Thus abc, and $defgh$ are diſſimilar Parts of the Coefficients C and F. The Sum of all the Products that can be made by multiplying the Parts of any Coefficient C by all the ſimilar Parts of G, I expreſs by $C' G'$ placing a ſmall Line over each Coefficient: After the ſame manner, $D'F'$ expreſſes the Sum of all the Products that can be made by multiplying the ſimilar Parts of D and F by each other; and $C' \times C'$ expreſſes the Sum of the Squares of the Parts of the Coefficient C, but $C' \times C_{,}$ expreſſes the Sum of the Products that can be made by multiplying any two Parts of C by one another. Theſe Expreſſions being underſtood, and the five Propoſitions in *Phil. Tranſ.* N° 394, being premiſed, next follows

Prop. VI.] *If the Difference of the Dimenſions of any two Coefficients* C *and* G *be called* (*m*) *then ſhall the Product of theſe Coefficients multiplied by one another be equal to* $C'G' + \overline{m+2} \times B'H' + \frac{m+3}{1} \times \frac{m+4}{2} A'I' + \frac{m+4}{1} \times \frac{m+5}{2} \times \frac{m+6}{3} \times I \times K$.

Where B and H are the Coefficients adjacent to the Coefficients C and G, A and I the Coefficients adjacent to B and H, I and K the Coefficients adjacent to B and H.

It is known that $C = abc + abd + abe + abf + abg$, &c. and $G = abcdefg + abcdefh + abcdefi + bcdefgh$, &c. and it is manifeſt,

1. That in the Product C G each Term of $C'G'$ will ariſe once as $a^2 b^2 c^2 defg$. But

2. Any Term of $B'H'$ as $a^2 b^2 cdefgh$ may be the Product of abc, and $abdefgh$, or of abd and $abcefgh$, or of abe and $abcdfgh$, or of abf and $abcdegh$, or of abg and $abcdefh$, or laſtly of abh and $abcdefg$; ſo that it may be the Product of any Term of C that involves with ab one of the Roots, c, d, e, f, g, h, multiplied by that Term of G, which involves ab and the other five; that is, it may ariſe in the Product C G as often as there are Roots in $a^2 b^2 cdefgh$ beſides a and b, or in general, as often as there are Units in the Difference of the Dimenſions of B and H, that is, $m + 2$ times; becauſe m expreſſes the Difference of the Dimenſions of C and G, and conſequently in expreſſing the Value of C G the Coefficient of the ſecond Term $B'H'$ muſt be $m + 2$.

3. Any Term of A I, as $a^2 bcdefghi$, may be the Product of any Part of C that involves the Root a with any two of the reſt b, c, d, e, f, g, h, i (the Number of which is the Difference of the Dimenſions of A and I, which is in general equal to $m + 4$) multiplied by the Part of G that involves a and the other ſix; and therefore $a^2 bcdefghi$ or any other Term of $A'I'$ muſt ariſe as often as different Products of two Quantities can be taken from Quantities whoſe Number is $m + 4$, that is $\overline{m+4} \times \frac{m+4-1}{2}$ times,

or $\frac{m+3}{1}\times\frac{m+4}{2}$ times; and conſequently in expreſſing the Value of C G the Coefficient of the third Term A′ I′ muſt be $\frac{m+3}{1}\times\frac{m+4}{3}$.

4. Any Term of 1 × K, as *a b c d e f g h i k*, may be the Product of any Part of C that involves three of its Factors, and of the Part of G that involves the reſt; and therefore may ariſe in the Product CG as often as different Products of three Quantities can be taken out of Quantities whoſe Number is $m+6$, that is, $\overline{m+6}\times\frac{m+5}{2}\times\frac{m+4}{3}$ times, and therefore the Coefficient of the fourth Term in the Value of C G muſt be $\frac{m+4}{1}\times\frac{m+5}{2}\times\frac{m+6}{3}$.

In general, in expreſſing the Value of the Product of any two Coefficients C and G, if x expreſs the Order of any Term of this Value as A′ I′, that is, the Number of Terms that precede it, the Coefficient of that Term muſt be $\frac{2x+m}{1}\times\frac{2x+m-1}{2}\times\frac{2x+m-2}{3}$ &c. taking as many Factors as there are Units in x.

Coroll. 1.] If it is required to find by this Propoſition the Square of any Coefficient E, then ſuppoſe $m=0$, the Difference of the Dimenſions of the Coefficients in this Caſe vaniſhing, and we ſhall have $E^2=E'\times E'+2D'F'+3\times\frac{4}{2}\times C'G'+4\times\frac{5}{2}\times\frac{6}{3}\times B'H'$ &c. $=E'\times E'+2D'F'+6C'G'+20B'H'+70A'I'+252K$. Therefore if $E'\times E,$ expreſs the Sum of the Products of any two Parts of E multiplied by each other, we ſhall have $E^2=E'\times E'+2E'\times E,$, and therefore $E'\times E,=D'F'+3C'G'+10B'H'+35A'I'+126K$.

Coroll. 2.] It follows from this Propoſition that

$$E^2=E'\times E'+2D'F'+6C'G'+20B'H'+70A'I'+252K,$$
$$DF=\cdots\cdots D'F'+4C'G'+15B'H'+56A'I'+210K$$

$$CG = \cdots\cdots\cdots C'G' + 6B'H' + 28A'I' + 120K$$
$$BH = \cdots\cdots\cdots B'H' + 8A'I' + 45K$$
$$AI = \cdots\cdots\cdots A'I' + 10K$$
$$K = \cdots\cdots\cdots K.$$

Coroll. 3.] It eaſily appears by comparing the Theorems given in the laſt Corollary, that

$$E'E' = E^2 - 2DF + 2CG - 2BH + 2AI - 2K$$
$$D'F' = \cdots\cdots DF - 4CG + 9BH - 16AI + 25K$$
$$C'G' = \cdots\cdots\cdots CG - 6BH + 20AI - 50K$$
$$B'H' = \cdots\cdots\cdots BH - 8AI + 35K$$
$$A'I' = \cdots\cdots\cdots AI - 10K.$$

Prop. VII.] *Let* $l = n \times \frac{n-1}{2} \times \frac{n-2}{3}$ &c. *taking as many Factors as the Coefficient* E *has Dimenſions, and* $\frac{l-1}{2l} \times E^2$ *ſhall always exceed* $DF - CG + BH - AI + K$ *when the Roots of the Æquation are all real Quantities.*

For it is manifeſt that l expreſſes the Number of Parts or Terms in the Coefficient E, and it is plain from Propoſition V. (*See Phil. Tranſ.* N° 394.) that $\frac{l-1}{2l} \times E^2$ muſt always be greater than the Sum of the Products that can be made by multiplying any two of the Parts of E by each other, that is, than $E' \times E_{,}$; but $2E' \times E_{,} = E^2 - E'E' =$ (by the firſt Theorem in the laſt Corollary) $2DF - 2CG + 2BH - 2AI + 2K$, and therefore ſince $\frac{l-1}{2l} \times E^2$ muſt always exceed $E'E_{,}$, it follows that $\frac{l-1}{2l} E^2$ muſt always be greater than $DF - CG + BH - AI + K$ when the Roots of the Æquation are real Quantities.

Schol.] In following my Method this was the firſt general Propoſition preſented it ſelf. For having firſt obſerved that if l expreſſes the Number of any Quantities, the Square of their Sum multiplied by $\frac{l-1}{2l}$ muſt always exceed the Sum of the Products made by multiplying any two of them by each other; and that the Exceſs was the Sum of the Squares of the Differences of the Quantities divided by $2l$. it was eaſy to ſee in the Æquation $x^n - Ax^{n-1} + Bx^{n-2} - Cx^{n-3} + Dx^{n-4}$ &c. $= 0$, ſince B is the Sum of the Products of any two of the Parts of A, that if l expreſſes the Number of the Root of the Æquation, $\frac{l-1}{2l} \times A^2$ muſt always exceed B; and this is one Part of the *fifth Propoſition.* In the next Place, I compared the Sum of the Products of any two Parts of B with AC, and found that it was not equal to AC but to AC — D; from which I inferred, that if l expreſſes the Number of the Parts of B, then $\frac{l-1}{2l} \times B^2$ muſt always exceed AC — D; and theſe eaſily ſuggeſted this general Propoſition.

Prop. VIII.] *Let r expreſs the Dimenſions of the Coefficient* C, *and s the Difference of the Dimenſions of the Coefficients* C *and* G, *then* B *and* H *being Coefficients adjacent to* C *and* G, $\overline{n-r-s} \times r\, C' G'$ *ſhall always be greater than* $\overline{s+1} \times \overline{s+2} \times B'H'$ *when the Roots of the Æquation are all real Quantities affected with the ſame Sign.*

For taking the Differences of all thoſe Parts of the Coefficient C that are ſimilar in all their Factors but one, as abc, abb, abi, &c. and multiplying the Square of each Difference by ſuch Parts of the Coefficient D (which is of s Dimenſions) as are diſſimilar to both the Parts of C in that Difference, the Sum of all thoſe Squares thus multiplied, will conſiſt of Terms of $C'G'$ taken poſitively, and of Terms of $B'H'$ taken negatively. By multiplying in this Manner

$$\overline{abc - abb}|^2 + \overline{abc - abi}|^2 + \overline{abc - abk}|^2 \text{ \&c.} +$$
$$\overline{abc - acb}|^2 + \overline{abc - aci}|^2 + \overline{abc - ack}|^2 \text{ \&c.} +$$
$$\overline{abc - bcb}|^2 + \overline{abc - bce}|^2 + \overline{abc - bck}|^2 \text{ \&c. by}$$

$defg$ the Term of D, that is diſſimilar to all thoſe Parts of C, you will find that $a^2b^2c^2defg$ will ariſe in the Sum of the Products $r \times \overline{n-r-s}$ times: For thoſe Products may be alſo expreſſed thus

$$defg\,a^2b^2 \times \overline{\overline{c-b}|^2 + \overline{c-i}|^2 + \overline{c-k}|^2 \text{ \&c.}}$$
$$+\, defg\,a^2c^2 \times \overline{\overline{b-b}|^2 + \overline{b-i}|^2 + \overline{b-k}|^2 \text{ \&c.}}$$
$$+\, defg\,b^2c^2 \times \overline{\overline{a-b}|^2 + \overline{a-i}|^2 + \overline{a-k}|^2 \text{ \&c.}}$$

where the Number of the Differences $c-b$, $c-i$, $c-k$, &c. whoſe Squares are multiplied by $defg\,a^2b^2$ is manifeſtly equal to the Number of the Roots of the Æquation that do not enter $a^2b^2c^2defg$ or $abcdefg$, that is, to the Exceſs of the Number of the Roots of the Æquation above the Dimenſions of $abcdefg$, a Term of G, that is, to $n-r-s$. But in collecting all the ſaid Products, $\overline{n-r-s}\times a^2b^2c^2defg$ muſt ariſe as often as there are Units in r: Becauſe the Terms which are ſubtracted from abc may differ from it in the Root c, as abb, abi, abk, &c. or in the Root b, as acb, aci, ack, &c. or in the Root a, as bcb, bci, bck; that is, $\overline{n-r-s}\times a^2b^2c^2defg$ muſt ariſe as often as there are Dimenſions in abc, a Term of C, or as often in general as there are Units in r, which expreſſes the Dimenſions of C: Therefore the Term $a^2b^2c^2defg$ will ariſe in the Sum of the abovementioned Products $r\times\overline{n-r-s}$ times.

The negative Part muſt conſiſt of the Terms of B′H′ doubled; each of which, as $2a^2b^2cdefg$ may ariſe as often as there can be Differences $c-d$, $c-e$, $c-f$, $c-g$, $d-e$, &c. aſſumed amongſt the Terms c, d, e, f, g whoſe Number is $= \text{to } s+2$ that is, $\overline{s+2}\times\frac{s+1}{2}$ times; and therefore a^2b^2cdefg or any other Part of B′H′ muſt ariſe in the negative Part $\overline{s+1}\times\overline{s+2}$ times; and ſince the whole Aggregate muſt be poſitive it follows $\overline{n-r-s}\times r\,C'G'$ muſt always exceed $\overline{s+1}\times\overline{s+2}\times B'H'$.

Coroll. 1] Suppoſe we are to compare E′E′ the Sum of the Squares of the Parts of E with D′F′ the Sum of the Products of the ſimilar Parts of D and F; in this Caſe s vaniſhes, and therefore $\overline{n-r}\times r\,E'E'$ muſt exceed $2\,D'F'$. Let $\overline{n-r}\times r=m$, and conſequently $\overline{n-r-1}\times\overline{r-1}=m-n+1$; $\overline{n-r-2}\times\overline{r-2}=m-2n+4$; $\overline{n-r-3}\times\overline{r-3}=m-3n+9$; $\overline{n-r-4}\times\overline{r-4}=m-4n+16$. Since it is plain that $\overline{n-r-q}\times\overline{r-q}=\overline{n-r}\times r-qn+q^2$, Then by this Propoſition, ſuppoſing

$$m\times E'E'-2D'F'=a'$$

$$\overline{m-n+1}\times D'F'-12\,C'G'=b'$$

$$\overline{m-2n+4}\times C'G'-30\,B'H'=c'$$

$$\overline{m-3n+9}\times B'H'-56\,A'I'=d'$$

$$\overline{m-4n+16}\times A'I'-90\,K'=e'$$

The Quantities a', b', c', d', e', muſt be always poſitive when the Roots of the Æquation are real Quantities affected with the ſame Sign. The Coefficients prefixed to the negative Parts are the Numbers 2, 12, 30, 56, 90, whoſe Differences equally increaſe by the ſame Number 8.

Coroll. 2] Suppoſing as before, that $\overline{n-r}\times r=m$; and alſo that $m\times\overline{m-n+1}=m'$; $m'\times\overline{m-2n+4}=m''$; $m''\times\overline{m-3n+9}=m'''$ &c. it may be demonſtrated after the manner of this Propoſition, that if

$$m\,E'E'-2\,D'F'=a'$$
$$m'\,E'E'-2\times 12\,C'G'=a''$$
$$m''E'E'-2\times 12\times 30\,B'H'=a'''$$
$$m'''E'E'-2\times 12\times 30\times 56\,A'I'=a''''\ \&c.$$

Then ſhall a', a'', a''', &c. be always poſitive when the Roots are real Quantities, whether they be affected with the ſame, or with different Signs. The negative Coefficients ariſe by multiplying thoſe in the preceding Corollary, 2, 12, 30, 56, 90, by one another.

Prop. IX.] *Let a', b', c', d', e', and m expreſs the ſame Quantities as in the Corollaries of the laſt Propoſition, and* $m\,E^2-\overline{m+n+1}\times DF=a'+b'+2c'+5d'+14e'$.

For by Cor. ii. Prop. vi.

$E^2=E'E'+2\,D'F'+6\,C'G'+20\,B'H'+70\,A'I'+252\,K$,

and by the ſame

$DF=\text{- - - - -}\ D'F'+4\,C'G'+15\,B'H'+56\,A'I'+210\,K$;

therefore $m\,E^2-\overline{m+n+1}\times DF=m\,E'E'+\overline{m-n-1}\times D'F'+\overline{m-2n-2}\times 2\,C'G'+\overline{m-3n-3}\times 5\,B'H'+\overline{m-4n-4}\times 14\,A'I'+\overline{m-5n-5}\times 42\,K=$ (by ſubſtituting ſucceſſively for $m\,E'E'$, $\overline{m-n+1}\times D'F'$, $\overline{m-2n+4}\times C'G'$, $\overline{m-3n+9}\times B'H'$, $\overline{m-4n+16}\times A'I'$ their Values deduced from the firſt Corollary of the laſt Propoſition) $=a'+b'+2c'+5d'+14e'$, where the Coefficients prefixed to a', b', c', d, e', are the Differences of the Coefficients of $E'E'$, $D'F'$, $C'G'$, $B'H'$, $A'I'$, and K in the Values of E^2 and DF taken from *Cor.* ii. Prop. VI. being $1-0$, $2-1$, $6-4$, $20-15$, $70-56$ and $252-210$.

Coroll. Since $m=\overline{n-r}\times r$ therefore $m+n+1=\overline{n-r+1}\times\overline{r+1}$; and conſequently $\dfrac{r}{r+1}\times\dfrac{n-r}{n-r+1}\times E^2$ muſt always be greater than DF the Product of the Coefficients adjacent to E; and hence the Fractions are deduced, that in Sir *Iſaac Newton*'s Rule are placed over the Terms of the Æquation, which multiplied by the Square of the Terms under them, muſt always exceed the Products of the adjacent Terms of the Æquation, when the Roots are real Quantities: For it is manifeſt that the Fraction to be placed over the Term Ex^{n-r} according to that Rule is the Quotient of $\frac{n-r}{r+1}$ divided by $\frac{n-r+1}{r}$.

Prop. 10. *The ſame Expreſſions being allowed as in the preceding Propoſitions, it will be found in the ſame manner that as*

$$mE^2 - \overline{m+n+1} \times DF = a' + b' + 2c' + 5d' + 14e', \text{ ſo}$$

$$\overline{m-n+1} \times DF - \overline{m+2n+4} \times CG = \cdots b' + 3c' + 9d' + 28e'$$

$$\overline{m-2n+4} \times CG - \overline{m+3n+9} \times BH = \cdots\cdots c' + 5d' + 20e'$$

$$\overline{m-3n+9} \times BH - \overline{m+4n+16} \times AI = \cdots\cdots\cdots d' + 7e'$$

$$\overline{m-4n+16} \times AI - \overline{m+5n+25} \times K = \cdots\cdots\cdots\cdots e'.$$

Theſe Theorems are eaſily deduced from the Theorems given in the ſecond *Coroll. Prop.* VI. and the firſt *Coroll. Prop.* VIII; and the Coefficients prefixed to a', b', c', d', e', are the Differences of the Coefficients of the correſponding Terms in the Values of E^2, DF, CG, BH, AI and K in *Cor.* 2. *Prop.* VI.

Corol.] Hence the Products of any two Coefficients, as DF and AI may be compared together when the Sum of the Dimenſions of D and F is equal to the Sum of the Dimenſions of A and I. Let the Dimenſions of A and F be equal to s and m reſpectively, and let $p = \frac{n-s}{s+1} \times \frac{n-s-1}{s+2} \times \frac{n-s-2}{s+3}$ &c. taking as many Factors as there are Units in the Difference of the Dimenſions of D and A. Let $q = \frac{n-m}{m+1} \times \frac{n-m-1}{m+2} \times \frac{n-m-2}{m+3}$ &c. taking as many Factors as you took in the Value of p. Then ſhall $\frac{q}{p} \times DF$ always exceed AI when the Roots of the Æquation are real Quantities affected with the ſame Sign; and this Rule obtains, though the Roots are affected with different Signs when the Coëfficients D and F are equal.

Prop. XI.] *The ſame Things being ſuppoſed as in the preceding Propoſitions.*

$$1.\ \left.\begin{array}{l} mE^2 - \overline{m+1}\times 2DF + \overline{m+4}\times 2CG - \overline{m+9}\times \\ 2BH + \overline{m+16}\times 2AI - \overline{m+25}\times 2K\ \text{- - -} \end{array}\right\} = a'.$$

$$2.\ \left.\begin{array}{l} \overline{m-n+1}\times DF - \overline{m-n+4}\times 4CG + \overline{m-n+9}\times \\ 9BH - \overline{m-n+16}\times 16AI + \overline{m-n+25}\times 25K \end{array}\right\} = b'.$$

$$3.\ \left.\begin{array}{l} \overline{m-2n+4}\times CG - \overline{m-2n+9}\times 6BH + \overline{m-2n+16}\times \\ 20AI + \overline{m-2n+25}\times 50K\ \text{- - - - - - - -} \end{array}\right\} = c'.$$

$$4.\ \overline{m-3n+9}\times BH - \overline{m-3n+16}\times 8AI + \overline{m-3n+25}\times 35K = d'.$$

$$5.\ \text{- - - -}\ \overline{m-4n+16}\times AI - \overline{m-4n+25}\times 10K = e'.$$

Theſe Theorems follow eaſily from the third *Coroll. Prop.* VI. The firſt eaſily appears thus, $a' = mE'E' - 2D'F' =$ (by that Corollary)

$$\begin{array}{l} mE^2 - 2mDF + 2mCG - 2mBH + 2mAI - 2mK. \\ \quad - 2DF + 8CG - 18BH + 32AI - 50K. \end{array}$$

$= mE^2 - \overline{m+1}\times 2DF + \overline{m+4}\times 2CG - \overline{m+9}\times 2BH + \overline{m+16}\times 2AI - \overline{m+25}\times 2K$. The other Theorems are deduced from the ſame Corollary compared with *Coroll.* 1. *Prop.* VIII.

Prop. XII.] *The ſame Things being ſuppoſed as in the ſecond Corollary of* Prop. VIII.

$$1.\ \left.\begin{array}{l} mE^2 - \overline{m+1}\times 2DF + \overline{m+4}\times 2CG - \overline{m+9}\times \\ 2BH + \overline{m+16}\times 2AI - \overline{m+25}\times 2K\ \text{- - - - - -} \end{array}\right\} = a'.$$

$$2.\ \left.\begin{array}{l} m'E^2 - 2m'DF + \overline{m'-12}\times 2CG - \overline{m'-72}\times \\ 2BH + \overline{m'-240}\times 2AI - \overline{m'-600}\times 2K\ \text{- - - -} \end{array}\right\} = a''.$$

$$3.\ \left.\begin{array}{l} m''E^2 - 2m''DF + 2m''CG - \overline{m''+360}\times \\ 2BH + \overline{m'''+360\times 8}\times 2AI - \overline{m'''+360\times 35}\times 2K \end{array}\right\} = a'''.$$

$$4.\ \left.\begin{array}{l} m'''\times\overline{E^2 - 2DF + 2CG - 2BH} + \overline{m'''-750\times 28}\times \\ AI - \overline{m''''-7200\times 28}\times 2K\ \text{- - - - - - - - -} \end{array}\right\} = a''''.$$

&c.

Theſe *Theorems* follow from the third *Coroll. Prop.* VI. compared with the ſecond *Coroll. Prop.* VIII. The firſt is the ſame with the firſt of the laſt Propoſition. The ſecond is demonſtrated by ſubſtituting in $m' E' E' - 24 C' G' = a''$, the Values of $E' E'$ and $C' G'$ given in the 3d *Cor.* of the 6th *Prop.* The third is found by ſubſtituting in $m'' E' E' - 270 B' H' = a'''$ the Values of $E' E'$ and $B' H'$; and by a like Subſtitution theſe *Theorems* may be continued.

A general Corollary.] From theſe Propoſitions a great Variety of *Rules* may be deduced for diſcovering when an Æquation has imaginary Roots. The Foundation of Sir *Iſaac Newton*'s Rule is demonſtrated in the *ninth Propoſition*, and *its Corollary.* The 7th Propoſition ſhews that if $\frac{l-1}{2l} \times E^2$ does not exceed $DF - CG + BH - AI + K$, ſome of the Roots of the Æquation muſt be imaginary; and ſometimes this Rule will diſcover impoſſible Roots in an Æquation, that do not appear by Sir *Iſaac Newton*'s Rule. Theſe are the only two Rules that have been hitherto publiſhed. But the Rules that ariſe from the Theorems in the 11th and 12th Propoſitions, are preferable to both; becauſe any imaginary Roots that can be diſcovered by the 7th or 9th always appear from the 11th and 12th *Propoſitions*; and impoſſible Roots will often be diſcovered by the 11th and 12th *Propoſitions* in an Æquation, that do not appear in that Æquation when examined by the 7th and 9th *Propoſitions.* The Advantage which the Rules deduced from the 11th *Propoſition*, have above thoſe deduced from the preceding Propoſitions, will be manifeſt by conſidering that in the 11th Propoſition we have the Values of the Quantities a', b', c', d', e', ſeparately; whereas in the preceding Propoſitions, we have only the Values of certain Aggregates of theſe Quantities joined with the ſame Signs. Now it is obvious that if theſe Quantities be ſeparately found poſitive, any ſuch Aggregates of them muſt be poſitive; but theſe Aggregates may be poſitive, and yet ſome of the Quantities a', b', c', d', e', themſelves may be found negative: From which it follows, that if the Roots of the Æquation are all affected with the ſame Sign, and no impoſſible Roots appear by *Prop.* 11th, none will appear by the preceding Propoſitions; but that ſome imaginary Roots may be diſcovered by Propoſition 11th, when none appear in the Æquation examined by the Propoſitions that precede the 11th. If ſome of the Roots of the Æquation are poſitive, and ſome negative (which always eaſily appears by conſidering the Signs of the Terms of the Æquation) then the 12th Propoſition will be in many Caſes more apt to diſcover imaginary Roots in an Æquation than thoſe that precede it.

The *Rule* that flows from the firſt *Theorem* of the 11th *Prop.* obtains when the Roots of the Æquation are affected with different Signs

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all the ſame Sign, then A C muſt exceed 16 D: Let the Exceſſes

The *Rule* that flows from the firſt *Theorem* of the 11th *Prop.* obtains when the Roots of the Æquation are affected with different

Signs, as well as when they all have the ſame Sign, and it is this; Multiply the Number of the Terms in an Æquation that precedes any Term, as Ex^{n-r} by the Number of Terms that follow it in the ſame Æquation, and call the Product m. Suppoſe that $+D-C$, $+B$, $-A$, $+I$ are the Coefficients preceding the Term Ex^{n-r}, and that $+F$, $-G$, $+H$, $-I$, $+K$ are the Coefficients that follow it; then if $\frac{1}{2}mE^2$ does not exceed $\overline{m+1}\times DF-\overline{m+4}\times CG+\overline{m+9}\times BH-\overline{m+16}\times AI+\overline{m+25}\times K$, the Æquation muſt have ſome imaginary Roots; where the Coefficients $m+1$, $m+4$, $m+9$, &c. are found by adding to m the Squares of the Numbers 1, 2, 3, 4, &c. which ſhew the Diſtances of the Coefficients to which they are prefix'd, from the Coefficient E. The ſecond Theorem of the 12th Propoſition ſhews, that if $\frac{1}{2}m'E^2$ does not exceed $m'DF-\overline{m'-12}\times CG+\overline{m-72}\times BH-\overline{m'-240}\times AI+\overline{m'-600}\times K$, the Æquation muſt have ſome Roots imaginary.

For an Example, If the four Roots of the Biquadratick Æquation $x^4-Ax^3+Bx^2-Cx+D=0$ are real Quantities, it will follow equally from the 5th, 7th, 9th, and 11th Propoſitions, that $\frac{3}{8}A^2$ muſt be greater than B, and that $\frac{3}{8}C^2$ muſt exceed BD. The 7th further ſhews that $\frac{5}{12}B^2$ muſt exceed $AC-D$; the 9th demonſtrates that $\frac{4}{9}B^2$ muſt exceed AC; but our Rule deduced from Prop. XI. ſhews that $2B^2$ muſt exceed $5AC-8D$, the Exceſs being $\frac{1}{2}a'$, and the Rule deduced from the ſecond Theorem of the 12th Propoſition ſhews that B^2 muſt always exceed $2AC+4D$, the Exceſs being $\frac{1}{4}a''$. It appears from ſeveral preceding Propoſitions, that if the Roots of the Æquation have all the ſame Sign, then AC muſt exceed 16 D: Let the Exceſſes

$5B^2-12AC+12D=p$, $4B^2-9AC=q$, $AC-16D=s$; and it is plain that a' $(=4B^2-10AC+16D)=q-s=\frac{2}{5}\times\overline{2p-s}$; and that $a''=q+s=\frac{2}{5}\times\overline{2p+4s}$. Let us ſuppoſe,

1. That s is poſitive, then it is manifeſt that if either p or q be negative, a' muſt alſo be found negative, and conſequently that when the 7th or 9th Propoſitions ſhew any Roots to be imaginary, the 11th Propoſition muſt diſcover them at the ſame time. But as a' $(=q-s=\frac{2}{5}\times\overline{2p-s})$ may be found negative when p and q are both poſitive, it follows that the Rule we have deduced from the 11th Propoſition may diſcover imaginary Roots in an Æquation, that do not appear by the preceding Propoſitions: Thus if you examine the Æquation $x^4-6x^3+10x^2-7x+1$ by Sir *Iſaac Newton*'s Rule, or by our 7th *Propoſition*, no imaginary Roots appear in it from either. But ſince $2B^2-5AC+8D\ (=\frac{1}{2}a')$ $=200-210+8=-2$ is in this Æquation negative, it is manifeſt that two Roots of the Æquation muſt be imaginary. Let us ſuppoſe,

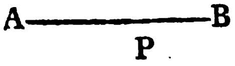
2. That s is negative, and that from the Signs of the Terms of the Æquation, it appears that ſome Roots are poſitive and ſome negative; then in order to ſee if the Æquation has any imaginary Roots, the moſt uſeful Rule is that we deduced from the ſecond Theorem of *Prop.* 12th. *viz.* that if B^2 does not exceed $2AC+4D$ ſome of the Roots of the Æquation muſt be imaginary: For the Exceſs of B^2 above $2AC+4D$ being $\frac{1}{4}a''=\frac{1}{4}\times\overline{q+s}=\frac{1}{10}\times\overline{2p+4s}$, and s being negative, it is manifeſt, that if q or p be negative, $\frac{1}{4}a''$ muſt be negative; and that $\frac{1}{4}a''$ may be negative when q and p are both poſitive; that is, This Rule muſt always diſcover ſome Roots to be imaginary when the 7th or 9th *Prop.* diſcover any impoſſible Roots in an Æquation; and will very often diſcover ſuch Roots in an Æquation when theſe Propoſitions diſcover none. *For Example*, If you examine the Æquation $x^4+5x^3+6x^2-x-12=0$, you will diſcover no imaginary Roots

in it by the 7th *or* 9th *Propoſitions*; and though $AC - 16D (= s)$ be negative, it does not follow, that the Æquation has any impoſſible Roots, becauſe it appears from the Signs of the Terms, that the Æquation has Roots affected with different Signs. But ſince $B^2 - 2AC - 4D (= 36 + 10 - 48 = -2)$ is negative, it appears from our Rule, that the Æquation muſt have ſome imaginary Roots.

I might ſhew in the next Place, how the Rules deduced from the 11th *and* 12th *Propoſitions* may be extended ſo as to diſcover when more than two Roots of an Æquation are imaginary, and in general to determine the Number of imaginary Roots in any Æquation; but as it would require a long Diſcuſſion, and ſome *Lemmata* to demonſtrate this ſtrictly, I ſhall only obſerve that the 11th *and* 12th *Propoſitions* will be found to be ſtill the moſt uſeful of all thoſe we have given for that Purpoſe. To give *one Example* of this; If we are to examine the Æquation $\underset{+}{x^4} - \underset{-}{4ax^3} + \underset{+}{6a^2x^2} - \underset{-}{4ab^2x} + \underset{+}{b^4} = 0$ by Sir *Iſaac Newton*'s Rule, it is found to have four impoſſible Roots when a is greater than b; for though the Square of the ſecond Term multiplied by $\frac{3}{8}$ be equal to the Product of the firſt and third Terms, yet in that Caſe, in applying Sir *Iſaac Newton*'s Rule, the Sign — ought to be placed under the ſecond Term, and the ſame is to be ſaid of the Square of the fourth Term. The Rule deduced from the 7th Propoſition ſhews four Roots imaginary, when a is greater than b, and alſo when b^2 is greater than $15a^2$; but a Rule founded on the 11th Propoſition, ſhews the four Roots to be imaginary always when a exceeds b, or when b^2 exceeds $9a^2$; from which the Excellency of this Rule above theſe two is manifeſt. I have ſaid ſo much of Biquadratick Æquations, that I muſt leave it to thoſe that are willing to take the Trouble, to make like Remarks on the higher Sorts of Æquations.

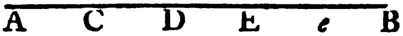
In inveſtigating the preceding Propoſitions, when I found my ſelf obliged to go through ſo intricate Calculations, I often attempted to find ſome more eaſy Way of treating this Subject. The following was of conſiderable Uſe to me, and may perhaps be entertaining to you. By it, I inveſtigate ſome *maxima* in a very eaſy Manner, that could not be demonſtrated in the common Way with ſo little Trouble.

Lemma V.] Let the given Line AB be divided any where in P, and the Rectangle of the Parts AP and PB will be a *maximum* when theſe Parts are equal.



This is manifeſt from the Elements of *Euclid.*

Lemma VI.] If the Line A B is divided into any Number of Parts A C, C D, D E, E B, the Product of all thoſe Parts multiplied into one another will be a *maximum* when the Parts are equal amongſt themſelves. For let the Point D be where you will, it is manifeſt that if D B be biſſected in E, the Product A C x C D x D E x E B will be greater than A C x C D x D *e* x *e* B, becauſe by the laſt Lemma D E x E B is greater than D *e* x *e* B; and for the ſame reaſon A D and C E muſt be biſſected in C and D; and conſequently all the Parts A C, C D, D E, E B muſt be equal amongſt themſelves, that their Product may be a *maximum.*



Lemma VII.] The Sum of the Products that can be made by multiplying any two Parts of A B by one another is a *maximum* when the Parts are equal. The Sum of theſe Products is $AC \times CB + CD \times DB + DE \times EB$: Now that D E x E B may be a *maximum,* D B muſt be biſſected in E by the 5th Lemma, and for the ſame reaſon A D and C E muſt be biſſected in C and D, that is, all the Parts, A C, C D, D E, E B muſt be equal, that the Sum of all theſe Products may be a *maximum.*

Lemma VIII.] The Sum of the Products of any three Parts of the Line A B is a *maximum,* when all the Parts are equal. For that Sum is $AC \times CD \times DE + EB \times \overline{AC \times CD + AC \times DE + CD \times DE}$; and ſuppoſing the Point E given, it is manifeſt that A E muſt be equally triſected in C and D that A C x C D x D E may be a *maximum* by Lemma VI. and that $AC \times CD + AC \times DE + CD \times DE$ may be a *maximum* by Lemma VII. From which it is manifeſt that all the Parts A C, C D, D E, E B muſt be equal, that the Sum of the Products of any three of them may be a *maximum.*

Lemma IX.] It is manifeſt that this way of reaſoning is general, and that the Sum of any Quantities being given, the Sum of all the Products that can be made by multiplying any given Number of them by one another, muſt be a *maximum* when theſe Quantities are equal. But the Sum of the Squares, or of any pure Powers of theſe Quantities, is a *minimum,* when the Quantities are equal.

Theorem.] *Suppoſe* $x^n - Ax^{n-1} + Bx^{n-2} - Cx^{n-3} + Dx^{n-4} - Ex^{n-5}$ &c. $= 0$, *to be an Æquation that has not all its Roots equal to one another: Let r expreſs the Dimenſions of any Coefficient* D, *and let* $l = n \times \frac{n-1}{2} \times \frac{n-2}{3} \times \frac{n-3}{4}$ &c. *taking as many Factors as there are Units in r; then ſhall* $\frac{l}{n^r} \times A^r$ *be always greater than* D, *if* *if the Roots of the Æquation are real Quantities affected with the ſame Sign.*

This may be demonſtrated from the preceding Propoſitions: But to demonſtrate it from the laſt *Lemmata,* let us aſſume an Æquation that has all its Roots equal to one another, and the Sum of all its Roots equal to A, the Sum of the Roots of the propoſed Æquation. This Æquation will be $\overline{x-\frac{1}{n}A}\Big|^{n}=0$, $x^{n}-Ax^{n-1}+n\times\frac{n-1}{2}\times\frac{A^{2}}{n^{2}}x^{n-2}-n\times\frac{n-1}{2}\times\frac{n-2}{3}\times\frac{A^{3}}{n^{3}}x^{n-3}$ &c. $=0$; and if r expreſs the Dimenſions of the Coefficient of any Term of this Æquation (or the Number of Terms which precede it) it is manifeſt that the Term itſelf will be $l\times\frac{A^{r}}{n^{r}}x^{n-r}$: But by the Suppoſition Dx^{n-r} is the Correſponding Term in the propoſed Æquation, and D muſt be the Sum of all the Products that can be made by multiplying as many Roots of that Æquation by one another, as there are Units in r; and $\frac{lA^{r}}{n^{r}}$ muſt be the Sum of the like Products of the Roots of the other Æquation; which muſt be the greater Quantity by the preceding Lemmata, becauſe its Roots are equal amongſt themſelves, and their Sum is equal to the Sum of the Roots of the propoſed Æquation; and the Sum of ſuch Products is a *maximum* when the Roots are equal amongſt themſelves. By purſuing this Method, it may be demonſtrated that $\overline{\frac{2B}{n\times\overline{n-1}}}\Big|^{\frac{r}{2}}\times l$ muſt always exceed the Coefficient prefixed to the Term x^{n-r} in an Æquation whoſe Roots are all real Quantities affected with the ſame Sign; providing that r be a Number greater than 2; and alſo that $\overline{\frac{2\times 3c}{n\times\overline{n-1}\times\overline{n-2}}}\Big|^{\frac{r}{3}}\times l$ muſt exceed the ſame Coefficient, if r be any Number greater than 3.

It is eaſy to continue theſe Theorems.

The 3*d Method* which I mentioned in the Beginning of this Letter, is deduced from the Conſideration of the *Limits* of the Roots of Æquations; and though it is explained by ſome Authors already, yet as I demonſtrate and apply it to this Subject in a different Manner, I ſhall add a ſhort Account of it.

Lemma X.] If you transform the Biquadratick $x^4 - Ax^3 + Bx^2 - Cx + D = 0$ into one that ſhall have each of its Roots leſs than the reſpective Values of x by a given Difference e; Suppoſe $y = x - e$, or $x = e + y$, the transformed Æquation, the Order of the Terms being inverted, will have this Form:

$$\begin{array}{lllll} e^4 & +4e^3y & +6e^2y^2 & +4ey^3 & +y^4 = 0. \\ -Ae^3 & -3Ae^2y & -3Aey^2 & -Ay^3 & \\ +Be^2 & +2Bey & +By^2 & & \\ -Ce & -Cy & & & \\ +D & & & & \end{array}$$

Where it is manifeſt,

1. That the firſt Term $e^4 - Ae^3 + Be^2 - Ce + D$ is the Quantity that ariſes by ſubſtituting e in Place of x in the propoſed Æquation $x^4 - Ax^3 + Bx^2 - Cx + D$.

2. That the Coefficient of the ſecond Term $4e^3 - 3Ae^2 + 2Be - C$ is the Quantity that ariſes by multiplying each Part of the firſt $e^4 - Ae^3 + Be^2 - Ce + D$ by the Index of e in that Part, and dividing the Product by e.

3. That the Coefficient of the third Term $6e^2 - 3Ae + B$ is the Quantity that ariſes from the preceding Coefficient $4e^3 - 3Ae^2 + 2Be - C$ by multiplying each Part by the Index of e in it, and dividing the Product by $2e$.

4. That the Coefficient of the fourth Term ariſes in like Manner from the preceding, only you now divide by $3e$; and in general, the Coefficient of any Term may be deduced from the Coefficient of that Term which precedes it, by multiplying each Part of the preceding Coefficient by the Index of e in that Part, and dividing the Product by e and by the Index of y, in the Term whoſe Coefficient is required.

Lemma XI.] If any Æquation $x^n - Ax^{n-1} + Bx^{n-2} - Cx^{n-3}$ &c. $= 0$ be transformed in the ſame Manner, by ſuppoſing $y = x - e$, or $x = e + y$, and conſequently $x^n = \overline{e+y}|^{n}$, $Ax^{n-1} = A \times \overline{e+y}|^{n-1}$, $Bx^{n-2} = B \times \overline{e+y}|^{n-2}$ &c. The transformed Æquation will have this Form, the Order of the Terms being inverted,

$$e^n + ne^{n-1}y + n \times \frac{n-1}{2} \times e^{n-2}y^2 \text{ \&c.} = 0$$

$$\begin{array}{r} -Ae^{n-1} - \overline{n-1} \times Ae^{n-2}y - \overline{n-1} \times \frac{n-2}{2} \times Ae^{n-3}y^2 \text{ \&c.} \\ +Be \end{array}$$

$$+Be^{n-2}+\overline{n-2}\times Be^{n-3}y+\overline{n-2}\times\frac{n-3}{2}\times Be^{n-4}y^2\ \text{\&c.}$$

$$-Ce^{n-3}-\overline{n-3}\times Ce^{n-4}y-\overline{n-3}\times\frac{n-4}{2}\times Ce^{n-5}y^2\ \text{\&c.}$$

&c. &c. &c.

Where it is manifeſt,

1. That the firſt Term $e^n - Ae^{n-1} + Be^{n-2} - Ce^{n-3}$ &c. is the Quantity that ariſes by ſubſtituting e in the Place of x in the propoſed Æquation $x^n - Ax^{n-1} + Bx^{n-2} - Cx^{n-3}$ &c.

2. That the Coefficient of the ſecond Term $ne^{n-1} - \overline{n-1}\times Ae^{n-2} + \overline{n-2}\times Be^{n-3} - \overline{n-3}\times Ce^{n-4}$ &c. is deduced from the preceding $e^n - Ae^{n-1} + Be^{n-2} - Ce^{n-3}$ &c. by multiplying each of its Parts by the Index of e in that Part, and dividing by e.

3. That the Coefficient of the third Term is deduced from the Coefficient of the ſecond Term, by multiplying after the ſame Manner, each of its Parts by the Index of e and dividing by $2e$. In general, the Coefficient of any Term, y^r is deduced from the Coefficient of the preceding Term, that is of y^{r-1} by multiplying every Part of that Coefficient by the Index of e in it, and dividing the Product by re.

Lemma XII.] If you ſubſtitute any two Quantities K and L in the Place of x in $x^4 - Ax^3 + Bx^2 - Cx + D$, and the Quantities that reſult from theſe Subſtitutions be affected with contrary Signs, the Quantities K and L muſt be *Limits* of one or more real Roots of the Æquation $x^4 - Ax^3 + Bx^2 - Cx + D = 0$. That is, one of theſe Quantities muſt be greater, and the other leſs than one or more Roots of that Æquation.

For if you ſuppoſe that a, b, c, d, are the Roots of that Æquation, then it is plain from the *Geneſis* of Æquations, that $x^4 - Ax^3 + Bx^2 - Cx + D = \overline{x-a}\times\overline{x-b}\times\overline{x-c}\times\overline{x-d}$; and therefore K and L being ſubſtituted for x in $\overline{x-a}\times\overline{x-b}\times\overline{x-c}\times\overline{x-d}$, the Product becomes in the one Caſe poſitive, and in the other negative; ſo that one of the Factors $x-a$, $x-b$, $x-c$, $x-d$ muſt have a Sign when K is ſubſtituted for x in it, contrary to the Sign which it is affected with when L is ſubſtituted in it for x; ſuppoſe that Factor to be $x-b$, and ſince $K-b$ and $L-b$ are Quantities whereof the one is poſitive,

and

and the other negative, it is manifeſt that b one of the Roots of the Æquation muſt be leſs than one, and greater than the other of the two Quantities K and L: So that K and L muſt be the *Limits* of the Root b.

I ſay further, that the Root whereof K and L are *Limits*, muſt be a real Root of the Æquation; for the Product of the Factors that involve impoſſible Roots in an Æquation, can never have its Signs changed by ſubſtituting any real Quantity whatſoever in place of x; becauſe the Number of ſuch Roots is always an even Number, and the Product of any two of theſe Roots ſuch as $x-m-\sqrt{-n}$, and $x-m+\sqrt{-n}$ is $\overline{x-m}|^2+n^2$ which muſt be always poſitive, whatever Quantity be ſubſtituted for x, while n remains poſitive, that is, while theſe two Roots are impoſſible.

Lemma XIII.] If you ſubſtitute K and L for x in $x^n-Ax^{n-1}+Bx^{n-2}$ &c. and the Quantities that reſult be affected with contrary Signs, then ſhall K and L be the *Limits* of one or more real Roots of the Æquation $x^n-Ax^{n-1}+Bx^{n-2}$ &c. $=0$. This may be demonſtrated after the ſame Manner as the laſt *Lemma*.

THEOREM I.] *If* a, b, c, d *are the Roots of the Æquation* $x^4-Ax^3+Bx^2-Cx+D=0$, *they ſhall be the* Limits *of the Roots of the Æquation* $4x^3-3Ax^2+2Bx-C=0$.

Suppoſe a to be the leaſt Root of the Biquadratick $x^4-Ax^3+Bx^2-Cx+D=0$, b the ſecond Root, c the third, and d the fourth, and the Values of y in the Æquation in the 10th *Lemma*, will be $a-e$, $b-e$, $c-e$, $d-e$; then by ſubſtituting ſucceſſively a, b, c, d for e in that Æquation of y, one of the Values of y will vaniſh in every Subſtitution, and the firſt Term of the Æquation of y, *viz.* $e^4-Ae^3+Be^2-Ce+D$ vaniſhing, the Æquation will be reduced to a Cubick of this Form.

$$\begin{array}{llll} 4e^3 & +6e^2y & +4ey^2 & +y^3=0 \\ -3Ae^2 & -3Aey & -Ay^2 & \\ +2Be & +By & & \\ -C & & & \end{array}$$

And conſequently $4e^3-3Ae^2+2Be-C$ muſt be the Product of the three remaining Values of y having its Sign changed; that is, it muſt be equal to $-\overline{b-a}\times\overline{c-a}\times\overline{d-a}$ when e is ſuppoſed equal to a; it muſt be $-\overline{a-b}\times\overline{c-b}\times\overline{d-b}$ when $e=b$; it muſt be $-\overline{a-c}\times\overline{b-c}\times\overline{d-c}$ when $e=c$; and it muſt be $-\overline{a-d}\times\overline{b-d}\times\overline{c-d}$ when $c=d$. Now it is manifeſt that theſe Products $\overline{b-a}\times\overline{c-a}\times\overline{d-a}$, $\overline{a-b}\times\overline{c-b}\times\overline{d-b}$, $\overline{a-c}\times\overline{b-c}\times\overline{d-c}$, $\overline{a-d}\times\overline{b-d}\times\overline{c-d}$ muſt be affected with the Signs $+$, $-$, $+$, $-$ reſpectively; the firſt being the Product of three poſitive Quantities, the ſecond the Product of one negative and two poſitives, the third the Product of two negatives and one poſitive, and the fourth the Product of three negatives. Therefore ſince by ſubſtituting a, b, c, d for e in the Quantity $4e^3-3Ae^2+2Be-C$, it becomes alternately a poſitive and a negative Quantity, it follows from the laſt *Lemma* that a, b, c, d muſt be the *Limits* of the Roots of the Æquation $4e^3-3Ae^2+2Be-C=0$, or of the Æquation $4x^3-3Ax^2+2Bx-C=0$.

Coroll.] It follows from this Theorem, that if a', b', and c' are the three Roots of the Æquation $4x^3-3Ax^2+2Bx-C=0$, they muſt be *Limits* betwixt a, b, c, d the Roots of the Biquadratick $x^4-Ax^3+Bx^2-Cx+D=0$: For if a, b, c, d are *Limits* of the Roots a', b', and c'; theſe Roots converſely muſt be *Limits* betwixt a, b, c and d.

THEOREM II.] *Multiply the Terms of any Biquadratick* $x^4-Ax^3+Bx^2-Cx+D=0$ *by any Arithmetical Series of Quantities* $l+4m$, $l+3m$, $l+2m$, $l+m$, l, *and the Roots of the Biquadratick* a, b, c, d *will be the* Limits *of the Roots of the Æquation that reſults from that Multiplication; that is, of the Æquation*

$$\begin{aligned}&lx^4-lAx^3+lBx^2-lCx+lD=0\\&+4mx^4-3mAx^3+2mBx^2-mCx.\end{aligned}$$

Suppoſe that ſubſtituting the Roots a, b, c, d of the biquadratick Æquation $x^4-Ax^3+Bx^2-Cx+D=0$ ſucceſſively, for x in $4x^3-3Ax^2+2Bx-C$, the Quantities that reſult are $-R$, $+S$, $-T$, $+Z$; while $x^4-Ax^3+Bx^2-Cx+D$ is in every Subſtitution equal to nothing; and it is manifeſt that the Quantity

$$\begin{aligned}&+lx^4-lAx^3+lBx^2-lCx+lD\\&+4mx^4-3mAx^3+2mBx^2-mCx\end{aligned}$$

will become (when a, b, c, d are ſubſtituted ſucceſſively in it for x) equal to $-mRx$, $+mSx$, $-mTx$, $+mZx$; where the Signs of theſe Quantities being alternately negative and poſitive, it follows that a, b, c, d muſt be *Limits* of that Æquation by *Lemma* XII.

Coroll.] Hence it follows, that a, b, c and d are *Limits* of the Roots of the cubick Æquation $Ax^3 - 2Bx^2 + 3Cx - 4D = 0$, and converſely, that the Roots of this Cubick are *Limits* of the Roots of the biquadratick Æquation $x^4 - Ax^3 + Bx^2 - Cx + D = 0$; for multiplying the Terms of this biquadratick Æquation by the Arithmetical Progreſſion 0, -1, -2, -3, -4, the Cubick $Ax^3 - 2Bx^2 + 3Cx - AD = 0$ ariſes.

THEOREM III.] *In general, the Roots of the Æquation* $x^n - Ax^{n-1} + Bx^{n-2} - Cx^{n-3}$ &c. $= 0$, *are the* Limits *of the Roots of the Æquation* $nx^{n-1} - \overline{n-1} \times Ax^{n-2} + \overline{n-2} \times Bx^{n-3}$ &c. $= 0$, *or of any Æquation that is deduced from it by multiplying its Terms by any Arithmetical Progreſſion* $l \mp d$, $l \mp 2d$, $l \mp 3d$ &c. *and converſely the Roots of this new Æquation will be the* Limits *of the Roots of the propoſed Æquation* $x^n - Ax^{n-1} + Bx^{n-2}$ &c. $= 0$.

This Theorem is demonſtrated from the 11*th* and 12*th Lemmata* in the ſame manner as the preceding Theorems were demonſtrated from the 10*th* and 12*th*. From theſe Theorems it is eaſy to infer all that is delivered by the Writers of Algebra on this Subject.

THEOREM IV.] *The Æquation* $x^n - Ax^{n-1} + Bx^{n-2} - Cx^{n-3}$ &c. $= 0$ *will have as many imaginary Roots as the Æquation* $nx^{n-1} - \overline{n-1} \times Ax^{n-2} - \overline{n-2} \times Bx^{n-3}$ &c. $= 0$, *or the Æquation* $Ax^{n-1} - 2Bx^{n-2} + 3Cx^{n-3}$ &c. $= 0$.

Suppoſe that any Root of the Æquation $nx^{n-1} - \overline{n-1} \times Ax^{n-2} + \overline{n-2} \times Bx^{n-3}$ &c. $= 0$, as p becomes imaginary, and the two Roots of the Æquation $x^n - Ax^{n-1} + Bx^{n-2}$ &c. $= 0$, which by *Theor.* III. ought to be its *Limits*, cannot both be real Quantities; for it is manifeſt from the Demonſtration of *Theor.* I. that if they are real Quantities, then being ſubſtituted for x in $nx^{n-1} - \overline{n-1} \times Ax^{n-2} + \overline{n-2} \times Bx^{n-3}$ &c. the Quantities that reſult muſt have contrary Signs, and conſequently the Root p, whereof they are *Limits*, muſt be a real Root; which is againſt the Suppoſition. The ſame is true of the Æquation $Ax^{n-1} - 2Bx^{n-2} + 3Cx^{n-3}$ &c. $= 0$, for the ſame Reaſon.

Coroll. 1.] The Biquadratick $x^4 - Ax^3 + Bx^2 - Cx + D = 0$, will have two imaginary Roots, if two Roots of the Æquation $4x^3 - 3Ax^2 + 2Bx - C = 0$ be imaginary; or if two Roots of the Æquation $Ax^3 - 2Bx^2 + 3Cx - 4D = 0$ be imaginary. But two Roots of the Æquation $4x^3 - 3Ax^2 + 2Bx - C = 0$ muſt be imaginary, when two Roots of the Quadratick $6x^2 - 3Ax + B = 0$, or of the Quadratick $3Ax^2 - 4Bx + 3C = 0$ are imaginary, becauſe the Roots of theſe quadratick

Æquations are the *Limits* of the Roots of that Cubick, by the third *Theorem*; and for the ſame reaſon two Roots of the cubick Æquation $Ax^3 - 2Bx^2 + 3Cx - 4D = 0$ muſt be imaginary, when the Roots of the Quadratick $3Ax^2 - 4Bx + 3C = 0$, or of the Quadratick $Bx^2 - 3Cx + 6D = 0$ are impoſſible. Therefore two Roots of the Biquadratick $x^4 - Ax^3 + Bx^2 - Cx + D = 0$ muſt be imaginary when the Roots of any one of theſe three quadratick Æquations $6x^2 - 3Ax + B = 0$, $3Ax^2 - 4Bx + 3C = 0$, $Bx^2 - 3Cx + 6D = 0$ become imaginary; that is, when $\frac{3}{8}A^2$ is leſs than B, $\frac{4}{9}B^2$ leſs than AC, or $\frac{3}{8}C^2$ leſs than BD.

Coroll. 2.] By proceeding in the ſame manner, you may deduce from any Æquation $x^n - Ax^{n-1} + Bx^{n-2} - Cx^{n-3}$ &c. $= 0$, as many quadratick Æquations as there are Terms excepting the firſt and laſt whoſe Roots muſt be all real Quantities, if the propoſed Æquation has no imaginary Roots. The Quadratick deduced from the three firſt Terms $x^n - Ax^{n-1} + Bx^{n-2}$ will manifeſtly have this Form, $n \times \overline{n-1} \times \overline{n-2} \times \overline{n-3}$ &c. $\times x^2 - \overline{n-1} \times \overline{n-2} \times \overline{n-3} \times \overline{n-4}$ &c. $\times Ax + \overline{n-2} \times \overline{n-3} \times \overline{n-4} \times \overline{n-5}$ &c. $\times B = 0$, continuing the Factors in each till you have as many as there are Units in $n-2$. Then dividing the Æquation by all the Factors $n-2$, $n-3$ &c. which are found in each Coefficient, the Æquation will become $n \times \overline{n-1} \times x^2 - \overline{n-1} \times 2Ax + 2 \times 1 \times B = 0$, whoſe Roots will be imaginary by *Prop.* I. when $n \times \overline{n-1} \times 2 \times 4B$ exceeds $\overline{n-1}|^2 \times 4A^2$, or when B exceeds $\frac{n-1}{2n}A^2$, ſo that the propoſed Æquation muſt have ſome imaginary Roots when B exceeds $\frac{n-1}{2n}A^2$; as we demonſtrated after another Manner in the 5th Propoſition. The quadratick Æquation deduced in the ſame Manner from the three firſt Terms of the Æquation $Ax^{n-1} - 2Bx^{n-2} + 3Cx^{n-3}$ &c. $= 0$, will have this Form $\overline{n-1}\times\overline{n-2}\times\overline{n-3}$ &c. $\times Ax^2 - \overline{n-2}\times\overline{n-3}\times\overline{n-4}$ &c. $\times 2Bx + \overline{n-3}\times\overline{n-4}\times\overline{n-5}$ &c. $\times 3C = 0$; which by dividing by the Factors common to all the Terms, is reduced to $\overline{n-1}\times\overline{n-2}\times Ax^2 - \overline{n-2}\times 4Bx + 6C = 0$, whoſe Roots muſt be imaginary when $\frac{2}{3}\times\frac{n-2}{n-1}\times B^2$ is leſs than AC; and therefore in that Caſe ſome Roots of the propoſed Æquation muſt be imaginary.

Coroll. 3.] In general, let $Dx^{n-r+1} - Ex^{n-r} + Fx^{n-r-1}$ be any three Terms of the Æquation, $x^n - Ax^{n-1} + Bx^{n-2}$ &c. $= 0$, that immediately follow one another; multiply the Terms of this Æquation firſt by the Progreſſion n, $n-1$, $n-2$, &c. then by the Progreſſion $n-1$, $n-2$, $n-3$, &c. then by $n-2$, $n-3$, $n-4$, &c till you have multiplied by as many Progreſſions as there are Units in $n-r-1$: Then multiply the Terms of the Æquation that ariſes, as often by the Progreſſion 0, 1, 2, 3 &c. as there are Units in $r-1$, and you will at length arrive at a Quadratick of this Form,

$$\overline{n-r+1}\times\overline{n-r}\times\overline{n-r-1}\times\overline{n-r-2}\text{ \&c. }\times\overline{r-1}\times\overline{r-2}\times\overline{r-3}\times\overline{r-4}\text{ \&c. }\times Dx^2.$$

$$-\overline{n-r}\times\overline{n-r-1}\times\overline{n-r-2}\times\overline{n-r-3}\text{ \&c. }\times r\times\overline{r-1}\times\overline{r-2}\times\overline{r-3}\text{ \&c. }\times Ex.$$

$$+\overline{n-r-1}\times\overline{n-r-2}\times\overline{n-r-3}\times\overline{n-r-4}\text{ \&c. }\times\overline{r+1}\times r\times\overline{r-1}\times\overline{r-2}\text{ \&c. }\times F = 0.$$

And dividing by the Factors $n-r-1$, $n-r-2$, &c. and $r-1$, $r-2$ &c. which are found in each Coefficient, this Æquation will be reduced to $\overline{n-r+1}\times\overline{n-r}\times 2\times 1\times Dx^2 - \overline{n-r}\times 2\times r\times 2Ex + 2\times 1\times\overline{r+1}\times rF = 0$, whoſe Roots muſt be imaginary (by *Prop.* I.) when $\frac{n-r}{n-r+1}\times\frac{r}{r+1}\times E^2$ is leſs than DF. From which it is manifeſt that if you divide each Term of this Series of Fractions $\frac{n}{1}$, $\frac{n-1}{2}$, $\frac{n-2}{3}$, $\frac{n-3}{4}$, &c. $\frac{n-r+1}{r}$, $\frac{n-r}{r+1}$ by that which precedes it, and place the Quotients above the Terms of the Æquation $x^{n} - \mathrm{A}x^{n-1} + \mathrm{B}x^{n-2} - \mathrm{C}x^{n-3}$ &c. $=0$, beginning with the ſecond: Then if the Square of any Term multiplied by the Fraction over it be found leſs than the Product of the adjacent Terms, ſome of the Roots of that Æquation muſt be imaginary Quantities.

There remain many things that might be added on this Subject, but I am afraid you will think I have ſaid as much of it as it deſerves; and therefore I ſhall only add the Demonſtration of ſome Algebraick Rules and Theorems that are very eaſily deduced from the 11th *Lemma*.

I. The Rule for diſcovering when two or more Roots of an Æquation are equal, immediately follows from that *Lemma*. Suppoſe that two Roots of the Æquation $x^{n} - \mathrm{A}x^{n-1} + \mathrm{B}x^{n-2} - \mathrm{C}x^{n-3}$ &c. $=0$ are equal, and two Values of y (which is equal always to $x-e$) will be equal. Suppoſe that e is equal to one of thoſe two equal Values of x; and two Values of y will vaniſh, and conſequently y^{2} muſt enter each of the Terms of the Æquation of y; and therefore in this Caſe the firſt and ſecond Term of the Æquation of y in *Lemma* 11th muſt vaniſh, that is, $e^{n} - \mathrm{A}e^{n-1} + \mathrm{B}e^{n-2} - \mathrm{C}e^{n-3}$ &c. $=0$, and $ne^{n-1} - \overline{n-1} \times \mathrm{A}e^{n-2} + \overline{n-2} \times \mathrm{B}e^{n-3} - \overline{n-3} \times \mathrm{C}e^{n-4}$ &c. $=0$ at the ſame time; and conſequently theſe two Æquations muſt have one Root common, which muſt be one of thoſe Values of x that were ſuppoſed equal to each other. It is manifeſt therefore that when two Values of x are equal in the Æquation $x^{n} - \mathrm{A}x^{n-1} + \mathrm{B}x^{n-2}$ &c. $=0$, one of them muſt be a Root of the Æquation $nx^{n-1} - \overline{n-1} \times \mathrm{A}x^{n-2} + \overline{n-2} \times \mathrm{B}x^{n-3}$ &c. $=0$.

A Demonſtration of ſome *Algebraic Rules* and *Theorems*.

If three Values of x be ſuppoſed equal amongſt themſelves and to e, then three Values of y ($=x-e$) will vaniſh, and the firſt three Terms of the Æquation of y in *Lemma* XI. will vaniſh, and therefore $n \times \overline{n-1} \times e^{n-2} - \overline{n-1} \times \overline{n-2} \times \mathrm{A}e^{n-3} + \overline{n-2} \times \overline{n-3} \times \mathrm{B}e^{n-4}$ &c. $=0$; and one of the equal Values of x will be a Root of this laſt Æquation, and two of them will be Roots of the Æquation $nx^{n-1} - \overline{n-1} \times Ax^{n-2} + \overline{n-2} \times Bx^{n-3}$ &c. $= 0$. In general, it appears that if the Æquation $x^n - Ax^{n-1} + Bx^{n-2}$ &c. $= 0$ have as many Roots equal amongſt themſelves as there are Units in S, then ſhall as many of thoſe be Roots of the Æquation $nx^{n-1} - \overline{n-1} \times Ax^{n-2} + \overline{n-2} \times Bx^{n-3}$ &c. $= 0$ as there are Units in $S - 1$; as many of them ſhall be Roots of the Æquation $n \times \overline{n-1} \times x^{n-2} - \overline{n-1} \times \overline{n-2} \times Ax^{n-3} + \overline{n-2} \times \overline{n-3} \times Bx^{n-4}$ &c. $= 0$, as there are Units in $S - 2$; and ſo on.

II. The general Rule which Sir *Iſaac Newton* has given in the *Article de Limitibus Æquationum* for finding a *Limit* greater than any of the Values of x immediately follows from the 11th *Lemma*; for it is manifeſt that if e be ſuch a Quantity as ſubſtituted in all the Coefficients of the Æquation of y, *viz.* in $e^n - Ae^{n-1} + Be^{n-2}$ &c. $ne^{n-1} - \overline{n-1} \times Ae^{n-2} + \overline{n-2} \times Be^{n-3}$ &c. $n \times \frac{n-1}{2} \times e^{n-2} - \overline{n-1} \times \frac{n-2}{2} \times Ae^{n-3} + \overline{n-2} \times \frac{n-3}{2} \times Be^{n-4}$ &c. gives the Quantities that reſult all poſitive; then there being no Changes of the Signs of the Æquation of y in this caſe, all its Values muſt be negative; and ſince y is always equal to $x - e$, it follows that e muſt be a greater Quantity than any of the Values of x; that is, it muſt be a *Limit* greater than any of the Roots of the Æquation $x^n - Ax^{n-1} + Bx^{n-2}$ &c. $= 0$.

III. From this 11*th Lemma* ſome important *Theorems* in the Method of *Series*, and of *Fluxions*, and the Reſolution of Æquations are demonſtrated with great Facility; it is obvious that the Coefficient of the ſecond Term of the Æquation of y in that *Lemma* is the *Fluxion* of the firſt Term divided by the *Fluxion* of e; the Coefficient of the third Term is the ſecond *Fluxion* of that firſt Term divided by $2\dot{e}^2$; ſuppoſing e to flow uniformly. The third Term is the third *Fluxion* of the firſt Term divided by $2 \times 3\dot{e}^3$; and ſo on. Therefore ſuppoſing $e^n - Ae^{n-1} + Be^{n-2}$ &c, $= c$, the Æquation for determining y will be $c + \frac{\dot{c}}{\dot{e}}y + \frac{\ddot{c}}{1\times 2\,\dot{e}^2}y^2 + \frac{\dddot{c}}{1\times 2\times 3\,\dot{e}^3}y^3$ &c. $=0$; and hence, when e is near the true Value of x, Theorems may be deduced for approximating to y, and conſequently to x; which is ſuppoſed equal to $y+e$.

IV. Let A P $(=x)$ be the Abſciſs, and P M $(=z)$ the Ordinate of any Curve B L M; and ſuppoſe any other Abſciſs A K $=e$, and Ordinate K L $=c$, then ſhall z $(=\mathrm{PM})$ $= c \mp \frac{\dot{c}}{\dot{e}}y + \frac{\ddot{c}}{2\dot{e}^2}y^2 \mp \frac{\dddot{c}}{2\times 3\dot{e}^3}y^3 + \frac{\ddddot{c}}{2\times 3\times 4\dot{e}^4}y^4$ &c. Fig. 1.

For let z be ſuppoſed equal to any Series conſiſting of given Quantities, and the Powers of x, as to $Ax^n + Bx^r + Cx^s$ &c. and ſubſtituting $e \mp y$ for x, we ſhall find after the manner of the 11th *Lemma*,

$$z = Ae^n \mp nAe^{n-1}y + n\times\frac{n-1}{2}\times Ae^{n-2}y^2 \text{ \&c.}$$

$$+ Be^r \mp rBe^{r-1}y + r\times\frac{r-1}{2}\times Be^{r-2}\times y^2\text{\&c.}$$

$$+ Ce^s \mp sCe^{s-1}y + s\times\frac{s-1}{2}\times Ce^{s-2}\;y^2\text{\&c.}$$

&c. &c. &c.

But when $x=e$ then $z=c= Ae^n + Be^r + Ce^s$ &c. $\dot{c} = nAe^{n-1}\dot{e} + rBe^{r-1}\dot{e} + sCe^{s-1}\dot{e}$&c. $\ddot{c} = n\times\overline{n-1}\times Ae^{n-2}\dot{e}^2 + r\times\overline{r-1}\times Be^{r-2}\dot{e}^2 + s\times\overline{s-1}\times Ce^{s-2}\dot{e}^2$&c. and therefore

fore $z = c \mp \frac{\dot{c}}{\dot{e}}y + \frac{\ddot{c}}{2\dot{e}^2}y^2 \mp \frac{\dddot{c}}{2\times3\dot{e}^3}y^3$ &c. After the ſame manner you will find that $c = z \pm \frac{\dot{z}}{\dot{x}}y + \frac{\ddot{z}}{2\dot{x}^2}y^2 \pm \frac{\dddot{z}}{2\times3\dot{x}^3}y^3$, &c. for $c = Ae^n + Be^r + Ce^s$ &c. $= A\times\overline{x\pm y}|^n + B\times\overline{x\pm y}|^r + C\times\overline{x\pm y}|^s$ &c. $= z \pm \frac{\dot{z}}{\dot{x}}y + \frac{\ddot{z}}{2\dot{x}^2}y^2$ &c. The Area KLMP is equal to the Fluent of $z\dot{y}$ or of $c\dot{y}$; but

$$c\dot{y} = z\dot{y} \pm \frac{\dot{z}}{\dot{x}}y\dot{y} + \frac{\ddot{z}}{2\dot{x}^2}y^2\dot{y} \pm \frac{\dddot{z}}{2\times3\dot{x}^2}y^3\dot{y} \text{ \&c.}$$

$$\text{and } z\dot{y} = c\dot{y} \mp \frac{\dot{c}}{\dot{e}}y\dot{y} + \frac{\ddot{c}}{2\dot{e}^2}y^2\dot{y} \mp \frac{\dddot{c}}{2\times3\dot{e}^2}y^3\dot{y} \text{ \&c.}$$

And conſequently by finding the Fluents

$$\text{KLMP} = cy \mp \frac{\dot{c}}{2\dot{e}}y^2 + \frac{\ddot{c}}{2\times3\dot{e}^2}y^3 \mp \frac{\dddot{c}}{2\times3\times4\dot{e}^3}y^4 \text{ \&c.}$$

$$\text{or KLMP} = zy \pm \frac{\dot{z}}{2\dot{x}}y^2 + \frac{\ddot{z}}{2\times3\dot{x}^2}y^3 \pm \frac{\dddot{z}}{2\times3\times4\dot{x}^3}y^4 \text{ \&c.}$$

This laſt is the *Theorem* publiſhed by the learned Mr. *Bernouilli* in the *Acta Lipſiæ* 1694.

V. *Prop.* I.]

Of Algebraic Fractions and recurring Series, by Mr. Abr. de Moivre. N°. 373. p. 163.

V. *Prop.* I.] *Si ſit Fractio quælibet* $\frac{1}{1-ex+fxx-gx^3\ \&c.}$, *cujus Numerator ſit data Quantitas, & Denominator ſit Multinomium utcunque compoſitum ex datis,* 1, e, f, g, &c. *& indeterminata* x; *dico Fractionem ſupradictam ad Fractiones ſimpliciores reducibilem fore.*

Caſus 1.] Sit Fractio propoſita $\frac{1}{1-ex+fxx}$; finge Denominatorem $1-ex+fxx=0$, ſintque $\frac{1}{m}$, $\frac{1}{p}$, Radices iſtius Æquationis, ſive facto $xx-ex+f=0$, ſint m, p, Radices Æquationis novæ; fac $A=\frac{m}{m-p}$, atque $B=\frac{p}{p-m}$, & erit Fractio propoſita æqualis Summæ $\frac{A}{1-mx}+\frac{B}{1-px}$.

Caſus 2.] Sit Fractio propoſita $\frac{1}{1-ex+fxx-gx^3}$; fingatur $x^3-exx+fx-g=0$, ſintque m, p, q, Radices iſtius Æquationis; pone $A=\frac{mm}{\overline{m-p}\times\overline{m-q}}$, $B=\frac{pp}{\overline{p-m}\times\overline{p-q}}$, $C=\frac{qq}{\overline{q-m}\times\overline{q-p}}$; & erit Fractio propoſita æqualis Summæ $\frac{A}{1-mx}+\frac{B}{1-px}+\frac{C}{1-qx}$.

Caſus 3.] Sit Fractio propoſita $\frac{1}{1-ex+fxx-gx^3+hx^4}$; fingatur $x^4-ex^3+fxx-gx+h=0$, ſintque m, p, q, s, Radices iſtius Æquationis; pone $A = \frac{m^3}{\overline{m-p} \times \overline{m-q} \times \overline{m-s}}$, $B = \frac{p^3}{\overline{p-m} \times \overline{p-q} \times \overline{p-s}}$, $C = \frac{q^3}{\overline{q-m} \times \overline{q-p} \times \overline{q-s}}$, $D = \frac{s^3}{\overline{s-m} \times \overline{s-p} \times \overline{s-q}}$, eritque Fractio propoſita æqualis Summæ

$$\frac{A}{1-mx} + \frac{B}{1-px} + \frac{C}{1-qx} + \frac{D}{1-sx}.$$

Caſus 4.] Sit Fractio propoſita $\frac{1}{1-ex+fxx-gx^3+bx^4-kx^5}$; ſingatur $x^5-ex^4+fx^3-gxx+bx-k=o$, ſintque $m, p, q, s, t,$ Radices iſtius Æquationis; pone $A = \frac{m^4}{\overline{m-p} \times \overline{m-q} \times \overline{m-s} \times \overline{m-t}}$, $B = \frac{p^4}{\overline{p-m} \times \overline{p-q} \times \overline{p-s} \times \overline{p-t}}$, $C = \frac{q^4}{\overline{q-m} \times \overline{q-p} \times \overline{q-s} \times \overline{q-t}}$, $D = \frac{s^4}{\overline{s-m} \times \overline{s-p} \times \overline{s-q} \times \overline{s-t}}$, $E = \frac{t^4}{\overline{t-m} \times \overline{t-p} \times \overline{t-q} \times \overline{t-s}}$. Eritque Fractio propoſita æqualis Summæ, $\frac{A}{1-mx} + \frac{B}{1-mx} +$

$$\frac{C}{1-qx} + \frac{D}{1-sx} + \frac{E}{1-sx}.$$

Lex Reductionis illiuſque continuatio patent, ut inutile foret illas verbis explanare.

Coroll. 1.] Si Radices omnes ſint æquales, non poterit Fractio propoſita reduci ad ſimpliciores.

Coroll. 2.] Si Radices aliquæ ſint æquales, aliæ vero inæquales, poterit reduci Fractio propoſita ad ſimpliciores; ſit *v. g.* Fractio propoſita $\frac{1}{1-ex+fxx-gx^3}$, factoque ut præſcriptum eſt $x^3-exx+fx-g=0$. Sint Radices iſtius Æquationis m, p, q, quarum m & p ſint æquales: erunt Fractiones ſimplices in quas reſolvitur propoſita

$$\frac{mm}{\overline{m-p}\times\overline{m-q}\times\overline{1-mx}}+\frac{pp}{\overline{p-m}\times\overline{p-q}\times\overline{1-px}}+$$

$$\frac{qq}{\overline{q-m}\times\overline{q-p}\times\overline{1-qx}};$$ addantur duæ priores in unam Summam, & erit Summa (diviſis Numeratore & Denominatore per $m-p$)

$$\frac{mp-q\times\overline{m+p}+mpqx}{\overline{m-q}\times\overline{p-q}+\overline{1-mx}\times\overline{1-px}},\text{ ſive }\frac{mm-2qm+mmqx}{\overline{m-q}|^2\times\overline{1-mx}|^2},$$

ſive $\frac{m}{\overline{m-q}\times\overline{1-mx}|^2}-\frac{qm}{\overline{m-q}|^2\times\overline{1-mx}|^2}$; adeoque Fractiones reductæ erunt $\frac{m}{\overline{m-q}\times\overline{1-mx}|^2}-\frac{qm}{\overline{m-q}|^2\times\overline{1-mx}|^2}$

$+\frac{qq}{\overline{m-q}|^2\times\overline{1-qx}}$.

Coroll. 3.] Si Fractiones ſimplices, in quas reſolvitur Fractio propoſita, involvant Quantitates imaginarias, tunc quicquid eſt imaginarii ſemper deſtruetur per additionem duarum vel plurium Fractionum numero pari ſumptarum.

Coroll. 4.] Ex combinatione Fractionum ſimplicium, & apta limitatione Radicum, plurima ſuborientur Theoremata in quibus inerit concinnitas quædam minime aſpernenda. *Ex. g.* ſit Fractio propoſita $\frac{1}{1-ex+fxx-gx^3+bx^4}$, factoque ut antea $x^4-ex^3+fxx-gx+b=0$. Sint m, p, q, s, Radices Æquationis, ſintque

que Fractiones in quas reſolvitur propoſita, $\frac{A}{1-mx}+\frac{B}{1-px}+\frac{C}{1-qx}+\frac{D}{1-sx}$. Ponatur $q=-m$, atque $s=-p$; addantur ſimul duæ priores, itemque duæ poſteriores, & reducetur Fractio propoſita ad $\frac{m+p-mpx}{2\times\overline{m+p}\times\overline{1-mx}\times\overline{1-px}}+\frac{m+p+mpx}{2\times\overline{m+p}\times\overline{1+mx}\times\overline{1+px}}$; ſi vero ponatur $p=-m$, atque $s=-q$, & addantur duæ priores, itemque duæ poſteriores, reducetur Fractio propoſita ad

$$\frac{mm}{\overline{mm-qq}\times\overline{1-mmxx}}+\frac{qq}{\overline{qq-mm}\times\overline{1-qqxx}}.$$

Prop. II.] *Si ſit Fractio quælibet cujus Numerator ſit data Quantitas, & Denominator ſit Trinomium vel Quadrinomium vel Quinquinomium,* &c. *radicalitate non affectum, & utcunque compoſitum ex datis,* 1, e, f, g, h, &c. *& indeterminata* x, *atque dividatur Numerator per Denominatorem, ut habeatur Series Infinita; dico fore ut, ſi ſumantur Termini quilibet iſtius Seriei æqualibus intervallis a ſe invicem diſtantibus, Series infinitæ inde reſultantes, ſummabiles futuræ ſint.*

Exemp. 1.] Sit Fractio propoſita $\frac{1}{1-x-xx}$; reducatur illa ad Seriem infinitam, nempe ad $1+x+2xx+3x^3+5x^4+8x^5+13x^6+21x^7+34x^8$ &c. ſumanturque Termini omnes alterni, incipiendo a primo, itidemque ſumantur Termini omnes alterni, incipiendo a ſecundo, hincque conficiantur Series binæ,

Videlicet, $1+2xx+5x^4+13x^6+34x^8$ &c.
$x+3x^3+8x^5+21x^7+55x^9$ &c.

Fingatur Denominator Fractionis propoſitæ, $1-x-xx=0$, jam cum indices poteſtatum indeterminatæ x, in novis ſeriebus ſe invicem ſuperent communi differentia 2, pone $xx=z$, atque ope duarum æquationum $1-x-xx=0$, & $xx=z$, exterminetur x; fietque $1-3z+zz=0$; jam nunc reſtituatur x, & erit $1-3xx+x^4=0$; dividatur hæc æquatio per primam, quotiens erit $1+x-xx$; ſumantur alternatim Termini quotientis, propter Terminos alternatim ſumptos in ſerie propoſita, hincque orientur ſummæ duæ, $1-xx$, & x; conſtituantur hæ ſummæ Numeratores Fractionum duarum quarum communis Denominator ſit $1-3xx+x^4$. eruntque

que $\frac{1-xx}{1-3xx+x^4}$ & $\frac{x}{1-3xx+x^4}$ ſummæ reſpectivæ novarum Serierum.

Exemp. II.] Si vero deſiderentur ſummæ terminorum intervallis binis a ſe diſtantium, fiat ut prius $1-x-xx=0$, jam cum indices poteſtatum in novis ſeriebus ſe invicem ſuperent communi differentia 3, ponatur $x^3=z$, & fiet $1-4z-zz=0$, atque reſtituto x, fiet $1-4x^3-x^6=0$; dividatur $1-4x^3-x^6$ per $1-x-xx$, quotiens erit $1+x+2xx-x^3+x^4$, cujus termini ordinatim ſumpti ad intervalla bina, tres conficient ſummas, videlicet, $1-x^3$, $x+x^4$, $2xx$, quæ ſigillatim ſumptæ, erunt illæ Numeratores trium Fractionum, quibus ſi apponatur communis Denominator $1-4x^3-x^6$, erunt tres Fractiones, $\frac{1-x^3}{1-4x^3-x^6}$, $\frac{x+x^4}{1-4x^3-x^6}$, $\frac{2xx}{1-4x^3-x^6}$, ſummæ tres Terminorum omnium binis intervallis a ſe diſtantium, incipiendo reſpective a primo, ſecundo & tertio Termino; atque eadem methodo colligere licet ſummas terminorum ternis vel quaternis vel quinis intervallis a ſe diſtantibus, ſive denominator ſit quadrinomium, vel multinomium quodcunque ex terminis finitis compoſitum.

Prop. III.] *Si dividatur Unitas per Trinomium utcunque compoſitum ex datis* 1, e, f, g, &c. *& indeterminata* x; *dico Terminum quemvis Seriei ex hac diviſione reſultantis, aſſignabilem fore.*

Sit Trinomium $1-ex+fxx$; finge $xx-ex+f=0$, ſint m & p, radices Æquationis; ſit $l+1$ locus termini deſiderati, hoc eſt exprimat l intervallum inter primum Terminum & Terminum quæſitum.

Fac $A=\frac{m}{m-p}$, $B=\frac{p}{p-m}$, et erit Terminus deſideratus $\overline{Am^l+Bp^l}\times x^l$.

Eodem modo ſi dividatur Unitas per Quadrinomium $1-ex+fxx-gx^3$; pone $x^3-exx+fx-g=0$, ſintque m, p, q, radices Æquationis, fac $A=\frac{mm}{\overline{m-p}\times\overline{m-q}}$, $B=\frac{pp}{\overline{p-m}\times\overline{p-q}}$, $C=\frac{qq}{\overline{q-m}\times\overline{q-p}}$. Et erit Terminus deſideratus $\overline{Am^l+Bp^l+Cq^l}\times x^l$, & lex eadem obtinet pro Multinomiis quibuſcunque.

Problema I.] A & B *quorum Dexteritates ſint in ratione data, videlicet ut* a *ad* b; *ea conditione ludant, ut quoties* A *ludum unum vicerit,* B *tradat ipſi nummum unum: quoties vero* B *vicerit,* A *tradat ipſi nummum unum: & non prius ludo deſiſtant, quam eorum alter nummos omnes alterius lucratus fuerit; quæritur quantum probabile futurum ſit ut certamen intra datum ludorum numerum x, vel expirante illo numero, finiatur.*

Caſus 1.] Sit *n* numerus nummorum quos uterque Colluſorum habeat; ſit etiam *n* numerus par, ponaturque *a* ad *b* habere rationem æqualitatis.

Fig. 2. Centro D, Intervallo DA = 1, deſcribatur Semicircumferentia AMZ, quæ dividatur in tot partes æquales quot ſunt unitates in *n*; tunc ex primo H, tertio K, quinto M, &c. & impari quoque diviſionis termino, demittantur ad diametrum perpendicula HB, KC, MD, OE, QF, &c. ponatur

$$Q = \frac{HB^{x+1}}{AB^{\frac{1}{2}x+1}} - \frac{CK^{x+1}}{AC^{\frac{1}{2}x+1}} + \frac{DM^{x+1}}{AD^{\frac{1}{2}x+1}} - \frac{EO^{x+1}}{AE^{\frac{1}{2}x+1}} + \frac{QF^{x+1}}{AF^{\frac{1}{2}x+1}},$$

&c. donec ſinus omnes exhauriantur: quo facto, erit probabilitas certaminis finiendi intra ludos non plures quam *x*, ad probabilitatem non finiendi, ut $2^{\frac{1}{2}x-1}n - Q$ ad Q, accurate.

Coroll. 1.] Si ſumatur pro Q Terminus primus $\frac{HB^{x+1}}{AB^{\frac{1}{2}x+1}}$ neglectis cæteris, habebitur approximatio ſufficiens niſi forte ſit *x* numerus valde exiguus.

Exemp.] Sit *n* numerus nummorum quos uterque Colluſorum habeat = 10. Sit etiam $x = 76$. Si ſumatur pro Q primus terminus & negligantur cæteri, invenietur probabilitas certaminis finiendi intra ludos non plures quam 76 ad probabilitatem non finiendi ut 50747 ad 49235; ſi vero ſumantur pro Q termini duo priores neglectis cæteris, invenietur ratio probabilitatum ut 50743 ad 49247.

Coroll. 2.] Invenire quotenis ludis, probabilitates certaminis finiendi & non finiendi erunt æquales.

Solutio.] Ponatur pro Q Terminus unicus $\frac{HB^{x+1}}{AB^{\frac{1}{2}x+1}}$, fiatque $2^{\frac{1}{2}x-1}n - Q = Q$. Et poſito *n* maximo numero, invenietur $x = 0.756\,nn$ proxime, aliquanto major quam $\frac{1}{4}nn$.

Caſus 2.] Sit *n* numerus impar, ponaturque *a* ad *b* habere rationem æqualitatis.

Centro G, intervallo G A deſcribatur ſemicircumferentia A M Z quæ dividatur in tot partes æquales, quot ſunt unitates in *n*; tunc ex primo H, tertio K, quinto M, & impari quoque diviſionis termino, demittantur ad diametrum perpendicula H B, K C, M D, O E, Q F, *&c.* ex diametri extremitate A, primo ſcilicet arcui contermina, ducantur ſubtenſæ A H, A K, A M, *&c.* ad quas e Centro G ducantur perpendicula Gα, Gβ, Gγ, Gδ, Gϵ, &c. ponatur *Fig.* 3.

$$Q = \frac{BH^x \times G\alpha}{AB^{\frac{x+1}{2}}} - \frac{CK^x \times G\beta}{AC^{\frac{x+1}{2}}} + \frac{DM^x \times G\gamma}{AD^{\frac{x+1}{2}}} - \frac{EO^x \times G\delta}{AE^{\frac{x+1}{2}}} + \frac{FQ^x \times G\epsilon}{AF^{\frac{x+1}{2}}} \text{ \&c. quo facto,}$$

erit probabilitas certaminis finiendi intra ludos non plures quam *x*, ad probabilitatem non finiendi, ut $2^{\frac{x-3}{2}} n - Q$ ad Q accurate.

Coroll. 1.] Si ſumatur pro Q terminus primus $\frac{HB^x \times G\alpha}{AB^{\frac{x+1}{2}}}$ neglectis cæteris, habebitur approximatio ſufficiens.

Exempl.] Sit *n* numerus nummorum quos uterque Colluſorum habeat $= 45$. Sit etiam $x = 1519$. Sumatur pro Q terminus primus neglectis cæteris, & invenietur probabilitas certaminis finiendi intra ludos non plures quam 1519 ad probabilitatem non finiendi, ut 49959 ad 50441, quæ proportio eſt vero proxima.

Coroll. 2.] Invenire quotenis ludis probabilitates certaminis finiendi & non finiendi erunt æquales.

Solutio.] Ponatur pro Q Terminus unicus $\frac{HB^x \times G\alpha}{AB^{\frac{x+1}{2}}}$, fiatque $2^{\frac{x-3}{2}} n - Q = Q$; & poſito *n* magno numero, invenietur $x = 0.756\, nn$ proxime, aliquanto major quam $\frac{1}{4} nn$; contra quam ſentiebat *Clar. Monmortius*.

Caſus 3.] Poſitis cæteris ut in primo caſu, ſit a ad b ratio inæqualitatis (*vid. Fig.* 2.) Pone $\frac{a^{n}+b^{n}}{\overline{a+b}|^{n}} = \mathrm{L}$, $\frac{\overline{a-b}|^{2}}{\overline{a+b}|^{2}} = d$, $\frac{ab}{\overline{a+b}|^{2}} = r$, Fac, 1, $2r :: \frac{\mathrm{HB}q}{\mathrm{AB}}$; $m :: \frac{\mathrm{CK}q}{\mathrm{AC}}$; $p :: \frac{\mathrm{MD}q}{\mathrm{AD}}$, $q :: \frac{\mathrm{OE}q}{\mathrm{AE}}$, $s :: \frac{\mathrm{QF}q}{\mathrm{AF}}$, t.

$$\text{Pone } \mathrm{Q} = \frac{\mathrm{HB}}{2r\mathrm{AB}+d}m^{\frac{1}{2}x} - \frac{\mathrm{CK}}{2r\mathrm{AC}+d}p^{\frac{1}{2}x} + \frac{\mathrm{MD}}{2r\mathrm{AD}+d}q^{\frac{1}{2}x}$$

&c. quo facto erit probabilitas ludi finiendi intra ludos non plures quam x, ad probabilitatem non finiendi, ut $nr^{\frac{1}{2}n-1} - 2\mathrm{LQ}$, ad $2\mathrm{LQ}$.

Coroll.] Si ſumatur pro Q Terminus primus $\frac{\mathrm{HB}}{2r\mathrm{AB}+d}m^{\frac{1}{2}x}$ neglectis cæteris, habebitur approximatio ſufficiens.

Caſus 4.] Poſitis cæteris ut in ſecundo caſu, ſit a ad b ratio inæqualitatis (*vid. Fig.* 3.)

Pone quantitates L, d, r, m, p, q, s, t, &c. ut in tertio caſu.

$$\text{Pone } \mathrm{Q} = \frac{\mathrm{BH} \times \mathrm{G}\alpha}{2r\mathrm{AB}+d}m^{\frac{x-1}{2}} - \frac{\mathrm{CK} \times \mathrm{G}\beta}{2r\mathrm{AC}+d}p^{\frac{x-1}{2}} + \frac{\mathrm{DM} \times \mathrm{G}\gamma}{2r\mathrm{AD}+d}q^{\frac{x-1}{2}}$$

&c. quo facto erit probabilitas ludi finiendi intra ludos non plures quam x, ad probabilitatem non finiendi, ut $nr^{\frac{n-3}{2}} - 4\mathrm{LQ}$, ad $4\mathrm{LQ}$.

Coroll.] Si ſumatur pro Q Terminus unicus $\frac{\mathrm{BH} \times \mathrm{G}\alpha}{2r\mathrm{AB}+d}m^{\frac{x-1}{2}}$ neglectis cæteris habebitur approximatio ſufficiens.

Quemadmodum in Progreſſione Geometricâ, Terminus quilibet ad proxime præcedentem habet rationem datam, ita ſunt aliæ Progreſſiones quæ ſic conſtitui poſſunt ut aſſumptis ad libitum Terminis duobus primis, Ter-

minus quilibet ſubſequens ad duos proxime præcedentes habeat rationes datas; hujuſmodi eſt ſubjecta Series,

$$\begin{array}{llllll} A & B & C & D & E & F \\ 1 + & 3x + & 7xx + & 17x^3 + & 41x^4 + & 99x^5 \end{array}$$ &c. in qua

$C = 2Bx + 1Axx$

$D = 2Cx + 1Bxx$

$E = 2Dx + 1Cxx$

$F = 2Ex + 1Dxx$ &c.

Definitio] *Quantitates autem Numerales* $2 + 1$ *ſimul ſumptas ſubque propriis ſignis connexas appellare licet* Indicem Relationis.

Eodem modo conſtitui poſſunt ſeries aliæ in quibus aſſumptis ad libitum Terminis tribus primis, Terminus quilibet ſubſequens ad tres proxime præcedentes habeat rationes datas; hujus generis eſt ſubjecta Series.

$$\begin{array}{llllll} A & B & C & D & E & F \\ 1 + & 2x + & 3xx + & 10x^3 + & 34x^4 + & 97x^5 \end{array}$$ &c. in qua

$D = 3Cx - 2Bxx + 5Ax^3$

$E = 3Dx - 2Cxx + 5Bx^3$

$F = 3Ex - 2Dxx + 5Cx^3$ &c.

Quantitates autem Numerales $3 - 2 + 5$ *ſimul ſumptæ ſubque propriis ſignis connexæ, componunt* Indicem Relationis.

Sunt aliæ ſeries in quibus Relatio fit ad quatuor, vel ad quinque, vel ad ſex Terminos præcedentes, &c.

Definitio.] *Series* autem omnes hujus generis *recurrentes* appellare licebit, propter *Relationem* Terminorum perpetuo *recurrentem.*

Problema II.] *In ſeriebus recurrentibus, ex datis Terminis duobus primis, ſi relatio fiat ad duos præcedentes; vel datis Terminis tribus primis fiat ad tres præcedentes,* &c. *dato etiam indice relationis, invenire ſummam Terminorum quotlibet quorum numerus datus ſit.*

Problema ſolvitur in Tractatu noſtro qui inſcribitur, *The Doctrine of Chances.*

Problema III.] *Aſſumptis ad libitum ſeriebus quotcunque recurrentibus; Terminiſque, iiſdem intervallis a principio ſerierum diſtantibus, in ſe invicem multiplicatis, invenire ſummam ſeriei ex hac multiplicatione reſultantis.*

Inveſtigatio.] I° Proponantur ſeries duæ, ſitque $m + n$ Index Relationis in prima ſerie, atque $p + q$ Index Relationis in ſecunda, ex primo Indice $m + n$, formetur Æquatio $xx - mx - n = 0$, ex ſecundo Indice $p + q$, formetur Æquatio $yy - py - q = 0$, pone

$xy = z$. Atque ope trium iſtarum Æquationum, expungantur x & y, & orietur Æquatio

$$z^4 - mpz^3 \begin{array}{l} -mmqzz \\ -npp \\ -2nq \end{array} - mnpqz + nnqq = 0.$$

in qua deleto primo termino z^4, mutatis ſignis omnibus, atque poſito $z = 1$, obtinebitur Index Relationis, quo obtento, ſeries reſultans facile ſummabitur.

II° Eodem modo procedere licet, ſi dentur ſeries tres vel quatuor &c. recurrentes.

A Demonſtration of the 11th Propoſition of Sir Iſaac Newton's *Quadratures by Mr.* B. Robins. N°. 397. *p.* 230. *Fig.* 4.

VI. This Propoſition conſiſts of two Parts: The firſt is as follows.

Firſt PART.] Let there be any Curve A D I, whoſe Abſciſſe A B ſhall be denoted by z, and its Ordinate B D by y; which may be related in any manner to the Abſciſſe. And calling this the firſt Curve, let other Curves A E K, A F L, A G M, A H N, *&c.* be formed to the common Abſciſſe A B, or z, by making the Ordinate B E of the ſecond Curve always equal to the Area A B D of the firſt divided by Unity; the Ordinate B F of the third equal to the Area A B E of the ſecond divided by Unity; the Ordinate B G of the fourth equal to the Area A B F of the third divided by Unity; and ſo on continually.

Suppoſe now, that other Curves A O S, A P T, A Q V, A R W, be deſcribed to the ſame common Abſciſſe A B or z; in which Curves the Ordinate B O of the Curve A O S ſhall be equal to zy, the Ordinate B P of the Curve A P T equal $z^2 y$, the Ordinate B Q of the Curve A Q V equal to $z^3 y$, the Ordinate B R of the Curve A R W equal to $z\ \ y$, *&c.* And let the whole Area A C I be denoted by A, the Area A C S by B, the Area A C T by C, the Area A C V by D, the Area A C W by E, *&c.* Then the Series of Curves A D I, A E K, A F L, A G M, A H N are thus meaſured:

The Area of the firſt Curve A D I is $= \mathrm{A}$

—— of the ſecond A E K is $= z\mathrm{A} - \mathrm{B}$

—— of the third A F L $= \dfrac{zz\mathrm{A} - 2z\mathrm{B} + \mathrm{C}}{2}$

—— of the fourth A G M $= \dfrac{z^3\mathrm{A} - 3z^2\mathrm{B} + 3z\mathrm{C} + \mathrm{D}}{6}$

—— of the fifth A H N $= \dfrac{z^4\mathrm{A} - 4z^3\mathrm{B} + 6z^2\mathrm{C} - 4z\mathrm{D} + \mathrm{E}}{24}$

and ſo on perpetually.

Here in all the Curves following the firſt, the Index of the higheſt Power of z is always the Number which expreſſes the Diſtance of the Curve from the firſt, and afterwards decreaſes regularly by Unity; the firſt Term is multiplied into A, the ſecond into B, the third into C, the fourth into D, and ſo on; the Coefficients are the ſame as in a Binomial raiſed to the higheſt Power of z, and the Diviſor is ſo many Terms of this Progreſſion $1 \times 2 \times 3 \times 4 \times 5 \times 6$ *&c.* as is expreſs'd by a Number equal to the higheſt Index of z.

Otherwiſe ſuppoſing x to repreſent the Diſtance of the Curve to be meaſured from the firſt; then the Area ſought will be found by extending $\overline{z-1}|^{x}$ into a Series, and multiplying the firſt Term by A, the ſecond by B, the third by C, the fourth by D, *&c.* and dividing the whole by $n \times \overline{n-1} \times \overline{n-2}$ *&c.* continued to Unity.

Second PART.] Suppoſing the firſt, ſecond, third, *&c.* Curves to be the ſame as before: Let t denote the whole Abſciſſe AC, and put x for BC: then deſcribe the Curves CXA, CYA, CZA, CΓA, where BX ſhall be equal to xy, $BY = x^2 y$, $BZ = x^3 y$, $B\Gamma = x^4 y$, &c. This being done, and in the Series of Curves CIDA, CXA, CYA, CZA, CΓA, *&c.* the firſt Area CIDA being put equal to P, the ſecond CXA equal to Q, the third $CYA = R$, the fourth $CZA = S$, the fifth $C\Gamma A = T$, *&c.* the whole Areas of the aforeſaid Series of Curves are alſo determin'd as follows.

The firſt $AIC = P$
The ſecond $AKC = Q$
The third $ALC = \frac{1}{2} R$
The fourth $AMC = \frac{1}{6} S$
The fifth $ANC = \frac{1}{24} T$.

Here the Areas P, Q, R, S, T are divided by Numbers produced by multiplying as many Terms of this Series $1 \times 2 \times 3 \times 4 \times 5$ *&c.* together, as in the former Caſe.

Demonſtration of the 1ſt Part.] Let the Area ABD be denoted by a, the Area ABO by b, ABP by c, ABQ by d, and ABK by e. Then it is evident, that

The Fluxion of the Area ABD is $= \dot{z} \times BD = \dot{z} y = \dot{a}$

The Fluxion of the Area ABO is $= \dot{z} \times BO = \dot{z} z y = \dot{b}$

The Fluxion of the Area ABP is $= \dot{z} \times BP = \dot{z} z^2 y = \dot{e}$
&c. &c.

Hence

$$z \times \dot a \text{ is } (= \dot z z y) = \dot b$$

$$z^2 \times \dot a \text{ is } (= \dot z z^2 y) = z \dot b = \dot c$$

$$z^3 \times \dot a \text{ is } (= \dot z z^3 y) = z^2 \dot b = z \dot c = \dot d$$

Or generally

$$z^n \times \dot a = z^{n-1} \times \dot b = z^{n-2} \times \dot c = z^{n-3} \times \dot d, \text{ \&c.}$$

Now as $\dot z \times ABD$ or $\dot z \times a$ is $=$ Fluxion of A B E, if you add, to the firſt Part, $z \times \dot a\ (= \dot z z y)$ and its equal $\dot b$, to the other Part, it follows, that

$$\left.\begin{array}{r} \dot z \times a \\ + z \times \dot a \end{array}\right\} = \text{Fluxion of } ABE + \dot b$$

And taking the Fluents
$z \times a = ABE + b$, or $ABE = z \times a - b$; and when z or A B becomes $=$ A C, then A B E becomes A C K, and a and b become A and B; therefore A C K is $= z \times A - B$.

Again, The Ordinate B F of the next Curve is equal to A B E, which has been proved equal to $z \times a - b$. Conſequently the Fluxion of A B F is $= \dot z z a - \dot z b$; and if you add to the firſt Part of this Equation, $\frac{1}{2} z^2 \times \dot a - z \dot b$ $(= \frac{1}{2} z^2 \dot z y - \dot z z^2 y = - \frac{1}{2} \dot z z^2 y)$ and its Equal $- \frac{1}{2} \dot c$ to the other, it follows, that

$$\left.\begin{array}{r} \dot z z a - \dot z b \\ + \frac{1}{2} z z \dot a - z \dot b \end{array}\right\} = \text{Fluxion of } ABF - \tfrac{1}{2} \dot c$$

And taking the Fluents
$\frac{1}{2} z^2 a - z b = ABF - \frac{1}{2} c$; or by tranſpoſing

$ABF = \dfrac{z^2 a - 2 z b + c}{2}$; or ſuppoſing z equal to A C,

$$ACL = \frac{z^2 A - 2zB + C}{2}.$$

The Ordinate BG is equal to ABF, which has been proved equal to $\frac{z^2 a - 2zb + c}{2}$: Therefore the Fluxion of ABG is equal to $\frac{\dot{z}z^2 a - 2\dot{z}zb + \dot{z}c}{2}$. And adding $\frac{1}{6}z^3\dot{a} - \frac{1}{2}z^2\dot{b} + \frac{1}{2}z\dot{c}$ $(= \frac{1}{6}\dot{z}z^3 y - \frac{1}{2}\dot{z}z^3 y + \frac{1}{2}\dot{z}z^3 y = \frac{1}{6}\dot{z}z^3 y)$ on one ſide, and its Equal $\frac{1}{6}\dot{d}$ on the other, it will be

$$\left.\begin{array}{l}\frac{1}{2}\dot{z}z^2 a - \dot{z}zb + \frac{1}{2}\dot{z}c \\ + \frac{1}{6}z^3\dot{a} - \frac{1}{2}z^2\dot{b} + \frac{1}{2}z\dot{c}\end{array}\right\} = \text{Fluxion of } ABG + \tfrac{1}{6}\dot{d}$$

And taking the Fluents
$\frac{1}{6}z^3 a - \frac{1}{2}z^2 b + \frac{1}{2}zc = ABG + \frac{1}{6}d$; and tranſpoſing,

$ABG = \frac{z^3 a - 3z^2 b + 3zc - d}{6}$; or ſuppoſing z equal to AC;

then $ACM = \frac{z^3 A - 3z^2 B + 3zC - D}{6}$.

In the ſame Manner the Fluxion of ABH is equal to $\frac{\dot{z}z^3 a - 3\dot{z}z^2 b + 3\dot{z}zc - \dot{z}d}{6}$; and adding on one ſide $\frac{1}{24}z^4\dot{a} - \frac{1}{6}z^3\dot{b} + \frac{1}{4}z^2\dot{c} - \frac{1}{6}z\dot{d}$, and its Equal $-\frac{1}{24}\dot{e}$ on the other, it becomes

$$\left.\begin{array}{l}\frac{1}{6}\dot{z}z^3 a - \frac{1}{6}\dot{z}z^2 b + \frac{1}{6}\dot{z}zc - \frac{1}{6}\dot{z}d \\ + \frac{1}{24}z^4\dot{a} - \frac{1}{6}z^3\dot{b} + \frac{1}{4}z^2\dot{c} - \frac{1}{6}z\dot{d}\end{array}\right\} \doteq \text{Fluxion of } ABH - \tfrac{1}{24}\dot{e}$$

And taking the Fluents

$\frac{1}{24}z^4 a - \frac{1}{6}z^3 b + \frac{1}{4}z^2 c - \frac{1}{6}z d = \mathrm{ABH} - \frac{1}{24}e$: therefore

$$\mathrm{ABH} = \frac{z^4 a - 4z^3 b + 6z^2 c - 4z d + e}{24};$$

Or $\mathrm{ACN} = z^4 A - 4z^3 B + 6z^2 C - 4z D + E$, ſuppoſing z equal to AC. In like manner you may proceed to meaſure any of theſe Curves: and you will always find their Value the ſame as is expreſſed in the Propoſition.

The 2d Part demonſtrated.] Suppoſe any Curve whoſe Diſtance from the firſt is denoted by n; then the Curve whoſe Abſciſſe is BC or x, and its Ordinate $x^n y$ divided by $n \times \overline{n-1} \times \overline{n-2} \times \overline{n-3}$ &c. continu'd to Unity will be equal to it, when x is equal to AC or t.

It is evident, that when the Areas ABD, ABO, ABP, ABQ, ABR, *&c.* decreaſe, the Areas BCID, BCSO, BCTP, BCVQ, BCWR increaſe reſpectively; and conſequently the Decrements of the Areas ABD, ABO, ABP, *&c.* or their Fluxions with a negative Sign, are the Increments or Fluxions of the Areas BCID, BCSO, BCTP, &c. that is, calling the Area BCID, α; the Area BCSO, β; the Area BCTP, γ; BCVQ, δ; BCWR, ϵ:

then $\dot\alpha = -\dot a$, $\dot\beta = -\dot b$, $\dot\gamma = -\dot c$, $\dot\delta = -\dot d$, $\dot\epsilon = -\dot e$.

Now the Fluxion of the Curve, whoſe Abſciſſe is $= x$, or BC, and its Ordinate $= x^n y$ is $\dot x\, x^n y$; that is, equal to $\dot x\, y \times \overline{t-z}|^n$; x being $= t - z$; or ſince the Increment of x, or $\dot x$ is equal to the Decrement of z, or $-\dot z$, the Fluxion of the ſame Curve is equal to $-\dot z\, y \times \overline{t-z}|^n = -\dot z\, y$ in

$$\overline{t^n - n \times t^{n-1} z + n \times \frac{n-1}{2} t^{n-2} \times z^2, \&c.}$$

$$= -t^n \dot z y + n t^{n-1} \dot z z y - n \times \frac{n-1}{2} t^{n-1} t^{n-2} \dot z z^2 y, \&c.$$

that is, $= t^n \times -\dot a - n t^{n-1} \times -\dot b + n \times \frac{n-1}{2} t^{n-2} \times -\dot c$, &c.

or $= t^n\dot{\alpha} - nt^{n-1}\beta + n\times\frac{n-1}{2}t^{n-2}\dot{\gamma}$, &c. and taking the Fluents, the Area of the Curve, whoſe Abſciſſe is x, or B C, and Ordinate $x^n y$, is equal to $t^n\alpha - nt^{n-1}\beta + n\times\frac{n-1}{2}t^{n-2}\gamma$, &c. But when x is equal to A C, then α, β, γ, &c. will be equal to A, B, C, &c. as is very evident; conſequently the Area of the Curve whoſe Abſciſſe is x, and Ordinate $x^n y$, when x is $=$ A C, is $t^n\text{A} - nt^{n-1}\text{B} + n\times\frac{n-1}{2}\times t^{n-2}\text{C}$ &c. that is, equal to $\overline{t-1}|^n$ thrown into a Series, and the firſt Term multiplied by A, the ſecond by B, the third by C, *&c.* But $\overline{t-1}|^n$ thrown into a Series, and the firſt Term multiplied by A, the ſecond by B, the third by C, *&c.* and then the whole divided by $n\times\overline{n-1}\times\overline{n-2}$, *&c.* continued to Unity, is equal to the Area of the Curve, whoſe Place in the Series is denoted by n: Therefore the Area of the Curve, whoſe Abſciſſe is equal to x, and its Ordinate to $x^n y$, taken when x is equal to A C, and divided by $n\times\overline{n-1}\times\overline{n-2}\times\overline{n-3}$, *&c.* continued to Unity, is equal to the Area of a Curve whoſe Place in the Series is denoted by n; that is, Q, which is the Area of a Curve, whoſe Abſciſſe is x, and Ordinate xy taken when x is $=$ A C, is equal to the ſecond Curve A K C; $\frac{1}{2}$ R, which is the Area of the Abſciſſe x, and Ordinate $x^2 y$, taken in the ſame manner, is equal to the third Curve A L C; $\frac{1}{6}$ S, which is a like Area to x and $x^3 y$, is equal to the fourth Curve A M C; $\frac{1}{24}$ T, the Area to x and $x^4 y$, x being equal to A C, is equal to the fifth Curve A N C; and ſo on perpetually Q. E. D.

Of the Section of an Angle by Mr. Abr. de Moivre. N°374. *p.* 228.

VII.] Ineunte Anno 1707, incidi in Methodum quâ, Æquatione datâ hujus formæ.

$$ny + \frac{nn-1}{2\times3}\text{A}y^3 + \frac{nn-9}{4\times5}\text{B}y^5 + \frac{nn-25}{6\times7}\text{C}y^7 \text{ \&c.} = a,$$

Vel iſtius,

$$ny+\frac{nn-1}{2\times3}Ay^3+\frac{nn-9}{4\times5}By^5+\frac{nn-25}{6\times7}Cy^7\ \&c.=a;$$

Vel iſtius,

$$ny+\frac{1-nn}{2\times3}Ay^3+\frac{9-nn}{4\times5}By^5+\frac{25-nn}{6\times7}Cy^7\ \&c.^{*}=a;$$

ubi quantitates A, B, C, &c. repræſentant Coefficientes Terminorum præcedentium, Radices determinavi ad hunc modum.

Poſito $a+\sqrt{\overline{aa+1}}=v$ in primo caſu.

$a+\sqrt{\overline{aa-1}}=v$ in ſecundo.

Erit $y=\frac{1}{2}\sqrt[n]{v}-\frac{\frac{1}{2}}{\sqrt[n]{v}}$ in primo caſu.

$y=\frac{1}{2}\sqrt[n]{v}+\frac{\frac{1}{2}}{\sqrt[n]{v}}$ in ſecundo.

Solutiones autem iſtæ inſertæ fuerunt in Philoſ. Tranſ. Num. 309, pro *Jan. Feb. Mart.* ejuſdem anni.

Jam quibus perſpectum erit quo artificio Formulæ iſtæ inventæ fuerint, his procul dubio patebit aditus ad demonſtrationem ſequentis Theorematis.

Sit x Sinus Verſus Arcus cujuſlibet.

t Sinus Verſus Arcus alterius.

1 Radius Circuli.

Sitque Arcus prior ad poſteriorem ut 1 ad n, Tunc, aſſumptis binis Æquationibus quas cognatas appellare licet,

$$1-2z^n+z^{2n}=-2z^nt$$
$$1-2z+zz=-2zx.$$

Expunctoque z, orietur Æquatio quâ Relatio inter x & t determinatur.

Coroll. 1.] Si Arcus poſterior ſit Semicircumferentia, Æquationes erunt

1 + z

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* *Vide* Auctoris doctiſſimi Miſcellanea Analytica, nuper in Lucem edita, à Pag. 13.

$$1 + z^n = 0$$
$$1 - 2z + zz = -2zx.$$

e quibus ſi expungatur z, orietur Æquatio quâ determinantur Sinus Verſi Arcuum qui ſint ad Semicircumferentiam, ſemel, ter, quinquies, &c. ſumptam, ut 1 ad n.

Coroll. 2.] Si Arcus poſterior ſit Circumferentia, Æquationes erunt

$$1 - z^n = 0$$
$$1 - 2z + zz = -2zx;$$

e quibus ſi expungatur z, orietur Æquatio quâ determinantur Sinus Verſi Arcuum qui ſint ad Circumferentiam, ſemel, bis, ter, quater, &c. ſumptam, ut 1 ad n.

Coroll. 3.] Si Arcus poſterior ſit 60 Graduum, Æquationes erunt

$$1 - z^n + z^{2n} = 0$$
$$1 - 2z + zz = -2zx;$$

e quibus ſi expungatur z, orietur Æquatio quâ determinantur Sinus Verſi Arcuum qui ſint ad Arcum 60 Graduum,

$$\text{per}\begin{Bmatrix}1, & 7, & 13, & 19, & 25 & \\ 5, & 11, & 17, & 23, & 29 & \end{Bmatrix}\text{\&c. multiplicatum}$$

ut 1 ad n.

Si Arcus poſterior ſit 120 Graduum, Æquationes erunt

$$1 + z^n + z^{2n} = 0$$
$$1 - 2z + zz = -2zx;$$

e quibus ſi expungatur z, orietur Æquatio quâ determinantur Sinus Verſi Arcuum qui ſint ad Arcum 120 Graduum,

$$\text{per}\begin{Bmatrix}1, & 4, & 7, & 10, & 13 \\ 2, & 5, & 8, & 11, & 14\end{Bmatrix}\text{\&c. multiplicatum}$$

ut 1 ad n.

VIII. *Def.* I.] *Flores Geometricos* generatim appello quaſlibet figuras curvâ quadam per aliquot foliorum, ſeſe ab uno centro expandentium, perimetrum recurrente circumſcriptas, quales exhibent Fig. 5, 6, 7, 8, 9; quos quidem flores, pro numero foliorum, bifolios, trifolios, tetrafolios, pentafolios, hexafolios, &c. licebit nuncupare.

A Collection of Geometrical Flowers; by Mr. Guido Grand. N°378 *p*.355.

Def. II.] Cùm porrò innumeris modis ejuſmodi flores generari poſſint, eam geneſim hic ſpeciatim conſideramus, quæ per ramos a centro floris prodeuntes, æquales verò ſinubus angulorum, iis angulis, quos cum data poſitione linea rami comprehendunt, in data aliqua ratione proportionalium, procedit: cujuſmodi curvas *Rhodoneas* dudum appellavimus, eamque proportionem *Rhodoneæ* cuilibet propriam dicimus.

Def. III.] *Rhodoneam ſimplicem* appellamus, quæ una circulatione perficitur, *duplicem* quæ duplici, *triplicem* quæ triplici, & ſic deinceps pro numero circulationum.

Fig. 9, 10. Itaque ad Rhodonearum deſcriptionem aſſumptò quolibet circulo, cujus centrum C, & ducto ubilibet radio C D ad radium poſitione datum C A utcunque inclinato, ſit angulus A C D ad angulum A C G (ſive arcus A D ad arcum A G) in data ratione *a* ad *b*, ductoque ſinu G H, fiat C I æqualis G H; erit punctum I ad Rhodoneam ſupra definitam.

Ejuſmodi Rhodonearum proprietates præcipuas enucleabimus, nec non ſpatia, & perimetros breviter dimetiemur ſequentibus propoſitionibus.

Prop. I.] *Si fuerit arcus E A ad quadrantem A F (ſive angulus E C A ad rectum) ut a ad b, erit E C unus e maximis ramis Rhodoneæ,* *Fig.* 10, 11. *ſive erit E apex unius ex ejus foliis.*

Nam ex deſcriptione patet, ponendum eſſe ramum C E æqualem F C ſinui quadrantis A F, qui omnium ſinuum eſt maximus.

Prop. II.] *Quodlibet folium Rhodoneæ circa axem C E hinc inde æquali, uniformi, & ſimili expanſione ſpargitur.*

Factis enim hinc inde æqualibus angulis E C M, E C D, ob arcus æquales interceptos E M, E D, ſi fuerit arcus A M ad A N, ut A E ad A F, ut A D ad A G, nempe in data ratione *a* ad *b*, etiam reſidua E M, F N, itemque E D, F G in eadem ratione erunt, adeoque cùm antecedentia E M, E D æqualia ſint, etiam, conſequentia F N, F G invicem æquabuntur, uti & reſidua ad quadrantes N K, G A, quorum ſinubus cùm æquari debeant rami Rhodoneæ C L, C I, & ipſi æquales erunt; quare ab axe C E hinc inde æquali, & uniformi expanſione ſpargitur quodlibet folium Rhodoneæ. Quod erat, &c.

Coroll. 1.] Ob æquales arcus E M, E D fit A E medius Arithmeticus inter A M, A D, qui intercipiunt æquales ramos Rhodoneæ; ideoque horum ſumma illius duplum adæquat, ſive æquatur toti A E P arcui ſectoris circumſcribentis unum Rhodoneæ folium.

Coroll. 2.] Hinc etiam arcus M P æquatur A D.

Coroll. 3.] Et eorundem arcuum A M, A D ſumma ad ſemiperipheriam A N K eſt in data ratione *a* ad *b*, quam habet A E ad quadrantem A F.

Coroll. 4.] Et ſector A P C Rhodoneæ circumſcriptus, eſt ad ſemicirculum in eadem data ratione *a* ad *b*, quam habet arcus

A P, ſive ſumma duorum A M, A D ad ſemiperipheriam A N K.

Prop. III.] *Numerus foliorum, quibus integra Rhodonea ſimplex compingitur, eſt ad unitatem, ut* $2\,b$ *ad* a.

Tot enim folia emergunt ex hac deſcriptione, quot ſectores unicuique folio circumſcripti, intra circulum diſponi poſſunt; ſed quilibet ſector eſt ad ſemicirculum, ex *Coroll.* 3. *præced.* ut a ad b, adeoque ad circulum ut a ad $2\,b$, quare numerus foliorum in una circulatione eſt ad unitatem ut $2\,b$ ad a. Quod erat, &c.

Coroll. 1.] Hinc Rhodoneam ſimplicem deſcribere poſſumus, quæ datum foliorum numerum m, puta ſex, complectatur, ſi nempe pro ratione a ad b aſſumatur ratio 1 ad $\frac{m}{2}$ (in caſu propoſito 1 ad 3) quomodo erit $2\,b$ ad a, ut m ad 1 (in propoſito ut 6 ad 1) adeoque prodibit datus foliorum numerus m.

Coroll. 2] Sed & Rhodoneam duplicem, triplicem, quadruplicem, &c. eadem arte componemus, dato foliorum numero in ſe recurrentem, ſi nimirum pro Rhodonea duplici ſumatur ratio 1 ad $\frac{m}{4}$, exiſtente dato numero m impari, alias prodiret Rhodonea ſimplex ſubduplo foliorum numero, quæ in ſecunda circulatione ſibimet ſuperponeretur, per eadem foliorum veſtigia recurrens. Pro Rhodonea triplici ratio 1 ad $\frac{m}{6}$, dummodo numerus m non ſit per 3 diviſibilis, alias iterum ſimplex Rhodonea prodiret ſubtriplo foliorum numero contenta. Similiter pro quadruplici Rhodonea ratio 1 ad $\frac{m}{8}$ inſerviet, dummodo numerus m ſit impar, alias Rhodonea ſimplex, aut duplex, ut antea orietur; oportet enim in prima circulatione reſpectu Rhodoneæ duplicis haberi integrum aliquem foliorum numerum cum $\frac{1}{2}$ alterius folii, reſpectu triplicis cum $\frac{1}{3}$, vel $\frac{2}{3}$ folii, reſpectu quadruplicis cum $\frac{1}{4}$, vel $\frac{1}{4}$ alterius folii atque ita pariformiter in aliis.

Prop. IV.] *Si ratio* a *ad* b *non ſit numeris effabilis, ſed arcus* D A, G A *ſint incommenſurabiles, innumera folia ſibimet per infinitas circulationes advoluta circumponentur.*

Quælibet enim circulatio, præter certum foliorum integrorum numerum, partem folii ſuo toti incommenſurabilem comprehendet, nec unquam ad idem punctum deſcriptio revertetur, adeò ut æquatio ejuſmodi curvæ infinitorum ſit graduum. *Vid. Fig.* 8.

Prop. V.] *At ſi ratio a ad b fuerit dupla, prodibit Rhodonea unifolia.*

Nam ex *Prop.* 4. multitudo foliorum eſt ad unitatem ut $2b$ ad a; ſed in hoc caſu a eſt 2, & b eſt 1, quare multitudo foliorum eſt ad unitatem ut 2 ad 2, ſive ut 1 ad 1; adeoque nnmerus foliorum eſt unitas. Et ſane arcus E A, qui ſit ad quadrantem A F ut a ad b, nempe in ratione dupla, eſt ſemiperipheria, adeoque ſemicirculus eſt ſector A F E circumſcriptus ſemifolio, cujus axis E C ex *Prop.* I. ideoque integro folio circulus integer circumſcribitur.

Coroll. 1.] Facilis eſt hujuſmodi Rhodoneæ unifoliæ deſcriptio, ſi ſuper radio E C deſcribatur ſemicirculus, & ducta chorda E S D, in radio C D ponatur C I æqualis intervallo C S; nam cum C S ſit ſinus anguli C E S ad radium C E computatus, ejuſque anguli duplus ſit A C D, erit ramus C I ad Rhodoneam rationis duplæ, juxta geneſim præmiſſam.

Coroll. 2.] Unde etiam, ſi centro C, quolibet intervallo C S, in dicto ſemicirculo arcus P S deſcribatur, & tantundem extendatur in I, ut ſint arcus P S, S I æquales, erit punctum I ad Rhodoneam; quippe C S perpendicularis chordæ E D bifariam ſecat in præcedenti deſcriptione angulum E C D; cumque ſit C M æqualis C S, punctum I eſt in arcu circulari, centro C per I, & S tranſeunte, qui continuatus in P remanet bifariam ſectus in S.

Coroll. 3.] Et hinc patet, hanc Rhodoneam duplam eſſe circuli ſuper diametro E C deſcripti, ob quoſlibet arcus I S P duplos ipſorum S P, indeque dimidiam circumſcripti circuli, cujus diameter E A; id, quod conſonat infra generaliter demonſtrandis *Prop.* 8.

Prop. VI.] *Ubi ratio a ad b eſt æqualitatis, efficitur Rhodonea bifolia, quæ nihil aliud eſt, quàm duplex circulus ſubduplæ diametri ad diametrum circuli, qui Rhodoneæ circumſcribitur.*

Nam ratio $2b$ ad a erit ratio dupla, ergo ex *Prop. quarta* multitudo foliorum dupla erit unitatis: & ſane deſcripto circa radium
Fig. 12. F C, velut diametrum, ſemicirculo, quoniam ramus Rhodoneæ C I debet in hoc caſu æquari ſinui ipſiuſmet arcus A D, utique punctum I ad peripheriam dicti ſemicirculi pertinget, adeoque duplex circulus, circa radios F C, C V, velut diametros, deſcriptus, erit locus talium ramorum, id eſt, Rhodoneam ipſam bifoliam conſtituet.

Coroll. 1.] Etiam hic conſtat Rhodoneam bifoliam dimidiam eſſe circuli circumſcripti, atque adeo æqualem unifoliæ Rhodoneæ præcedentis propoſitionis.

Prop. VII.] *Quodlibet folium Rhodoneæ eſt ad quadrantem circularem ut a ad b.*

Ductis enim radiis infinitè proximis C I D, C *i d*, & ductis ſinu-
Fig. 13, 14. bus G H, *g h* correſpondentibus, nempe æquantibus ramos interceptos C I, C *i*, deſcriptoque concentrico arcu I R, patet fore elemen-

tum CIi ſemifolii Rhodoneæ ad elementum $GHbg$ quadrantis, ut $\frac{1}{2}$ arcus IR ad Hb, eo quod baſes Ci, gb trianguli elementaris CiI, & rectanguli elementaris $gbHG$ æquentur, ergo duplum ipſius CIi ad $GHbg$ eſt ut integra RI ad Hb, nempe in ratione compoſita ex RI ad Dd, & Dd ad Gg, & Gg ad Hb; ſed quia Gg ad Hb (ex theoria infinitè parvorum) eſt ut radius Cg ad ſinum gb, nempe ut CD ad CI, vel Dd ad RI, ratio Gg ad Hb elidit æqualem ſibi reciprocam RI ad Dd; quare ſupereſt, ut ratio RI ad Hb eadem ſit, quæ Dd ad Gg; ſed hæc eadem eſt quæ a ad b, cum in tali ratione ſint, tam AD ad AG, quam Ad ad Ag, adeoque & reſidua eandem rationem ſervent; ergo RI ad Hb, ſive duplum elementare ſpatium CIi ad elementum quadrantis $GHbg$, eſt in dicta ratione a ad b, & hoc ſemper; igitur duplum ſemifolii CIE, nempe integrum folium Rhodoneæ, eſt ad quadrantem, ut a ad b; Quod erat, *&c.*

Coroll. 1.] Hinc ſemifolium CIE ad quadrantem eſt ut $\frac{1}{2}$ a ad b, (ſive ut a ad 2 b.

Coroll. 2.] Item ſegmentum Rhodoneæ CIi ad ſemiſegmentum circuli Agb eſt in eadem ratione a ad 2 b.

Prop. VIII.] *Quodlibet folium Rhodoneæ medietas eſt ſectoris circularis ſibi circumſcripti, & integra Rhodonea ſimplex medietas circuli, duplex duorum, triplex trium circulorum*, &c.

Nam ex *præc.* quodlibet folium eſt ad quadrantem ut a ad b, ideoque ad ſemicirculum ut a ad 2 b; ſed ex *Coroll.* 4. *Prop.* 2. ſemicirculus ad ſectorem folio circumſcriptum eſt ut b ad a; ergo ex æquo perturbatè quodlibet folium eſt ad circumſcriptum ſectorem, ut b ad 2 b, ſcilicet in ratione ſubdupla; quare & omnia folia Rhodoneæ ad omnes circumſcriptos ſectores, id eſt Rhodonea ſimplex ad circulum, duplex ad duos circulos, triplex ad tres, &c. in eadem ſubdupla ratione erit.

Aliter. Numerus foliorum ex *Prop.* 3, eſt ad unitatem, ideoque Rhodonea ipſa ad unum folium (ſi eſt ſimplex) ut 2 b ad a; ſed folium eſt ad quadrantem circuli, *ex præc.* ut a ad b, ergo Rhodonea ſimplex eſt ad quadrantem circuli ut 2 b ad b, ſcilicet in ratione dupla; quare ſimplex Rhodonea æquatur ſemicirculo. Similis diſcurſus Rhodoneis duplicibus, & triplicibus applicari poteſt; nam in illis numerus foliorum eſt ad unitatem ut 4 b ad a, in his verò ut 6 b ad a, &c.

Coroll. 1.] Quælibet Rhodonea ſimplex cuilibet ſimplici Rhodoneæ eidem circulo inſcriptæ æqualis eſt, quocunque foliorum numero conſtet, ſemper enim æqualis eſt ſpatio ejuſdem ſemicirculi.

Coroll. 2.] Item quælibet Rhodonea duplex cuilibet duplici, & quælibet triplex cuivis triplici æqualis eſt, ob eandem rationem; quippe illa ſpecies eſt ſemper circulo æqualis, hæc ſeſquicirculo; & ſic de aliis. Oportet autem in duplici, aut triplici Rhodonea computare ſpatia foliorum, quæ ſibi ſuperponuntur, tanquam diſtincta eſſent.

Prop. IX.] *Bifariam ſecto angulo* E C A, *quem axis folii Rhodoneæ cum tangente* C A *continet, per rectam* C D, *& ramo* C I *deſcripto arcu circulari* I S T, *erit lunula* T E I *quadrabilis, nempe ad quadratum radii, ut* a *ad* $4\,b$.

Fig. 15. Cùm ſit enim quadrans F A ad A E, ut A G ad A D, qui eſt ipſius A E ſemiſſis, erit A G medietas quadrantis, ergo quadratum radii C G, vel C D, duplum eſt quadrati ſinus G H, ſive rami C I; ideoque ſector S C I ad ſectorem E C D ſimilem, ut 1 ad 2; ſector vero E C D ad F C G eſt ut a ad b; hæc enim eſt ratio arcuum E D, G F, ut eadem eſt integrorum E A, F A, & ablatorum A D, A G; ergo ex æquo ſector S C I ad ſectorem F C G erit ut a ad $2\,b$, nempe ut ſemifolium C I E ad quadrantem F G A C, vel ut ſegmentum Rhodoneæ C I ad ſegmentum A G H, vel ut reſiduum C E I C ad reſiduum F G H C, quare etiam reliquum ſemifolii S E I eſt ad reliquum triangulum C H G, aut tota lunula ad quadratum C H G P, in eadem ratione a ad $2\,b$, & ad quadratum radii C G, quod prædicti quadrati eſt duplum, erit ut a ad $4\,b$. Quod erat, &c.

Coroll. 1.] Cùm numerus foliorum Rhodoneæ ſimplicis ſit ad unitatem; adeoque etiam ſumma omnium lunularum, quas integra peripheria radio C T deſcripta abſcindit, ad unam lunulam T E I, ut $2\,b$ ad a; ipſa vero lunula ad quadratum radii ut a ad $4\,b$, patet eſſe ſummam dictarum lunularum ad quadratum radii ut $2\,b$ ad $4\,b$, nempe ſubduplam; hoc eſt ſummam talium lunularum æquare quadratum ipſum G H C P quadranti inſcriptum.

Coroll. 2.] Unde ſumma lunularum, ex una Rhodonea per dictam peripheriam abſciſſarum, æquatur ſummæ lunularum ex qualibet alia Rhodonea, quotcunque foliorum fuerit, eidem circulo inſcripta ſimiliter determinatarum.

Coroll. 3.] Cùm ejuſdem ſectoris E C A medietas ſit tam ſemifolium E I C, quàm ſector E C D, vel E D A, nec non ſector C S V, fiunt ſegmentum C I æquale trilineo E I D, & ſemilunula E S I trilineo C I V æqualis, quod propterea erit pariter quadrabile, utpote ad triangulum C G H in data ratione a ad $2\,b$.

Coroll. 4.] Et ſumma horum trilineorum in qualibet Rhodonea pariter ejuſdem erit quantitatis, utpote ſummæ lunularum ejuſdem, vel cujuſcunque alterius Rhodoneæ ſimplicis eidem circulo inſcriptæ ſemper æqualis.

Coroll. 5.] Adeoque ſi illa triangularia foliorum Rhodoneæ interſtitia pro foliis computentur, flos inde totidem foliorum perfectè quadrabilis exurget, ut in *Fig.* 16.

Fig. 16. Prop. X. *Ad quodlibet Rhodoneæ punctum I tangentem ducere.*

Fig. 17, 18. Factum jam ſit; ductaque ramo I C perpendicularis C M, conveniat cum tangente I M in M; & radio C I arcus I R infinitè parvus

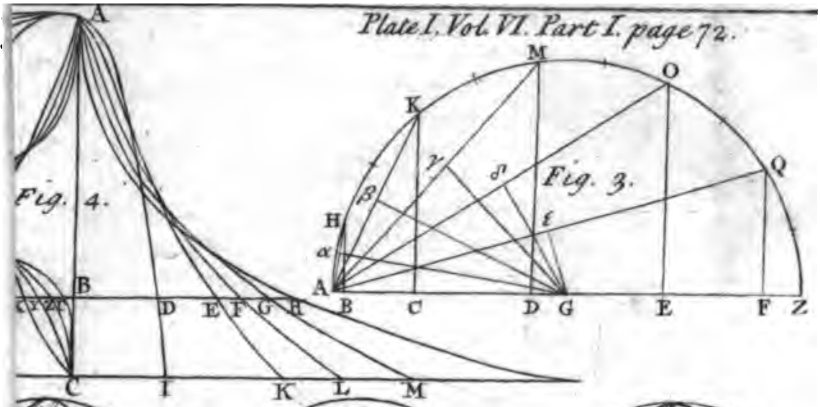


Fig. 4.

Fig. 3.

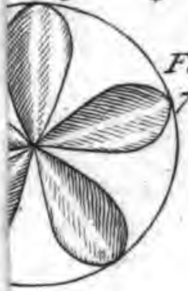


Fig 7

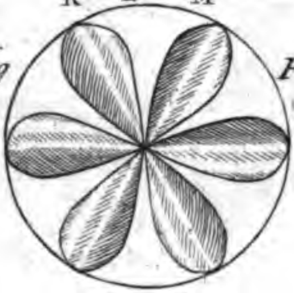
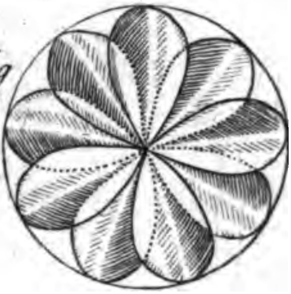


Fig 8



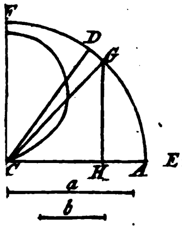


Fig. 11.

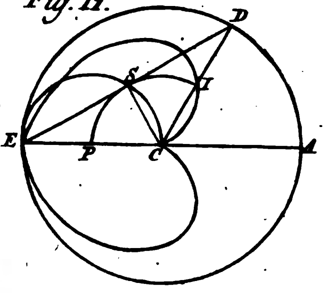


Fig. 12.

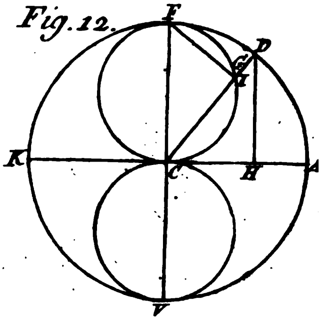
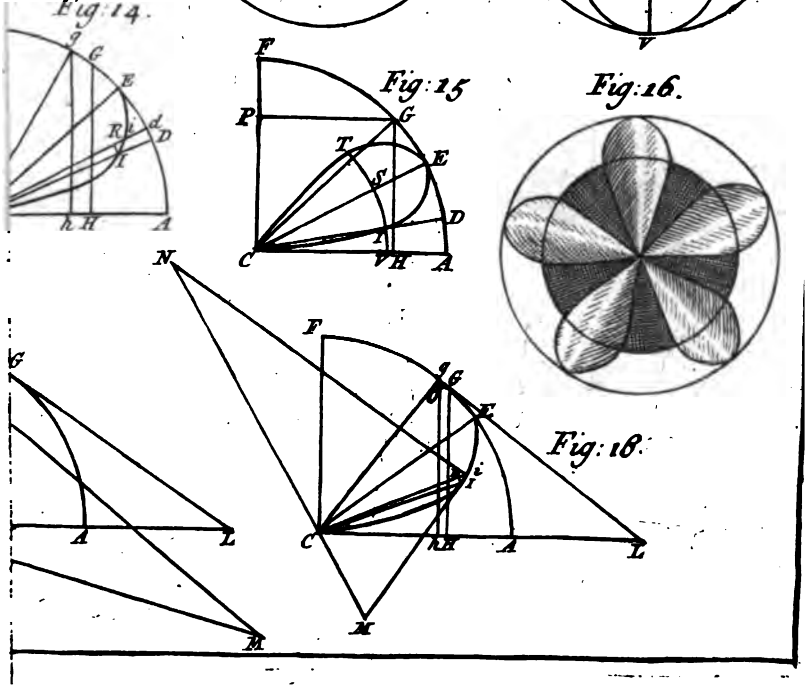


Fig: 14.

Fig: 15

Fig: 16.

Fig: 18.



vus deſcribatur uſque ad alium ramum C *i* infinitè proximum, ſintque ramis C I, C *i* æquales ſinus G H, *g b*, & circuli tangens G L occurrat diametro in L. Erit ergo I C ad C M ut *i* R ad R I, nempe in ratione compoſita ex *i* R, ſeu *g* O, ad O G (hoc eſt *g b*, vel *i* C, ad *b* L) & O G, ſive H *b*, ad R I (quæ ex *Prop.* 7. eſt eadem rationi *b* ad *a*) quare *i* C ad C M erit in ratione compoſita ex *i* C ad *b* L, & ex *b* ad *a*; ſed eadem ratio *i* C ad C M componitur quoque ex *i* C ad *b* L, & *b* L ad C M; ergo oportet rationem *b* L, ſive H L, ad C M eſſe datam, ſcilicet eam, quæ *b* ad *a*, ideoque ſi fiat, ut *b* ad *a*, ita ſubtangens circuli H L ad C M ramo C I perpendicularem, juncta M I erit tangens Rhodoneæ in puncto I; Quod erat faciendum.

Coroll. 1.] Si fiat *a* ad *b*, ita C H ad C N ramo perpendicularem, juncta N I erit curvæ Rhodoneæ normalis; nam quia H L ad C M eſt ut *b* ad *a*, & C H ad C N ut *a* ad *b*, erit H L ad C M ut reciprocè C N ad C H; & ideo rectangulum M C N æquabitur rectangulo L H C, id eſt, quadrato G H, vel quadrato rami C I; ergo juncta N I eſt tangenti M I, ſeu curvæ Rhodoneæ in puncto I, perpendicularis.

Coroll. 2.] Patet, tangentes angulorum C I M, & L G H, vel G C A ſemper eſſe in data ratione *a* ad *b*.

Prop. XI.] *Si fiat ut b ad a, ita radius* A C *ad* C Q, *& ſemi-axibus* F C, C Q *deſcribatur quadrans ellipſis* F V Q, *erit ejus perimeter æqualis perimetro ſemifolii Rhodoneæ* E C I, *& partes partibus correſpondentibus.* (Vid. Fig. 19, 20.)

Erit enim ubique etiam G P ad V P, vel *g p* ad *u p* in eadem ratione, quæ eſt A C ad C Q, id eſt, *b* ad *a*; quare & reſidua G O, V X in eadem ratione erunt. Quod ſi infinitè proximæ ſint P G, *p g*, G H, *g b*, & correſpondentes C I, C *i* cum arcu infinitè parvo I R, quoniam I R ad H *b*, vel G O ex *Prop.* 7. eſt ut *a* ad *b*, in qua etiam ratione erit V X ad eandem G O, patet ipſas IR, V X æquales fore; cum ergo & ſint æquales R *i*, V X (ob æqualitatem quarumvis C I, G H, vel T V, nec non C *i*, *g b*, *t u*) patet ſubtenſas quoque I *i*, V *u* æquales futuras. Singula igitur elementa, cum curvæ Rhodoneæ E I C, tum ellipticæ F V Q invicem æquantur; quare & perimeter ſemifolii Rhodoneæ erit quadranti curvæ ellipticæ æqualis, & duo quælibet folia perimetrum habebunt integræ curvæ ellipſeos æqualem; Quod erat, &c.

Coroll. 1.] Patet, Rhodoneam eſſe ellipſim quandam contractam; nam ſi confluentibus in centrum C punctis T, *t*, ordinatæ elliptici quadrantis V T, *u t*, in ramos abeant a centro C diductos, quadrans ellipſis in ſemifolium Rhodoneæ contrahetur, eâdem curvæ longitudine manente.

Coroll. 2.] Hinc iterum patet, Rhodoneam eſſe medietatem ſectoris circularis circumſcripti; eſt enim ſemifolium E I C medietas quadrantis elliptici F V Q C, in quem expanderetur, ſi rami ab eorum centro diſſoluti fierent paralleli, & rectæ C Q perpendiculares; cumque quadrans ellipſis ſit ad quadrantem circularem, ut baſis Q C ad baſim C A, nempe ut *a* ad *b*, in qua etiam ratione eſt ſector E C A ad eundem quadrantem, ex *Prop. prima*, patet, ejuſmodi ſectorem æquari quadranti elliptico, ideoque duplum eſſe inſcripti folii Rhodoneæ.

Coroll. 3.] Inſuper colligitur, æquales eſſe foliorum perimetros in Rhodoneis, quarum ratio ſit reciproca, & radii ſuorum circulorum in eadem reciproca ratione ſibi reſpondeant; nam ſi radius C F, vel E C *Fig.* 20. æquaretur baſi ellipſis C Q *Fig.* 21. & viciſſim radius C F iſtius æquaret baſim C Q ellipſis alterius *Figuræ*, patet, eandem ellipſim F V utrobique reſultare debere, quippe iiſdem ſemiaxibus deſcriptam, eamque fore utrivis folio iſoperimetram, exiſtente ibi ratione *a* ad *b*, hic reciprocè *b* ad *a*. Exempli cauſa, ſi ratio *a* ad *b* ſit ſubdupla, ut juxta *Prop.* 3. hinc proveniat Rhodonea tetrafolia, radio autem ſubduplo (adeoque æquali baſi quadrantis ellipſis iſoperimetræ) viciſſim fiat Rhodonea juxta rationem duplam, quæ ex *Prop.* 5. unifolia evadet, erit hæc iſoperimetra uni folio illius; nam baſis quadrantis elliptici huic reſpondens baſim habebit illius radio æqualem, adeoque eadem curva elliptica utrivis folio æqualis oſtenditur.

Coroll. 4.] Si verò in eodem circulo duæ Rhodoneæ deſcribantur, altera juxta rationem *a* ad *b*, altera juxta reciprocam *b* ad *a*, perimetros ſuorum foliorum habebunt ipſis rationum antecedentibus *a*, & *b* proportionales; nam ſi primæ Rhodoneæ tertia quædam Rhodonea ſimilis deſcriberetur in circulo, ad cujus radium prioris radius eſſet ut *a* ad *b*, eſſet perimeter primæ ad perimetrum tertiæ ſibi ſimilis in ipſa ratione radiorum *a* ad *b*. Verùm perimeter hujus tertiæ, ex *Coroll. præced.* æquaretur perimetro ſecundæ, utpotè reciproca ratione, & juxta reciprocos radios deſcriptæ; ergo perimeter primæ ad perimetrum ſecundæ eſt in eadem ratione *a* ad *b*.

Prop. XII.] *Rhodoneam datæ rationis a ad b minoris inæqualitatis ex conica ſuperficie ſecare.*

Fiat ut *a* ad *b*, ita radius baſis N B ad latus N C coni recti N C K, cujus baſis diametro N K ſit perpendicularis radius B F, *Fig.* 21. qui ſit ad B R ut *b* ad *a*, & circa diametros B R, B F deſcribantur ſemicirculi B L R, B S F, quos ſecet quilibet radius B G in punctis L, S, ſitque G H diametro N K perpendicularis. Si ſuper circulo B L R erecta ſuperficies cylindrica intelligatur ſecare conicam in communi ſectione C I E, erit hæc (in planum explicata) ipſamet Rhodonea propoſitæ rationis. Nam communes ſectiones cylindricæ illius ſuperficiei cum planis triangulorum C B G, C B F per axem coni C B tranſeuntium, erunt rectæ L I, R E ipſi axi parallelæ, ideoque tam C I ad B L, quàm C E ad B R erunt ut latus coni ad radium baſis, ſcilicet ut *b* ad *a ex conſtructione*, ſive ut F B ad B R, ſive S B ad B L; adeoque C E æquatur B F, & C I æquatur

tur B S, ſive ſinui G H. Explicata autem ſuperficie conica in planum ſectorem circularem ipſi æqualem, radio C N deſcriptum, ejus angulus planus N C G ſubtendetur eodem arcu N G, ſubtendente in baſi coni angulum N B G; adeoque ut B N ad N C, ſive ut *a* ad *b*, ita erit angulus N C G ad ipſum N B G, cujus ſinui G H, ut vidimus, æquatur ramus C I folii C I E, cujus maximus ramus C E æquat radium B F circuli baſis; quare folium ipſum ad Rhodoneam pertinet in data ratione *a* ad *b* deſcriptam; Quod erat, &c.

Coroll. 1.] Cùm ſit etiam C E ad E O, ut C F ad F B, ut *b* ad *a*, ut F B ad B R ſintque C E, F B æquales, itidem æquales erunt B R, E O, & ſemicirculus B L R quarta pars erit ſemicirculi A E P duplum diametrum habentis, ſive erit medietas quadrantis A E O; eſt verò (ex noſtra Appendice de Fornicibus conicis, quam *Vivianeis* ſubjunximus jam inde ab anno 1698) ſuperficies conica A D E C ad ſuam baſim A D E O, ut ſuperficies ſemifolii C I E ad ſuam ichnographiam B L R, nempe in eadem ratione lateris coni ad radium baſis; ergo cum A D E O dupla ſit B L R, & ſuperficies A D E C ipſius ſemifolii C I E dupla erit, ut aliunde ſupra demonſtravimus ſectorem folio circumſcriptum illius duplum eſſe.

Coroll. 2.] Cùm oſtenſum ſit eſſe angulum A C I ad N B G, uti & A C E ad N B F, in data ratione *a* ad *b*, patet etiam in eadem ratione eſſe angulum reliquum I C E ad reliquum S B F, exiſtente (ut probavimus) ramo C I æquali ipſi B S; *Fig.* 22. unde ſi ſemicirculi CSE, in arcus concentricos, centro C deſcriptos, reſoluti, arcus quilibet P S, *p s* dividantur ad puncta I, *i*, ut ſit ſemper P I ad P S, *pi* ad *ps* in data illa ratione *a* ad *b*, erunt puncta I, *i* ſic inventa ad curvam Rhodoneam. *Fig.* 22.

Coroll. 3.] Imo etſi ratio *a* ad *b* majoris ſit inæqualitatis, adhuc Rhodoneas ope ſemicirculi deſcribere licebit generalius quam in *Coroll.* 2. *Prop.* 5. ſi arcus P S, *p s* producantur ad puncta I, *i*, ut ſint P I ad P S, *pi* ad *ps* in data ratione *a* ad *b*. Facto enim arcu E A R ad quadrantem E A in eadem ratione, ductoque radio C R, fiet angulus R C E ad A C E, ut angulus I C E ad angulum S C E, adeoque & reliquus *i* C R ad reliquum *s* C A, cujus ſinus æquatur C *s*, ſive C *i*, in eadem ratione erit *a* ad *b*; ideoque puncta I, *i* ſunt ad Rhodoneam datæ rationis.

Coroll. 4.] Et ſi arcus illi P S, *p s* in ſemicirculo deſcripti, tum dividantur in ratione *a* ad *b*, tum augeantur in reciproca ratione *b* ad *a*, curvæ interioris longitudo ad longitudinem exterioris erit ut *a* ad *b*, per *Coroll.* 4. *Prop. præcedentis.*

Scholion.] Verùm hæc, pro inſtituto noſtro, circa hujuſmodi curvas delibaſſe ſufficiat: quanquam alia etiam Rhodonearum ſymptomata enucleare in promptu eſſet, uti & alias florum ſpecies diverſâ geneſi efformatas exhibere facilè foret. Unum hoc admonere non prætermittam, quod ex certâ quadam generali foliorum Rhodoneæ deſcriptione ſimpliciſſima ex circulo derivatâ, ſuſpicari quis non immeritò poſſet etiam prima naturalium foliorum ſtamina, quæ in floris, aut fruticis ſemine latent, non neceſſariò ſimilia eſſe foliis ipſis conſpicuis, & jam germinantibus, ſive adultis; ſicut enim ſi florum, & fruticum folia noſtras Rhodoneas reipſa imitarentur, poſſet quis concipere, illorum prima ſtamina ſeminibus cujuſlibet ſpeciei incluſa ſimpliciſſima circulari figurâ infinitè parvâ circumſcribi, ſed mox peculiari vi cujuſlibet ſingularis ſpeciei, dum germinant, ita determinari ſuccum nutritium, ut dum in longum eorum axis extenditur, per quaſdam undas, ſive gyros, ipſi origini ſui pedunculi, velut centro, circumpoſitos, expandatur, eoſque ſemper in determinata ratione, vel arctiores, vel ampliores, quàm ſi circularis primorum ſtaminum figura retinenda eſſet: quo poſito talis ſpecies foliorum Rhodoneæ, ac talis numerus, & forma exurgeret, qualem ratio illa determinaret. Ita etiamſi aliâ lege florum, & fruticum frondes natura moliatur, non neceſſe eſt earum figuram, uſque ad ipſa prima earundem ſtamina, ex quibus germinant, obſervari; ſed illa in quibuſlibet unius certæ, ac determinatæ figuræ eſſe poſſet, quæ tantum pro diverſa vi determinante in ipſis expanſionem ſucci nutritii, in ſingulis ſpeciebus varianda foret, juxta diverſam rationem, quæ per ipſorum ſtaminum fibras dirigeretur. Sed ne extra chorum ſaltemus, hæc Philoſophis innuiſſe ſufficiat.

Textus Pappi* *hoc modo Reſtituendus videtur.*

IX. *Two general Propoſitions of* Pappus *of* Alexandria *reſtor'd* by Mr. *Rob. Simſon.* N° 377. *p.* 330.

Prop. 1.] “ Si duæ rectæ lineæ in duas rectas lineas ſibi mutuo “ occurrentes vel inter ſe parallelas ducantur,] & dentur in unâ “ earum tria † puncta [vel duo, ſi recta in qua ſunt, parallela “ fuerit alicui ex tribus reliquis]: cætera vero puncta præter u- “ num ‖ tangant rectam poſitione datam, etiam hoc quoque tanget “ rectam poſitione datam.” Hoc autem de quatuor tantum rectis dicitur, quarum non plures quam duæ per idem punctum tranſeunt. In quolibet vero propoſito rectarum numero ignoratur, quamvis vera ſit, hujuſmodi Propoſitio.

Prop. 2.] “ Si quotcunque rectæ occurrant inter ſe, nec plures “ quam duæ per idem punctum; data vero ſint puncta omnia in “ earum unâ, unumquodque autem punctum in aliâ tangat rectam “ poſitione datam;” vel generalius ſic. “ Si quotcunque rectæ oc- “ currant inter ſe, neque ſint plures quam duæ per idem punctum, “ omnia vero puncta in earum unâ data ſint; reliquorum nume- “ rus erit numerus triangularis, cujus latus exhibet numerum punc- “ torum rectam poſitione datam tangentium; quarum interſectio-

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* *Videſis* Pappi præfationem lib. 7. Coll. Math. *Apollonii* de Sectione rationis lib. duobus a Clariſſ. *Halleio* præmiſſam, pag. VIII. & XXXIV.

† *Tria puncta*] interſectionum ſc.

‖ *Tangant rectam*] i. e. unum punctum tangat unam aliquam rectam poſitione datam, & aliud tangat aliam poſitione datam, &c.

“ num ſi nullæ tres exiſtant ad angulos trianguli ſpatii, [nullæ quatuor ad angulos quadrilateri, nullæ quinque, &c. i. e. univerſim, ſi nullæ harum interſectionum in orbem redeant] unaquæque interſectio reliqua tanget poſitione datam.

Propoſitio prima in decem dividitur caſus, monente ipſo Pappo, quorum ejus, in quo nullæ ex quatuor rectis ſunt inter ſe parallelæ, neque rectæ poſitione datæ per data puncta tranſeunt, demonſtrationem hic apponemus; hic enim Caſus inter omnes maximè eſt generalis, ejuſque demonſtratio ſecundæ propoſitionis demonſtrationi omnino eſt neceſſaria. *Sint igitur quatuor rectæ* A B, A D, B E, C E. *Et data ſint tria puncta interſectionum* A, B, C *in earum qualibet, reliquarum vero interſectionum* D, E, F, *una* D *tangat rectam* G K *poſitione datam, alia* E *tangat rectam* H K *poſitione datam; tanget etiam reliqua* F *rectam poſitione datam.* Ducatur per F recta M F parallela ad A B, quæ occurrat ipſis H K, K G, C E, in M, N, O. Quoniam igitur data eſt ratio H B ad B C dabitur eidem æqualis ratio M F ad F O, & quoniam datur ratio A C ad A G, dabitur eidem æqualis ratio F O ad F N; quare datur ratio M F ad F N, igitur ſi jungatur F K, quæ occurrat ipſi A B in L, dabitur ratio H L ad L G; & datur H G poſitione & magnitudine; quare punctum L datur, & datum eſt punctum K, igitur K L poſitione datur.

Sit igitur H L ad L G in ratione compoſita ex rationibus H B ad B C & A C ad A G, & jungatur K L, erit hæc recta quam tangit punctum F, hoc eſt, ſi ducatur quævis C E, poſitione datis occurrens in D, E, & jungantur A D, B E ſibi mutuo occurrentes in F; recta erit linea quæ per K, F, L tranſit. Nam per F ducatur M F parallela ipſi A B, & quoniam ratio M F ad F N compoſita eſt ex rationibus M F ad F O & F O ad F N, hoc eſt, ex rationibus H B ad B C & A C ad A G, ex quibus etiam componitur ratio H L ad L G; erit H L ad L G ut M F ad F N, & igitur H G ad M N, hoc eſt, H K ad M K ut H L ad M F: Quare recta eſt linea quæ per K, F, L tranſit, per 14. 1 aut 32. 6 Elem.

Explicatio 2. *Prop.*] Obſervandum hic eſt, Numerum interſectionum, quæ in una recta reperiuntur in quâcunque propoſita multitudine rectarum, quarum non plures quam duæ per idem punctum tranſeunt, & quarum nullæ ſunt inter ſe parallelæ, unitate minorem eſſe ipſo numero rectarum: Nam duæ in unico puncto ſe invicem ſecant, tertia vero ducta priores in duobus, quarta priores in tribus punctis ſecat, &c. Et igitur numerus interſectionum in tribus rectis eſt unitas binario aucta, i. e. ternarius; numerus eorundem in quatuor rectis eſt ternarius ternario auctus; in quinque vero rectis eſt ultimus præcedens ſeu ſenarius quaternario auctus, &c. in infinitum; Qui numeri, ut manifeſtum eſt, triangulares ſunt, quorum cujuſque latus eſt numerus interſectionum, quæ inveniuntur in unaquâlibet recta, i. e. numerus qui unitate minor eſt numero omnium rectarum. Igitur ſi ex hoc numero omnium interſectionum dematur numerus omnium punctorum datorum, qui idem eſt cum numero interſectionum in una quâvis recta; reliquus erit adhuc triangularis, cujus latus ſc. unitate deficit a latere prioris, quod exhibet numerum omnium punctorum, & proinde binario minus eſt numero rectarum propoſitarum. Et hic eſt numerus interſectionum quas tangere rectam poſitione datam *Pappus* in hac Propoſitione requirit, quarumque ſi nullæ tres ſint ad angulos trianguli; [nullæ quatuor ad angulos * quadrilateri & ita deinceps;] unamquamque interſectionem reliquam tangere rectam poſitione datam affirmat. *Quæ autem uncis incluſa ſunt, textui neceſſitate coacti adjecimus, nam ſine iis propoſitio vera non eſſet extra caſum quinque rectarum.*

Commode vero in duos caſus dividitur propoſitio; quos etiam aperte ſatis indicat *Pappus*, qui Hypotheſin caſus facilioris propoſitioni hujus generis univerſaliſſimæ ſimul & elegantiſſimæ præmittit.

Caſus 1.] *Si quotcunque rectæ occurrant inter ſe nec plures quàm duæ per idem punctum; data vero ſint puncta omnia in earum unâ, unumquodcunque autem punctum in alia tangat rectam poſitione datam; unaquæque interſectio reliqua tanget rectam poſitione datam.* Sint enim quotcunque rectæ, *ex. gr.* ſex A F, B G, C H, D K, E L, E A; & data ſint omnia puncta in earum unâ, ſc. A, B, C, D, E; omnia vero puncta in aliâ ſc. F, L, M, N, tangant rectam poſitione datam: unaquæque reliqua interſectio tanget poſitione datam.

Sumatur enim quævis ex reliquis *ex. gr.* O, & quoniam quatuor ſunt rectæ O L, O N, A N, A B, & data ſunt tria puncta in unâ earum ſc. A, B, E, reliqua vero præter unum O, viz. ipſa L, N tangunt rectam poſitione datam, tanget etiam O poſitione datam per *Prop.* I. Eodem modo idem de omnibus reliquis oſtendetur.

Caſus 2.] Cæteris manentibus jam non ſint omnia puncta rectam poſitione datam tangentia (quorumque numerus binario minor eſt numero rectarum propoſitarum) in eâdem recta, ſed nulla eorum in orbem redeant; oſtendendum eſt reliqua omnia tangere rectam poſitione datam.

Lemma 1.] *Si quotcunque rectæ inter ſe occurrant neque plures quam duæ per idem punctum, & ſumantur quævis rectarum, ſit vero numerus interſectionum, qui conficitur ſumendo duo puncta in unaquâque rectarum ſumptarum æqualis numero harum rectarum; puncta hæc in orbem redibunt.* Nam quoniam ſunt duo puncta in unaquâque recta, erunt ad minimum tria in duabus rectis, & quatuor in tribus & ita deinceps; ſemper ſc. erit numerus punctorum ad minimum unitate major numero rectarum niſi recta ultima tranſeat per punctum primum; i. e. niſi rectæ in orbem redeant, in quo ſolo caſu æqualis erit numerus punctorum numero rectarum.

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* Intelliguntur etiam hic figuræ quarum latera ſe mutuo decuſſant Diagonalium inſtar, æque ac cæteræ.

Lemma 2.] *Si quotcunque rectæ inter ſe occurrant neque plures quam duæ per idem punctum, ſumantur vero quælibet ipſarum interſectiones, quarum numerus numero omnium rectarum æqualis ſit; vel hæ interſectiones omnes, vel earum aliquæ, in orbem redibunt, ſeu invenientur ad angulos polygoni vel trianguli.*

Nam tres interſectiones trium rectarum ſunt ad angulos trianguli; ſi vero ſint quatuor rectæ, & ſumantur quatuor puncta, una harum neceſſario invenietur in unâquaque recta; quod ſi in unâ aliqua ex quatuor rectis unum tantum inveniatur punctum, tria reliqua erunt in tribus reliquis rectis, & igitur ad angulos trianguli: Si vero nulla fuerit recta, in qua unum duntaxat punctum invenitur, erunt duo in unâquaque ex quatuor rectis, & ſunt quatuor puncta, ergo per *Lem.* 1. ſunt ad angulos quadrilateri. Et manifeſtum eſt ſi fuerint quatuor rectæ, & ſumantur plura quam quatuor puncta, multo magis aliqua eorum in orbem redire.

Sint jam quinque rectæ, & ſumantur quinque interſectionum puncta, &, ſi fuerit aliqua ex rectis in qua nullum invenitur punctum ex hiſce quinque, erunt omnia quinque in quatuor reliquis rectis; Si vero fuerit aliqua recta in quâ unum duntaxat invenitur punctum, erunt reliqua quatuor puncta in reliquis quatuor rectis; igitur in utroque caſu puncta aliqua erunt ad angulos trianguli, vel quadrilateri, per præcedentem caſum: Si autem nulla fuerit recta in qua vel nullum vel unicum invenitur punctum, erunt duo in unâquaque ex quinque rectis, & ſunt quinque puncta, ergo per *Lem.* 1. ſunt ad angulos quinquelateri. Eodem prorſus ratiocinio oſtendetur in ſex rectis & ita in infinitum.

In *Fig.* 28. ſunt octo rectæ, & octo ſumuntur puncta, quorum quatuor in orbem redeunt.

Demonſtratio.] Hiſce præmiſſis *Propoſitio* hoc modo demonſtratur: Primo ſint quinque rectæ *Fig.* 29. AD, AE, BF, CG, DH, & demptis punctis datis in una rectarum, viz. A, B, C, D, reliqua erunt ſex puncta E, F, G, H, K, L, in quatuor rectis, & tria horum (nam latus numeri triangularis 6 eſt 3) quæ non ſunt ad angulos trianguli, a tribus ſc. rectarum propoſitarum contenti, *ex. gr.* E, F, G, tangant rectam poſitione datam; oſtendendum eſt reliqua tria K, H, L etiam tangere rectam poſitione datam. *Fig.* 29.

Quoniam igitur ſunt quatuor rectæ A E, B F, C G, D F, & tria interſectionum puncta in ipſis ſumantur, viz. E, F, G; erit una aliqua harum rectarum in quâ neceſſario invenietur unum tantum ex hiſce tribus punctis; nam ſecus erit vel aliqua in qua nullum eſt punctum, & proinde erunt tria puncta in tribus reliquis rectis, i. e. ad angulos trianguli contra Hypotheſin; vel erunt ad minimum duo puncta in unâquaque quatuor rectarum, & igitur quatuor eſſent ad minimum puncta; ſed ſunt tantum tria; quare neceſſe eſt eſſe aliquam rectam in qua unum tantum invenitur punctum: Sit hæc recta A E, in qua ſc. eſt punctum E, ergo reliqua duo F, G, ſunt in reliquis tribus rectis B F, C G, D F; igitur, quoniam dantur tria puncta B, C, D, reliquum punctum L in iſtis tribus rectis, tangit rectam poſitione datam per *Prop.* I. Sumatur nunc G E, recta ſc, ex hiſce tribus quæ tranſit per punctum E in quartâ recta, & omnia puncta in hâc recta G E tangent poſitione datam. Quare, per caſum primum hujus propoſitionis, reliqua puncta K, H tangunt rectam poſitione datam.

Fig. 30. Sint jam ſex rectæ A E, A F, B G, C H, D K, E L; & demptis quinque datis punctis A, B, C, D, E, quæ ſunt in unâ rectarum, reliqua erunt decem puncta F, G, H, K, L, M, N, O, P, Q in quinque rectis; & ex Hypotheſi quatuor horum, quæ non in orbem redeunt, tangunt rectam poſitione datam; ſint hæ, F, G, H, K; & oſtendendum eſt reliqua ſex L, M, N, O, P, Q tangere rectam poſitione datam.

Quoniam igitur ſumuntur quatuor puncta interſectionum F, G, H, K, in quinque rectis A F, B G, C H, D K, E L; erit una aliqua recta in qua unum tantum ex hiſce punctis reperitur; nam ſecus erit vel aliqua in qua nullum eſt punctum, & proinde quatuor puncta erunt in quatuor reliquis rectis, & igitur aliqua eorum in orbem redibunt per *Lem.* 2. contra hypotheſin: vel erunt duo ad minimum puncta in unaquaque quinque rectarum, & ita eſſent quinque ad minimum puncta; ſed ſunt tantum quatuor, quare neceſſe eſt eſſe aliquam rectam in quâ unum tantum invenitur punctum; ſit hæc A F in quâ ſc. eſt punctum F; ergo reliqua tria G, H, K ſunt in reliquis quatuor rectis B G, C H, D K, E L, & dantur puncta B, C, D, E; ergo per primam partem hujus demonſtrationis reliqua tria puncta in his quatuor rectis, ſc. L, P, Q, tangunt rectam poſitione datam. Sumatur nunc B F, recta ſc. ex hiſce quatuor, quæ tranſit per punctum F in quinta recta; & omnia puncta in hac recta B F tangent recta poſitione datam: Quare per *Caſ.* 1. hujus Propoſitionis reliqua puncta M, N, O tangunt rectam poſitione datam. Eodem prorſus modo demonſtrabitur Propoſitio in ſeptem, octo, &c. rectis in infinitum, ut patet.

Quod autem conditio uncis incluſa in hac propoſitione omnino ſit neceſſaria, patet in his duobus exemplis; idem vero univerſaliter præcedentium ope demonſtrari poteſt.

His adjecit Clariſſimus Profeſſor Poriſmata duo ſequentia primi Libri Poriſmatum Euclidis *à ſe quoque explicata & emendata* *.

Poriſma 1.] *Si a duobus punctis datis inflectantur duæ rectæ ad rectam poſitione datam, abſcindat autem earum una a recta poſitione data ſegmentum dato in eâ puncto adjacens, auferet etiam altera ab aliâ recta ſegmentum datam habens rationem.*

Fig. 31. Sint enim duo puncta data D, C, a quibus ad poſitione datam A B inflectantur D B, C B; quarum una D B abſcindat a poſitione

data E F ſegmentum K M adjacens dato puncto M: Oſtendendum eſt alteram C B auferre ab aliâ quadam recta ſegmentum datam habens rationem ad ipſum K M.

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* *Vide* pag. xxxv.

Juncta C D occurrat poſitione datis A B, E F in A, F punctis, quæ proinde data erunt. A puncto K, in quo inflexa B D occurrit ipſi E F, ducatur K H parallela ad A D, & occurrens alteri inflexæ B C in H, ipſi vero B A in N. Quoniam igitur dantur puncta A, D, C, dabitur ratio A D ad D C, & igitur ratio N K ad K H; quare ſi jungatur E H occurrens ipſi A D in G, dabitur ratio A F ad F G; ſed datur A F, quare & F G datur, & punctum G; & datum eſt E, quare E G poſitione & magnitudine datur; & datur E F, quare ratio E F ad E G datur; & ducta M O per datum punctum M parallella ipſi A D, & occurrens E G in O, dabitur M O poſitione, & ideo punctum O; & propter parallelas M O, F G, K H eſt M K ad O H, ut E F ad E G, quæ ſunt in data ratione. Igitur recta B C aufert a rectâ E G poſitione data, ſegmentum O H dato puncto O adjacens, in datâ ratione ad ſegmentum M K. *Q. E. D.*

Componetur vero ita, fiat A F ad F G ut A D ad D C, & juncta E G, per M ducatur M O parallela ad A D; oſtendendum eſt, ſi a punctis D, C inflectantur ad A B quævis D B, C B abſcindentes ex ipſis E F, E G, ſegmenta M K, O H punctis M, O adjacentia, fore ipſa in data ratione E F ad E G, ſeu, quod idem eſt, eſſe junctam H K parallelam ipſi A D; hoc vero videtur omiſſum fuiſſe ab *Euclide*, utpote quod tribus verbis indirecte demonſtrari poſſit; *Pappus* autem in *Lem.* 1°. ad Poriſmata, duas directas ejuſdem demonſtrationes affert, quarum ſecundam, quæ paululum eſt corrupta apud *Commandinum*, hic ſubjungemus integritati ſuæ reſtitutam.

Vid. Pap. *Lib.* 7. *fol.* 239. *pag. prior.*

Per Compoſitam vero proportionem hoc pacto:

Quoniam eſt ut A F ad F G ita A D ad D C (*Vid. Fig.* Papp. *Fol.* 238. *pag. poſt. vel Fig. noſtr.* 31.) convertendo erit ut G F ad F A ita C D ad D A, & componendo, permutandoque & convertendo ut A D ad D F ita A C ad C G. Sed proportio A D ad D F compoſita eſt ex proportione A B ad B E, [& E K ad K F, & proportio A C ad C G compoſita eſt ex proportione A B ad B E] & proportione E H ad H G. Proportio igitur compoſita ex A B ad B E & E K ad K F eadem eſt, quæ componitur ex A B ad B E & E H ad H G. Communis auferatur ratio A B ad B E, reliqua igitur E K ad K F eadem eſt quæ E H ad H G; quare H K ipſi A G parallela eſt.

Poriſma 2.] *Quod punctum illud tangit rectam poſitione datam.*

Si a duobus punctis datis C, G (*Fig.* 32.) ducantur duæ rectæ CB, GD occurrentes duabus rectis poſitione datis AB, ED, ſitque recta DB puncta interſectionum jungens parallela ipſi CG, quæ per datum punctum ducitur, interſectio K ductarum tanget rectam poſitione datam.

Occurrant enim poſitione datæ ſibi mutuo in H, & juncta KH occurrat CG in F & BD in M. Igitur propter parallelas eſt AE ad EF (ut BD ad DM hoc eſt) ut CG ad GF; & igitur AE ad CG ut EF ad GF, datur itaque ratio EF ad GF, & datur EG, quare punctum F, & datur punctum H, quare HF, poſitione. Sit itaque ut AE ad EF ita CG ad GF, & juncta HF erit recta quam tangit punctum K; hoc eſt ducta quævis GD, occurrens ipſi FM in K, erit recta linea quæ per CKB tranſit, nam eſt DB ad DM ut AC ad EF, hoc eſt ex conſtructione ut CG ad GF, quare DB ad CG (ut DM ad GF hoc eſt ut) DK ad KG, & igitur eſt CKB recta linea.

Pappus idem aliter demonſtrat in *Lem.* 2 quod hoc modo debet legi: ſc. Ducatur per G, (*Vid. Fig.* Pap. *Fol.* 239. *pag. poſt. vel Fig. noſtr.* 32.) recta linea GL parallela DE, & juncta HK ad L producatur. Quoniam igitur eſt ut AE ad EF ita CG ad GF, & permutando [AE ad CG ut EF ad GF]; ut autem AE ad CG ita eſt EH ad GL, * quod duæ duabus ſunt parallelæ. Ut igitur EF ad FG ita EH ad GL, atque eſt EH parallela ipſi GL, ergo recta linea eſt quæ per HKLF tranſit. *Fig.* 32.

XI. *The General Quadrature of Trinomial Hyperbolical Curves, by Mr.* Sam. Klingenſtiern. N°417. *p.* 45.

XI. N.B. *Curvæ Hyperbolicæ, de quarum quadraturâ hic agitur ab Erud. Auctore, ad unum quaſi genus reducuntur, ex communi quâ gaudent proprietate. Ad hoc enim genus refertur, omnis curva, cujus ordinata datum efficit rectangulum cum recta, quæ ex tribus* partibus neceſſario diverſis & ordine genitis *conſtituitur.* Diverſæ partes *eſſe intelliguntur, quæ ex diverſis abſciſſæ poteſtatibus quomodocunque oriuntur*; Ordine autem genitæ ſunt, *ſi modo ab imâ ad ſummam poteſtatem æquis gradibus aſcendant.*

Species igitur determinantur ac definiuntur ex gradibus Poteſtatum determinatis & definitis.

Primas & ſimpliciſſimas hujus generis (ad quas etiam cæteræ omnes ultimo reducuntur) Neutonus *ipſe primus ex datis Circuli & Hyperbolæ areis dimenſus eſt.*

Coteſius *deinde plures eſſe hujus generis Species, etiam in infinitum (ſecundum ordinem determinatum) progredientes detexit, quæ ad eandem quadraturæ formam ac priores iſtæ & ſimpliciores reduci poſſint.*

Iiſdem veſtigiis inſiſtendo D. Moivræus *Theorema Coteſianum ulterius promovit, ad inventionem radicum æquationum Trinomialium, idque*

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* *Quod duæ duabus ſunt parallelæ*, ſc. AE ad DB, & GL ad DE, unde eſt AE ad DB ut EH ad DH, & eſt DB ad CG, (ut DK ad KG, i. e.) ut DH ad LG; ergo per 22. 5. eſt AE ad CG ut EH ad GL.

adhibendo arcum circuli determinatæ magnitudinis vice circumferentiæ totius. Quo invento omnes hujus generis Species inter ſe commenſurabiles eſſe ſecundum rationem quadraturæ ſuæ ſtatim perſpexit, Methodumque tradidit in exquiſitis ſuis ſcriptis Miſcellaneis *nuper editis, quâ perveniatur ad quadraturam uniuscujuslibet formæ ex datis Circuli & Hyperbolæ quadraturis.*

Ds. Klingens. *in Propoſitione ſua, quæ ſequitur, in unum collegit quicquid de quadraturis curvarum hujus generis antehac a prioribus inventum fuit. Verum tamen ita collegit non quaſi ſint variæ formæ ſub uno genere, ſed quaſi una ſit eademque forma generis ipſius. Theorema duplex eſt, quatenus quadratura referat ad aream, vel citra, vel ultra ordinatam. Exhibetur in ipſis æquationis terminis ſine reductione aut reſtrictione. Inſtituitur ſecundum* Coteſii *doctrinam, uſurpando menſuras Angulorum & Rationum pro areis Circuli & Hyperbolæ. Traditur ſine demonſtratione, utpote cujus veritas facile innoteſcat ex Propoſitionibus* Moivræanis.

Propoſitio.] *Quadrare curvam, cujus abſciſſa eſt z, & ordinata*

$$\frac{c z^{\theta \pm \frac{r}{n}\theta - 1}}{a^{2\theta} \pm a^{\theta-1} b z^{\theta} + z^{2\theta}},$$ *ubi* θ *deſignat numerum quemlibet,* r *&* n *numeros quoſlibet integros & primos inter ſe, & denominator* $a^{2\theta} \pm a^{\theta-1} b z^{\theta} + z^{2\theta}$ *non poteſt reſolvi in duos factores binomios.*

In circumferentia circuli (*Fig.* 33.) centro quovis O intervallo *Fig.* 33. $OR = a$ deſcripta applicetur chorda $RT = b$, cui parallelus ducatur radius O P, ita quidem ut arcus P R ſit quadrante major ſi habeatur $+ b$, minor vero ſi habeatur $- b$. Incipiendo in puncto R, ſumantur ordine tot arcus $R\overset{I}{R}$, $\overset{I}{R}\overset{II}{R}$, $\overset{II}{R}\,\overset{III}{R}$, $\overset{III}{R}\overset{IV}{R}$, $\overset{IV}{R}\overset{V}{R}$, *&c.* arcui P R æquales, quot unitates continet fractio $\frac{r}{n}$, & a punctis $\overset{I}{R}$, $\overset{II}{R}$, $\overset{III}{R}$, $\overset{IV}{R}$, $\overset{V}{R}$, *&c.* ducantur totidem rectæ $\overset{I}{R}\,\overset{I}{r}$, $\overset{II}{R}\,\overset{II}{r}$, $\overset{III}{R}\,\overset{III}{r}$, $\overset{IV}{R}\,\overset{IV}{r}$, $\overset{V}{R}\,\overset{V}{r}$, *&c.* radio O P parallelæ & rectæ O R occurrentes in punctis, $\overset{I}{r}$, $\overset{II}{r}$, $\overset{III}{r}$, $\overset{IV}{r}$, $\overset{V}{r}$, *&c.* Deinde dividatur arcus P R in tot partes æquales quot ſunt unitates in numero n, quarum illa quæ puncto P adjacet ſit P A. Facto initio in puncto A dividatur integra circumferentia in tot partes æquales A B, B C, C D, D E,

&c. quot ſunt unitates in n; ſumtaque in radio O P, producto ſi opus ultra P, abſciſſa $\mathrm{OS} = a.\left.\overline{\frac{z}{a}}\right|^{\frac{n}{n}}$, jungantur S A, S B, S C, S D, S E, *&c.* ut & O A, O B, O C, O D, O E, *&c.* Denique ſumantur arcus P A a, P B b, P C c, P D d, P E e, *&c.* qui ſint ad arcus P A, P B, P C, P D, P E, *&c.* ut $n + r$ ad unitatem, & a punctis a, b, c, d, e, *&c.* ducantur tum rectæ $a\alpha$, $b\beta$, $c\gamma$, $d\delta$, $e\varepsilon$, *&c.* parallelæ radio O P & occurrentes rectæ O R in punctis α, β, γ, δ, ε, *&c.* tum etiam rectæ a 1, b 2, c 3, d 4, e 5, *&c.* prioribus normales, & rectæ Q O, quæ ad R O ducatur perpendicularis, occurrentes in punctis 1, 2, 3, 4, 5, *&c.*

His factis area curvæ cujus abſciſſa eſt z & ordinata

$$\frac{cz^{n+\frac{r}{n}n-1}}{a^{2n} \pm a^{n-1}bz^{n} + z^{2n}}, \text{ erit } \frac{nc}{\eta a^{n+1}} z^{\frac{r}{n}n} \text{ in}$$

$$\frac{\overset{\mathrm{I}}{\mathrm{R}}{}^{\mathrm{I}}_{r}}{r-n} \times \left.\overline{\frac{a}{z}}\right|^{n} - \frac{\overset{\mathrm{II}}{\mathrm{R}}{}^{\mathrm{II}}_{r}}{r-2n} \times \left.\overline{\frac{a}{z}}\right|^{2n} + \frac{\overset{\mathrm{III}}{\mathrm{R}}{}^{\mathrm{III}}_{r}}{r-3n} \times \left.\overline{\frac{a}{z}}\right|^{3n}$$

$$-\frac{\overset{\mathrm{IV}}{\mathrm{R}}{}^{\mathrm{IV}}_{r}}{r-4n} \times \left.\overline{\frac{a}{z}}\right|^{4n} - \frac{\overset{\mathrm{V}}{\mathrm{R}}{}^{\mathrm{V}}_{r}}{r-5n} \times \left.\overline{\frac{a}{z}}\right|^{5n}, \ \&c.$$

$$+\frac{c}{n}a^{\frac{r}{n}n-n-1} \text{ in} \left\{ \begin{array}{l} -a\alpha(\mathrm{SA:AO}) - a1(+\mathrm{SAO}) \\ +b\beta(\mathrm{SB:BO}) + b2(+\mathrm{SBO}) \\ -c\gamma(\mathrm{SC:CO}) + c3(+\mathrm{SCO}) \\ +d\delta(\mathrm{SD:DO}) - d4(-\mathrm{SDO}) \\ +e\varepsilon(\mathrm{SE:EO}) + e5(-\mathrm{SEO}) \\ \qquad \&c. \qquad\qquad\qquad \&c. \end{array} \right\}$$

Et area curvæ cujus abſciſſa eſt z & ordinata

$$\frac{cz^{n-\frac{r}{n}n-1}}{a^{2n} \pm a^{n-1}bz^{n} + z^{2n}}, \text{ erit } \frac{nc}{\eta a^{n+1}} z^{-\frac{r}{n}n} \text{ in}$$

$\overset{\mathrm{I}}{\mathrm{R}}$

Fig. 20.

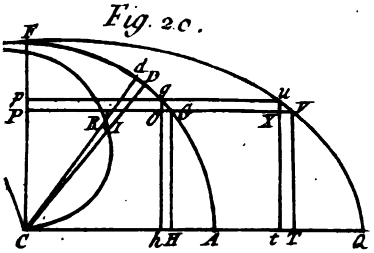


Fig. 21.

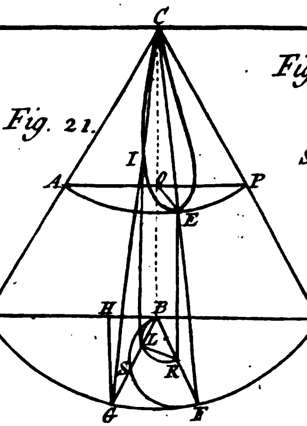
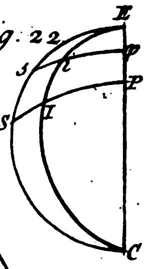


Fig: 22.



:25.

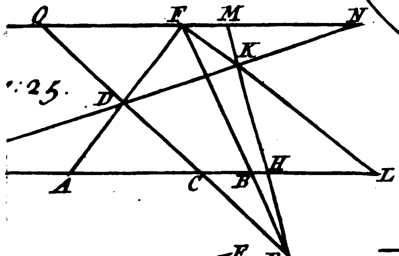
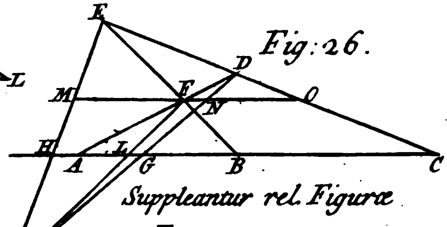


Fig: 26.



Suppleantur rel. Figuræ

7.

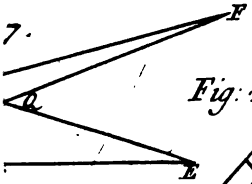
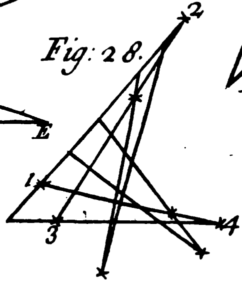


Fig: 28.



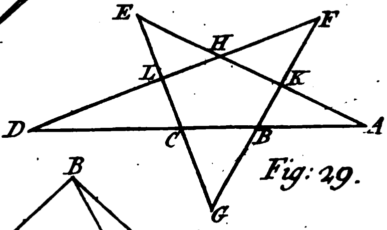


Fig: 29.

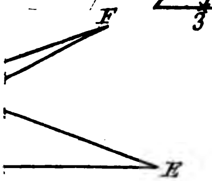
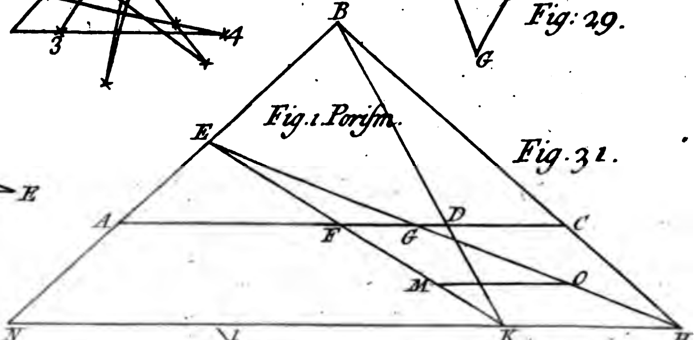


Fig. 1 Porism.

Fig. 31.



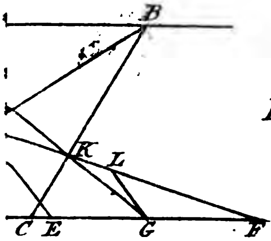
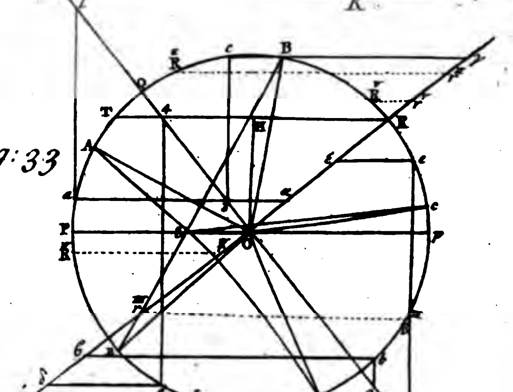


Fig: 33



$$\frac{\overset{\mathrm{I}}{\mathrm{R}}{}^{\mathrm{I}}_{r}}{r-n}\times\overline{\frac{z}{a}}\Big|^{n}-\frac{\overset{\mathrm{II}}{\mathrm{R}}{}^{\mathrm{II}}_{r}}{r-2n}\times\overline{\frac{z}{a}}\Big|^{2n}+\frac{\overset{\mathrm{III}}{\mathrm{R}}{}^{\mathrm{III}}_{r}}{r-3n}\times\overline{\frac{z}{a}}\Big|^{3n}$$

$$-\frac{\overset{\mathrm{IV}}{\mathrm{R}}{}^{\mathrm{IV}}_{r}}{r-4n}\times\overline{\frac{z}{a}}\Big|^{4n}-\frac{\overset{\mathrm{V}}{\mathrm{R}}{}^{\mathrm{V}}_{r}}{r-5n}\times\overline{\frac{z}{a}}\Big|^{5n},\ \&c.$$

$$+\frac{c}{n}a^{-\frac{r}{n}n-n-1}\ \text{in}\left\{\begin{array}{l}-a\alpha(\mathrm{SA}:\mathrm{AO})-a1(+\mathrm{ASO})\\+b\beta(\mathrm{SB}:\mathrm{BO})+b2(+\mathrm{BSO})\\-c\gamma(\mathrm{SC}:\mathrm{CO})+c3(+\mathrm{CSO})\\+d\delta(\mathrm{SD}:\mathrm{DO})-d4(-\mathrm{DSO})\\+e\varepsilon(\mathrm{SE}:\mathrm{EO})+e5(-\mathrm{ESO})\\\quad\&c.\qquad\qquad\&c.\end{array}\right\}.$$

Harum arearum prior adjacet abſciſſæ ad ordinatam terminatæ, poſterior vero abſciſſæ ultra ordinatam productæ. Signa autem quantitatum has expreſſiones ingredientium ita determinantur: 1. Rectæ $\overset{\mathrm{I}}{\mathrm{R}}{}^{\mathrm{I}}_{r}$, $\overset{\mathrm{II}}{\mathrm{R}}{}^{\mathrm{II}}_{r}$, $\overset{\mathrm{III}}{\mathrm{R}}{}^{\mathrm{III}}_{r}$, &c. afficiuntur ſignis affirmativis, ſi a punctis circumferentiæ $\overset{\mathrm{I}}{\mathrm{R}}$, $\overset{\mathrm{II}}{\mathrm{R}}$, $\overset{\mathrm{III}}{\mathrm{R}}$, tendunt ſecundum directionem OP, negativis vero ſi ab iiſdem punctis ſecundum directionem contrariam PO procedunt. 2 Moduli rationum $a\alpha$, $b\beta$, $c\gamma$, &c. ſigna habent affirmativa, ſi a punctis a, b, c, &c. tendunt ſecundum directionem OP, negativa ſi ſecundum contrariam. 3. E centro circuli O cadat in chordam RT normalis OH. Et moduli angulorum $a\,1$, $b\,2$, $c\,3$, &c. ſignis gaudebunt affirmativis ſi a punctis a, b, c, &c. tendunt ſecundum directionem HO, negativis ſi ſecundum contrariam. 4. Producatur radius PO donec circumferentiæ denuo occurrat in p, & anguli SAO, SBO, SCO, &c. ut & ASO, BSO, CSO, &c. ſumi debent affirmative ſi exiſtunt in ſemicirculo ſuperiore PRp, negative ſi in inferiore. Et ſecundum has regulas ſigna quantitatum quibus areæ exprimuntur noſtræ figuræ accommodavimus.

XII. The Problem of finding Curves, which being diſpoſed after a certain Manner in an inverted Situation, may cut each other in a given Angle ſolved. N° 372. *p.* 106.

XII. Solutio.] *Curvarum, quæ problemati conveniunt, quæcunque ſumatur ordinata, illius fluxio ſecunda ab ejuſdem fluxione prima diviſa (ut ſermone arithmeticorum utar) eandem dat quotientem, ſed contrario ſigno, ac fluxio ſecunda, a fluxione prima diviſa ordinatæ ex alterâ principii abſciſſæ parte jacentis,*

centis, & ad eandem ab eo principio diſtantiam[*]. *Hujuſmodi autem curvæ inveniri poſſunt tribus regulis.*

Prima regula *curvam, qualem problema requirit, ope ſpatii hyperbolici à curvâ quacunque deducit, quæ habeat ad æquales diſtantias a principio ſuæ abſciſſæ ordinatas æquales, & ab eâdem parte abſciſſæ poſitas. Eſt enim ordinata curvæ quæſitæ, ut area alius curvæ ordinatam habentis æqualem ſegmento aſymptoti hyperbolæ terminato a ſpatio hyperbolico æquali areæ curvæ primò aſſumptæ.*

Regula autem ſecunda *pendet a prima, & curvam problemati ſatisfacientem ſine ope ſpatii hyperbolici ex curvis derivat, quæ habeant ad æqualia intervalla a principio ſuæ abſciſſæ ordinatas æquales, ſed a contrariis partibus abſciſſæ poſitas.*

Et hæc ſecunda regula theorema ſequens præbet, nimirum, ſi aliqua curva ſumatur, qua problema ſolvi poſſit per regulam prima, & ſi hujus ordinatæ inſiſtunt abſciſſæ ad perpendiculum, invenietur curva problemati ſatisfaciens, ſi ad eandem abſciſſam conſtruatur alia linea curva, eâ lege, ut illius ordinata ex alterâ parte abſciſſæ ubique æqualis ſit aggregato aſſumptæ lineæ curvæ & ejuſdem ordinatæ; exceſſui autem hujus curvæ præ ordinatâ ſuâ æqualis ſit unaquæque curvæ conſtruendæ ordinata, quæ ex alterâ parte abſciſſæ jacet; omnes enim curvæ hac ratione conſtructæ problemati conveniunt.

Hoc autem theorema demonſtratur propoſitione ſequenti, quod in omni triangulo rectangulo quadratum ab alterutro latere angulo recto adjacenti æquale eſt rectangulo ſub ſummâ alterius lateris angulo recto adjacentis lateriſque angulo ei ſubtendentis, & ſub differentiâ eorundem laterum.

Denique tertia *derivatur a ſecunda, ope prop.* 9. *libri de quadraturâ curvarum* Neutoni.

Scholium.] *Exemplum generale, quod exhibui, curva logarithmica, & cyclois plurimis modis inveſtigari poſſunt his regulis.*

Unus caſus curvæ logarithmicæ commode invenitur per regulam primam, aſſumptâ lineâ rectâ loco curvæ in illâ regulâ memoratæ.

Alter hujus lineæ caſus deducitur ex regulâ ſecundâ ope ſpeciei quinquageſimæ nonæ linearum tertii ordinis, quæ omnium curvarum in illâ regulâ utilium eſt fere ſimpliciſſima præter parabolam cubicam & hyperbolam conicam.

Cyclois optime invenitur theoremate, quod a regulâ 2dâ deduci diximus.

Exemplum iſtud generale facile invenitur regulâ tertiâ, aliis vero regulis non ſine ambagibus.

Regulis ſecunda & tertia commodiſſime inveniuntur curvæ geometrice rationales; quæ deducuntur etiam a theoremate in regulam ſecundam pendente; quandocunque enim curva aſſumpta tam longitudinem quam ordinatam rationalem habet, cujuſmodi ſimpliciſſima eſt parabola ſemicubica, curvæ quoque inveniendæ ordinata rationalis erit.

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[*] Scilicet, ſi abſciſſa a ſuo principio in oppoſitas partes æqualibus momentis fluit.

Denique his regulis, vel etiam conditione in principio poſitâ facile eſt invenire, an curva aliqua propoſita problemati ſatisfaciet, & quibus poſitionibus id fiet: unde intelligi poteſt, an eadem curva diverſis modis problemati conveniat.

Horum brevem explicationem jam apponam, deſcribendo, ex amici chartâ, problematis ſequentis ſolutionem.

Problema.] *Datis duabus lineis rectis* A B, C D (*in Fig.* 34.) *parallelis, ad abſciſſam* A B *curva* E F *deſcribenda eſt, quæ talis ſit, ut in ſitu inverſo ad abſciſſam* C D *deſcripta ſeipſam ſemper interſecet in angulo quolibet dato.* Fig. 34.

Ad abſciſſam C D deſcribantur curvæ G H, K L ſimiles & æquales curvæ E F, quarum altera huic curvæ E F occurrat in puncto quolibet, I, altera vero per punctum M tranſeat, ut partes E M, K M curvarum E F, K L ſimiles ſint & æquales; & per punctum M, quod partes curvæ E F dirimit, quæ ſe mutuo interſecare debent, ducantur lineæ N M O, n M o, quæ cum rectis A B, C D angulos ſub N O B & ſub C N O, item angulos ſub n o A, & ſub o n D conſtituant ei æquales, in quo curva ſeipſam ſecare ponitur. Ducatur I P T S lineis A B, C D parallela; item huic proxima & parallela j x p t s; deinde ducatur I v lineæ N O parallela, & denique I w, S y parallelæ lineæ n o, ut angulus ſub I w j æqualis ſit angulo ſub I v x. Jam anguli ſub I x w & ſub I j v, ſimul ſumpti æquales erunt angulo ſub x I M, ideoque & angulo ſub I w v æquales; unde angulus ſub x I w æqualis erit ei ſub I j v; & eodem modo angulus ſub j I v, ei ſub I x w æqualis invenietur; adeo ut triangula I j v, x I w ſunt ſimilia, & j v : I v : : I w : w x. Porro pro abſciſſis æqualibus M P, M T ſcribatur z, pro ordinatâ P I, y, & $-v$ pro ordinatâ T S, pertinente ad curvæ K L arcum K M, qui arcui E M curvæ E F reſpondet. Creſcentibus autem abſciſſis M P, M T, & ſimul increſcentibus ordinatis P I, T S, earum fluxiones primæ eadem habebunt ſigna cum ſuis ordinatis, ſed utræque fluxiones ſecundæ idem habebunt ſignum; nam fluxio ſecunda unius ordinatæ idem habebit ſignum cum ſuâ ordinatâ, ſed alterius ordinatæ fluxio ſecunda ſignum habebit a ſigno ſuæ ordinatæ diverſum; propterea quod curvarum K M, M F alterius concavitas verſus convexitatem alterius convertitur, ut manifeſtum eſt. His autem cognitis invenietur j v : I v (= P p) : : $\dot{y} : \dot{z}$, I w (= T t) : w x (= s y) : : $\dot{z} : -\dot{v}$, & $\dot{y} : \dot{z} : : \dot{z} : -\dot{v}$, item $-\dot{y}\dot{v} = \dot{z}^2$, & denique poſitâ $\dot{z}$ invariabili $-\dot{y}\ddot{v} - \ddot{y}\dot{v} = 0$, vel $\dot{y}\ddot{v} + \ddot{y}\dot{v} = 0$, ideoque $\frac{\ddot{y}}{\dot{y}} = -\frac{\ddot{v}}{\dot{v}}$, quando y & $\ddot{y}$ ad curvam E F, ſed $\dot{v}$ & $\ddot{v}$ ad curvam K M pertinent. Idem vero locum quoque habet, quando omnes hæ fluxiones ad curvam E F referuntur, ſi abſciſſa in oppoſitas partes a ſuo principio fluere ſtatuitur; nam ſumptâ M Q = M P, & M q = M p, ductiſque Q R, q r ad A B, C D parallelis, puncta R, r in curvâ E F punctis S, s in curvâ K M reſpondent. Ponendo igitur abſciſſam in contrarias partes a ſuo principio æqualibus momentis fluere, *Curvarum, quæ problemati conveniunt, quæcunque ſumatur ordinata, illius fluxio ſecunda a fluxione primâ diviſa eandem dat quotientem*, &c. ut ſupra. Hæc autem curvarum quæſitarum conditio eſt, unde deducuntur regulæ ſequentes ad problematis ſolutionem.

Regula 1.] Cum requiritur, ut M Q exiſtente = M P ſit $\frac{\ddot{y}}{\dot{y}}$ proportionalis $-\frac{\ddot{v}}{\dot{v}}$, quando abſciſſa in oppoſitas partes a puncto M æquabiliter fluit, ita ut ejus fluxioni in partibus abſciſſæ, quæ a contrariis lateribus puncti M jacent, ſigna diverſa tribuenda ſint, ponere licet $\frac{\ddot{y}}{\dot{y}} = \dot{z}$ ductæ in quantitatem quamcunque, quæ eadem maneat, & ſub eodem ſigno, pro eâdem magnitudine z, ſive *Fig.* 35. illa affirmativa ſive negativa ſit. Deſcribatur igitur (in *Fig.* 35.) ad abſciſſam N O curva quælibet K L, cujus ordinatæ angulum quemcunque datum cum abſciſſâ conſtituant, & quæ habeat eas ordinatas æquales, & ab eodem latere abſciſſæ N O poſitas, quæ æqualiter diſtant a puncto M, ut ordinatæ P W, Q X; deinde fiat $\frac{\ddot{y}}{\dot{y}}$ ordinatæ P W × $\dot{z}$ proportionalis, & $\frac{\ddot{v}}{\dot{v}}$ ordinatæ Q X × $\dot{z}$.

Fig. 36. Jam (in *Fig.* 36.) exponatur hyperbola Y Z ad aſymptotos Γ Δ, Γ Θ, angulum ſub Θ Γ Δ angulo dato ſub N P W æqualem comprehendentes, deſcripta, & in alterutrâ aſymptoto, ut Γ Δ, ſumatur ad libitum punctum Λ, & ducatur Λ Ξ alteri aſymptoto Γ Θ parallela, & parallelogrammum Γ Ξ compleatur: deinde in curvâ K L ad abſciſſam N O, & ad punctum M ordinatim applicetur M Π; ſumatur ſpatium hyperbolicum Λ Ξ Υ Σ, rectâ Σ Υ aſymptoto

ſymptoto $\Gamma\Theta$ parallelâ abſciſſum, æquale ſpatio $WPM\Pi$, & fiat $P\Phi = \Gamma\Sigma$, eâque ratione deſcribatur curva $\omega\Psi\Phi\Omega$; dico PI curvæ quæſitæ ordinatam eſſe ut ſpatium $MP\Phi\Psi$. Hoc autem manifeſtum eſt; fluxio enim ſpatii $M\Pi WP$ æqualis eſt fluxioni ſpatii $\Lambda\Xi\Upsilon\Sigma$, ideoque $PW \times \dot{z} =$ fluxioni lineæ $\Gamma\Sigma$ ductæ in $\Sigma\Upsilon$ vel in $\dfrac{\Gamma\Lambda \times \Lambda\Xi}{\Gamma\Sigma}$; erit igitur $PW \times \dot{z}$ ut fluxio lineæ $\Gamma\Sigma$ ſive lineæ $P\Phi$ per ipſam $P\Phi$ diviſa; ſed $PW \times \dot{z}$ eſt ut $\dfrac{\ddot{y}}{\dot{y}}$; unde erit $P\Phi \times \dot{z}$ ut $\dot{y}$, & neceſſario y ſive PI ut ſpatium $MP\Phi\Psi$. *Prima* igitur *regula curvam, qualem problema requirit, ope ſpatii hyperbolici*, &c. ut ſupra.

In exemplum hujus regulæ loco curvæ KL (in *Fig.* 35.) ſumatur linea recta lineæ NO parallela, & erit linea $\omega\Psi\Phi\Omega$ ea, quæ logarithmica dicitur, cui NO aſymptotos eſt; ideoque & linea EF etiam logarithmica, per punctum M tranſiens, & aſymptoton habens lineæ NO parallelam; propterea quod area $MP\Phi\Psi$ hic erit ut $P\Phi - M\Psi$ *. Si vero ordinatæ $\varepsilon\eta\zeta$, $\beta\alpha\gamma$ ducantur æqualiter diſtantes a puncto M, ordinatæque $M\Psi$ proximæ, erunt $\varepsilon\eta$, $\alpha\beta$ æquales quando primum naſcuntur, quoniam ſpatia $\varepsilon\zeta\Psi M$, $M\Psi\gamma\alpha$ tunc æqualia ſunt; ex oſtenſis autem eſt $\varepsilon\eta \times \alpha\beta = M\varepsilon^{q}$ vel $M\alpha^{q}$, unde $\varepsilon\eta = M\varepsilon$; & $\varepsilon\eta$ ad $\dfrac{M\Psi\zeta\eta}{M\Psi}$ ut radius ad ſinum anguli ſub $NM\Psi$. Quoniam igitur PI ſemper eſt ut ſpatium $\Psi MP\Phi$, erit PI ubique ad $\dfrac{\Psi MP\Phi}{M\Psi}$ ut radius ad ſinum anguli ſub $NM\Psi$; & denique limes ordinatarum negativarum ad ſpatium totum comprehenſum a parte $\Psi\omega$ lineæ logarithmicæ $\omega\Psi\Omega$ ab ordinatâ $M\Psi$, & ab aſymptoto MO ad ordinatam $M\Psi$ applicatum ut radius ad ſinum anguli ſub $NM\Psi$: eſt autem rectangulum ſub $M\Psi$ & ſub lineæ logarithmicæ $\omega\Psi\Omega$ ſubtangente ad ſpatium prædictum etiam ut radius ad ſinum anguli ſub $NM\Psi$: adeo ut limes ordinatarum negativarum lineæ curvæ EF æqualis erit huic ſubtangenti; unde ſi ΨM retro producatur ad δ,

Fig. 35.

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* Vid. Barrov. Lection. Geometr. p. 123.

ut $M\delta$ huic ſubtangenti ſit æqualis, & ducatur $\theta\delta\lambda$ lineæ N O parallela, erit illa curvæ E F aſymptotos; erit autem curvæ hujus E F ſubtangens lineæ $M\delta$ æqualis; propterea quod $M\varepsilon =$ eſt $\varepsilon\eta$. *Unus* igitur *caſus curvæ logarithmicæ commode invenitur per regulam primam*, &c. ut ſupra.

Hæc autem regula *primum* oſtendit *modum* quo problema ſolvitur.

Regula ſecunda.] Deſcribatur curva quæcunque $\varkappa M \mu$ per punc-
Fig. 35, 37. tum M tranſiens in *Fig.* 35. vel $\varkappa$ n c, m p μ in *Fig.* 37. ubi curva
invenienda duobus cruribus e M F, E M f conſtat; ut curvarum
$\varkappa M \mu$, & $\varkappa$ n c, m p μ ordinatæ ut $P\nu$, $Q\varrho$, quæ æqualiter a puncto
M principio abſciſſæ diſtant, ſint æquales, ſed a contrariis partibus
abſciſſæ poſitæ, ita ut mutato abſciſſæ ſigno, ordinatæ ſignum
etiam mutetur.

Fig. 38. Exponatur porro (in *Fig.* 38.) hyperbola æquilatera a b cujus
axis tranſverſus a g, conjugatus h q, centrum d, aſymptoti d r, d s;
ſumatur d t $= P\nu$, & ducatur t v w ad h q perpendicularis, junctâ
d w, ſumatur quoque d x = M n, & ducatur x y item rectæ lineæ
Fig. 35. h q perpendicularis, junctâ d y. Jam ſit curva K L (in *Fig.* 35.)
Fig. 37. vel K k L l (in *Fig.* 37.) talis ut ſpatium $\Pi M P W$ æquale ſit
ſpatio a d w, ſi curva $\varkappa\mu$ per punctum M tranſit, aliter æquale ſpatio
d a w — d a y; hac enim ratione curvæ K L, & K k L l non
deſinent conditionem habere, quæ in regulâ priori requiritur, nempe
ut ordinatæ ad æquales diſtantias a puncto M ſint æquales, & ab
eâdem abſciſſæ parte poſitæ. Nam area hyperbolica a d w affirmativa
eſt, quando d t vel $P\nu$ eſt affirmativa, & eadem area negativa
eſt, quando d t vel $P\nu$ negativa eſt, quia area tota hyperbolica
ab eâdem parte lineæ h q jacet; ideoque area curvarum
K L, K k L l ad ordinatam $M\Pi$ terminata ſignum ſuum mutabit,
quando abſciſſa M P, magnitudine ſervatâ, ſignum mutat; &
curvæ ordinata nec magnitudinem nec ſignum mutabit, mutatione
ſigni abſciſſæ. Sit porro ad $\varrho =$ parallelogrammo $\Gamma\Xi$ in hyperbolâ
priori: quo efficietur ut t w + t v ſit ad a d ut $\Gamma\Sigma$ ad $\Gamma\Lambda$
*; ſi igitur $\Gamma\Lambda$ fiat = a d, erit t w + t v $= \Gamma\Sigma = P\Phi$. Porro
ducantur ordinatæ $\varepsilon\eta\zeta$, $\alpha\beta\gamma$ ordinatæ $M\Psi$ proximæ; deinde in
Fig. 35. *Fig.* 35. ubi curva $\omega\Psi\Omega$ ſimplex eſt, cum $\varepsilon\eta$ ſit ad $\alpha\beta$ ut ſpatium
$M\Psi\zeta\varepsilon$ ad ſpatium $M\Psi\gamma\alpha$, erit $\varepsilon\eta = \alpha\beta$; unde & earum utraque
$= M\varepsilon = M\alpha$. Ideoque $\varepsilon\eta$ ad $\dfrac{M\Psi\zeta\varepsilon}{M\Psi}$ ut radius ad ſinum
anguli ſub $NM\Psi$, & ubique P I ad $\dfrac{M\Psi\Phi P}{M\Psi}$ in eâdem ratione.

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* Vid. Philoſ. Tranſact. N° 338. prop. 4.

In *Fig.* 37. ubi curva $\omega\psi\Psi\Omega$ ex duobus cruribus compoſita eſt, *Fig.* 37. $\iota\eta$ eſt ad $\alpha\beta$ ut ſpatium $\Psi M\iota\zeta$ ad ſpatium $\psi M\alpha\gamma$ ſive ut $M\Psi$ ad $M\psi$, propterea quod $M\iota =$ eſt $M\alpha$. Cum igitur neceſſe ſit, ut $\iota\eta\times\alpha\beta =$ ſit $M\iota^{2}$, ſcilicet ut crura MF, ME in angulo propoſito ſe mutuo interſecent, erit ratio $\iota\eta$ ad $M\iota$ ſubduplicata rationis $\iota\eta$ ad $\alpha\beta$ vel ſubduplicata rationis $M\Psi$ ad $M\psi$: ideoque $\iota\eta$ ad ſpatium $M\Psi\zeta\iota$ applicatum ad mediam proportionalem inter $M\Psi$, $M\psi$ ut radius ad ſinum anguli ſub $NM\Psi$; & generatim PI ad ſpatium $M\Psi\Phi P$ applicatum ad mediam proportionalem inter $M\Psi$ & $M\psi$ in eâdem ratione. Eſt autem $M\Psi = yx + dx$, & $M\psi = yx - dx$, & ad media eſt proportionalis inter $yx + dx$ & $yx - dx$. Unde utrobique dictis ad, a; dt vel $P\nu$, R; erit $P\Phi = \sqrt{aa + RR} + R$; $R = \frac{1}{2}a \times \overline{\frac{P\Phi}{a} - \frac{a}{\Phi P}}$; & PI ad $\frac{MP\Phi\Psi}{a}$ ut radius ad ſinum anguli ſub $NM\Psi$. *Regula* igitur *ſecunda pendet a primâ, & curvam problemati ſatisfacientem ſine ope ſpatii hyperbolici*, &c. ut ſupra. Nam hic ſine ſpatio hyperbolico curva invenitur, cujus quadraturâ problema ſolvitur.

Duæ autem ſunt in hac regulâ formulæ. Formula prior nimirum $P\Phi = \sqrt{aa + RR} + R$, curvarum geometrice rationalium, quæ maxime hic requiruntur, inventioni accommodatur; facile enim eſt ita ſumere quantitatem indeterminatam R, ut curva $\omega\Psi\Phi\Omega$ quadraturam admittat.

Ne caſus magis compoſiti memorentur, ponatur R vel $P\nu = cz^{\frac{m}{n}}$, ut m & n numeri ſint impares vel inter ſe primi, vel eorum alter unitas: hac enim ratione curva, cujus ordinata eſt $P\nu$, conditionem habebit in hac regulâ neceſſariam, & erit $P\Phi = \sqrt{aa + RR} + R = \sqrt{aa + ccz^{\frac{2m}{n}}} + cz^{\frac{m}{n}} = z^{\frac{m}{n}}\sqrt{cc + aaz^{\frac{-2m}{n}}} + cz^{\frac{m}{n}}$.

Si igitur $\frac{m}{n} + 1$ ſit vel numero $\frac{-2m}{n}$ æqualis, vel ejuſdem multiplex, id eſt, ſi ſumatur $m = -1$, & n numero cuilibet impari æqualis; pars ordinatæ $z^{\frac{m}{n}}\sqrt{cc + aaz^{\frac{-2m}{n}}} + cz^{\frac{m}{n}}$ ſub vinculo incluſa, ideoque & ordinata tota quadraturam admittet *.

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* Vid. in Tract. de quadr. curv. Newton. tab. curv. ſimplicior. quæ quadrari poſſunt.

Verbi causâ, ponatur $\frac{m}{n} = -\frac{1}{3}$, $c = 1$, & $\text{P}\phi = z^{-\frac{1}{3}} \sqrt{1 + aaz^{\frac{2}{3}}} + z^{-\frac{1}{3}}$. Unde erit area $\text{M}\psi\phi\text{P} = \frac{\overline{1 + aaz^{\frac{2}{3}}}|^{\frac{3}{2}}}{aa} + \frac{3}{2} z^{\frac{2}{3}}$, & $\text{PI} = \overline{\frac{1}{aa} + z^{\frac{2}{3}}}|^{\frac{1}{2}} + \frac{3z^{\frac{2}{3}}}{2a}$, curvaque quæſita hac æquatione comprehendetur $a \times \text{PI}q - 3z^{\frac{2}{3}} \times \text{PI} = \frac{1}{a^5} + \frac{3z^{\frac{2}{3}}}{a^3} + \frac{3z^{\frac{4}{3}}}{4a} + az^2$. In hac æquatione cum $z^{\frac{2}{3}}$ ſignum non mutabit, mutatione ſigni abſciſſæ z; pro eâdem ipſius magnitudine tam negativâ quam affirmativâ PI eandem habebit magnitudinem, & ſub eodem ſigno; unicuique autem magnitudini abſciſſæ z reſpondet & affirmativa & negativa ordinata: adeo ut curva quæſita habebit formam hic appoſitam (in *Fig.* 39.); e tribus conſtans cruribus a b c, d e, d f punctis b, d æqualiter a puncto M diſtantibus; quippe eſt $\text{Md} = \text{Mb} = \frac{1}{a^3}$: quando enim eſt $z = 0$, erit $\text{PI}q = \frac{1}{a^6}$; & $\text{P} = \pm\frac{1}{a^3}$.

Fig. 39.

Hæc autem regulæ hujus formula prior *ſecundum* exhibet curvas quæſitas inveniendi *modum*.

In formulâ poſteriori, cum R ſit $= \frac{1}{2} a \times \overline{\frac{\text{P}\phi}{a} - \frac{a}{\text{P}\phi}}$, R vel $\text{P}\psi$ ejuſdem magnitudinis manebit, ſed ſignum mutabit, quando abſciſſa magnitudinem ſuam ſigno mutato retinet, ſi $\text{P}\phi$ talis ſumatur, ut mutando abſciſſæ ſignum $\frac{\text{P}\phi}{a}$ convertatur in $\frac{a}{\text{P}\phi}$, & contra ut $\frac{a}{\text{P}\phi}$ convertatur in $\frac{\text{P}\phi}{a}$. Et hæc formula poſterior *tertium* continet problema ſolvendi *modum*.

Verbi causâ, ſit $\text{P}\phi = a \times \frac{c - z}{c + z}$, quando z eſt affirmativa, &

erit R vel P ν eodem tempore $= \frac{1}{2} a \times \overline{\frac{c-z}{c+z} - \frac{c+z}{c-z}}$, quando autem z negativa eſt, fiet P $\phi = a \times \frac{c+z}{c-z}$, & R vel Q $\varrho = \frac{1}{2} a \times \overline{\frac{c+z}{c-z} - \frac{c-z}{c+z}}$. Hinc autem R æqualis erit $\frac{\mp 2acz}{cc-zz}$, & R $zz \mp 2acz = cc$ R; ideoque curva κ M μ linea tertii ordinis, imo ſpecies earum quinquageſima nona; propterea quod æquationis cc R R $+ aacc = 0$ radices ſunt impoſſibiles *. Linea autem curva hinc invenienda, ſi fiat (in *Fig.* 40) N M vel M O $= c$, logarithmica eſt, *Fig.* 40. cui recta A B eſt aſymptotos. Cum enim P ϕ ſit $= a \times \frac{c-z}{c+z}$, erit eadem $= \frac{ac}{c+z} - \frac{az}{c+z}$. Si igitur (in *Fig.* 41.) in rectâ lineâ qua- *Fig.* 41. cunque $\alpha \varepsilon$ ſumatur $\alpha \kappa = $ O M $= c$, & ei ad perpendiculum erigantur $\alpha \beta$, $\kappa \mu$ quarum $\kappa \mu$ ſit $= a$, & ſi aſymptotis, $\alpha \varepsilon$, $\alpha \beta$ per punctum μ deſcribatur hyperbola $\zeta \eta$, & ſumptâ $\kappa \nu = $ M P $= z$, ducatur $\nu \rho$ aſymptoto $\alpha \beta$ parallela; parti $\frac{ac}{c+z}$ ordinatæ P ϕ reſpondet area, quæ erit ad aream $\kappa \mu \rho \nu$ ut ſinus anguli ſub N P I ad radium, & alteri parti $\frac{az}{c+z}$ ejuſdem ordinatæ reſpondet area, quæ erit ad $a \times \kappa \nu - \kappa \mu \varrho \nu$ in eâdem ratione †. Unde P I, quæ eſt ad $\frac{\text{M P} \phi \Psi}{a}$ ut radius ad ſinum anguli ſub N M Ψ, erit $=$

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* Vid. Newton. Enumerat. linear. tert. ordin. ad Fig. 63.

† Vid. Newton. de quadr. curv. tab. curv. ſimpl. quæ cum circ. & hyperb. compar. poſſunt, form. prim.

$\frac{2\kappa\mu\rho\nu}{a}-\kappa\nu$. Si igitur ſumatur $O\varsigma = OM$, & ducatur ςM ordinatæ PI retro productæ occurrens in χ, ut ſit $P\chi = PM = \kappa\nu$, erit $\chi I = \frac{2\kappa\mu\varrho\nu}{a}$: ideoque linea MI logarithmica, cui AB aſymptotos eſt, & ςM ordinatim applicata, efficiens cum aſymptoto AB angulum ſub $A\varsigma M$ verſus contingentem æqualem dimidio anguli ſub AON. *Alter* igitur *hujus lineæ caſus deducitur*, &c. ut ſupra.

Magis generatim, ſi r ordinatam curvæ alicujus denotat, quæ inſtar curvarum $\kappa M\mu$, & κnc, $mp\mu$ ad abſciſſam NO deſcripta ordinatas habeat æquales, quæ æqualiter diſtant a puncto M, ſed a contrariis partibus abſciſſæ poſitas, poni poteſt ordinata $P\phi = a \times$

$$\left.\frac{\overline{\overline{b\mp cr+drr\mp er^3+\&c.}\times\overline{f\mp gr+\&c}|^{\lambda}\times\overline{b\mp kr+lrr\mp\&c}|^{\mu}}|^{\nu}}{\overline{\overline{b\pm cr+drr\pm er^3+\&c.}\times\overline{f\pm gr+\&c.}|^{\lambda}\times\overline{b\pm kr+lrr\pm\&c.}|^{\mu}}|^{\nu}}\right|^{\pi}$$

Ex priori hujus regulæ ſecundæ formulâ deducitur quoque theorema, cujus ſupra fit mentio, ad inveniendas curvas tam rationales utile, quod *quartus* erit *modus* problema ſolvendi.

Theorema.] Quoniam eſt $P\phi = \sqrt{aa+RR}+R$, & $R = P\nu$, manifeſtum eſt, ſi $\frac{R}{a}$ vel $\frac{P\nu}{a}$ ſit ut fluxio ordinatæ, quæ abſciſſæ ſuæ ad perpendiculum inſiſtat, alicujus curvæ, erit $\frac{\sqrt{aa+RR}}{a}$, ut ejuſdem curvæ fluxio; curvæ autem hujus ordinata æqualis erit areæ curvæ $\kappa\mu$ ad a applicatæ, ſi angulus ſub $MP\nu$ rectus ſit,
Fig. 35. 37. & cum area curvarum (in *Fig.* 35, 37.) $\kappa M\mu$, & κnc, $mp\mu$ eodem ſigno afficiatur, tam quando abſciſſa eſt affirmativa, quam quando eſt eadem negativa, quoniam areæ ad diverſas abſciſſæ partes in illis diverſis caſibus jacent; & præterea cum eiſdem abſciſſæ magnitudinibus areæ æquales reſpondeant, curvæ, quales problema requirit, inveniri poſſunt curvarum ope, quarum ordinatæ ad eaſdem abſciſſæ magnitudines æquales ſint, & ab eâdem abſciſſæ parte poſitæ, ſi modo ordinatæ inſiſtunt abſciſſæ ad perpendiculum.

Deſcripta ſit ejuſmodi curva no, quæ tangat abſciſſam in puncto

M

M (ut in *Fig.* 42.) ſi evaneſcat, quando abſciſſa eſt = 0, fluens *Fig.* 42. quantitas fluxioni longitudinis curvæ n o reſpondens; aliter, quæ habeat ordinatam primam M m (ut in *Fig.* 43.) æqualem magnitudini *Fig.* 43. fluentis iſtius quantitatis, quando abſciſſa eſt = 0. Erigantur ordinatæ P p, Q q; deinde erit P I curvæ quæſitæ ordinata, quæ ab alterâ parte puncti M jacet, vel = M p + P p, vel = M m p + P p; ordinata autem Q R, quæ ab alterâ parte puncti M cadit, vel = M q — Q q, vel M m q — Q q.

Obſervandum autem eſt hoc theorema aliquando partem duntaxat curvæ quæſitæ deſcribere.

Ex ratione autem, qua hoc theorema inveſtigatur, manifeſtum eſt duo crura curvæ hic deſcriptæ ejuſdem lineæ eſſe partes: nimirum utriuſque naturam eâdem æquatione definiri. Hanc autem curvam in ſitu inverſo diſpoſitam ſe interſecare in angulo æquali angulo ſub N O B inde manifeſtum eſt, quod rectangulum ſub fluxione P I & ſub fluxione Q R, ordinatarum ſcilicet æqualiter a puncto M diſtantium, æquale eſt quadrato fluxionis abſciſſæ: ſi enim curvæ n o ordinatæ w r, x t applicentur ordinatis Q q, P p proximæ, & P x, Q w ſint æquales, & ducantur r s t v abſciſſæ N O parallelæ, erunt triangula p t v, q r s rectangula ſimilia & æqualia: *in omni* autem *triangulo rectangulo quadratum ab alterutro latere angulo recto adjacenti æquale eſt rectangulo ſub ſummâ alterius lateris angulo recto adjacentis lateriſque angulo ei ſubtendentis, & ſub differentiâ eorundem laterum.* Igitur $\overline{tv}^2 = \overline{Px}^2 = \overline{pt+pv} \times \overline{pt-pv}$ $= \overline{pt+pv} \times \overline{qr-qs}$: eſt autem ultima ratio P x ad p t + p v ea, quam fluxio abſciſſæ habet ad fluxionem ordinatæ P I; & ratio P x vel Q w ad q r — q s ea, quam fluxio abſciſſæ habet ad ordinatæ Q R fluxionem. Unde conſtat propoſitum. *Regula* igitur *ſecunda theorema*, &c. ut ſupra.

Jam ſi n o ſit circuli circumferentia, linea E F cyclois erit, quando angulus ſub N O B vel ſub N P I rectus eſt. Porro ſi curvæ n o longitudo cum rectâ conferri poteſt, quarum curvarum ſimpliciſſima eſt parabola ſemicubica, curva inventa rationalis erit. Speciatim parabola ſemicubica, ſi rite diſponatur, ejus curvæ partem dimidiam exhibebit, quam in exemplum formulæ prioris regulæ ſecundæ delineavimus; ſcilicet (in *Fig.* 39.) crus d e, partem- *Fig.* 39. que inferiorem b c cruris a b c. Reliquæ autem illius partes deſcribi poſſunt, ſi retro producatur ordinata I P donec pars producta æqualis ſit M m p — P p, & producatur R Q ab altero abſciſſæ latere, donec pars producta æqualis ſit M m q + Q q.

Nunc tranſeundum eſt ad *regulam 3am*, quæ etiam curvas geometrice rationales largitur.

Regula 3.] Regula hæc tertia duos quoque complectitur problema ſolvendi modos a prioris regulæ formulis propoſitione nonâ tractatus de quadraturâ curvarum *Newtoni* derivatos.

Propoſitione iſtâ ad formulam regulæ præcedentis priorem adhibitâ invenitur area curvæ, cujus abſciſſa eſt z, & ordinata $\sqrt{aa+RR}+R$, æqualis areæ curvæ, cujus abſciſſa eſt R & ordinata $\frac{\dot{z}}{\dot{R}}\sqrt{aa+RR}+\frac{\dot{z}}{\dot{R}}R$. Hinc autem *quinto modo* ſolvitur problema.

Verbi causâ, ut exemplum generale, quod antea[*] exhibui, inveſtigetur, poſitis M P $= z$, & P v $=$ R, ut prius, fiat $\frac{\dot{z}}{\dot{R}} = R^{\frac{m-n}{n}} \times \overline{c+dR^2}$, & erit $z = R^{\frac{m}{n}} \times \overline{\frac{n}{m}c+\frac{n}{m+2n}dR^2}$; ſint autem m & n numeri impares vel inter ſe primi vel eorum alter unitas; ut ſigna abſciſſæ z & ordinatæ R ſimul mutentur, ſicut in regulâ priori requiritur; jam erit ordinata $\frac{\dot{z}}{\dot{R}}\sqrt{aa+RR}+\frac{\dot{z}}{\dot{R}}R = R^{\frac{m-n}{n}} \times \overline{c+dR^2} \times \sqrt{aa+RR} + R^{\frac{m}{n}} \times \overline{c+dR^2}$; area igitur curvæ, cujus abſciſſa eſt z & ordinata $\sqrt{aa+RR}+R$, æqualis erit areæ curvæ, cujus abſciſſa eſt R & ordinata $R^{\frac{m-n}{n}} \times \overline{c+dR^2} \times \sqrt{aa+RR} + R^{\frac{m}{n}} \times \overline{c+dR^2}$, ſi modo hæc poſterior ordinata cum abſciſsâ ſuâ angulum contineat æqualem angulo ſub N M Ψ; unde hujus poſterioris curvæ quadraturâ linea exhibetur problemati ſatisfaciens. Erit autem hæc linea curva geometrice irrationalis, niſi m & n certos quoſdam numeros deſignant, vel certa quædam ſit relatio inter coefficientes c, d; hæ autem conditiones ratione ſequenti inveniuntur. Erit * area curvæ, cujus abſciſſa R & ordinata $R^{\frac{m}{n}} \times \overline{c+dR^2} + R^{\frac{m-n}{n}} \times \overline{c+dR^2} \times \sqrt{aa+RR}$,

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[*] In Act. Erud. Menſ. *April.* 1721.

$$\text{ad } R^{\frac{m+n}{n}} \times \overline{\frac{n}{m+n}c + \frac{n}{m+3n}dR^2 + R^{\frac{m}{n}} \times \overline{aa+RR}\big|^{\frac{1}{2}}}$$

$$\overline{\times \frac{n}{maa}c + \frac{d - \big|\frac{m+3n}{maa} \times c}{\frac{m+2n}{n}aa} R^2 + \&c.}$$

ut ſinus anguli ſub NMψ ad radium. Hæc autem ſeries terminabitur, & quadraturam finitam dabit, ſi n ſit unitas & m numerus negativus ternario major, vel ſi ultimus terminorum hic ſcriptorum ſit nihilo æqualis, id eſt, ſi ſit $d = \frac{m+3n}{maa}c$, vel ſi ſit $d = 0$, $n = 1$, & $m = -3$.

Et hic quidem ultimus caſus curvam exhibet, quæ theoremate præcedenti a parabolâ ſemicubicâ invenitur.

Magis generatim ponere licet $\frac{\dot{z}}{\dot{R}} = R^{\frac{m-n}{n}} \times \overline{c + dR^2 + eR^4 + \&c. \ldots + fR^p}$, ubi p numerum quemcunque parem denotat; unde fiat $z = R^{\frac{m}{n}} \times \overline{\frac{n}{m}c + \frac{n}{m+2n}dR^2 + \frac{n}{m+4n}eR^4 \ldots + \frac{n}{m+pn}fR^p}$, & curvæ $\omega\Omega$ ordinata $= R^{\frac{m-n}{n}} \times \overline{c+dR^2+eR^4 \ldots + fR^p} \times \overline{\sqrt{aa+RR}+R}$. Hinc † ſi n ſit unitas & m numerus negativus numero $p+1$ major, curva dabitur geometrice rationalis, vel ſi certa quædam relatio ſit inter coefficientes c, d, e, &c. .... f, quæ relatio facile invenitur ut antea.

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* Per prop. quint. quadr. cur. Newton. † Per prop. proxim. citat.

Porro ad alteram regulæ adæ formulam adhibendo propoſitionem nonam memoratam libri de quadraturâ curvarum, *ſextus* oritur problema ſolvendi modus.

Literâ r denotante ut ſupra, fieri poteſt ordinata $P\Phi = a \times \frac{b+cr+drr+\&c}{b-cr+drr-\&c}$, area curvæ, cujus abſciſſa eſt z & ordinata $P\Phi$, æqualis erit areæ curvæ, cujus abſciſſa eſt r & ordinata $a \times \frac{\dot{z}}{\dot{r}} \times \frac{b+cr+drr+\&c}{b-cr+drr-\&c}$. Ponatur igitur $\frac{\dot{z}}{\dot{r}} = r^{\frac{m-n}{n}} \times \overline{\overline{b+cr+drr+\&c} \times \overline{b-cr+drr-\&c}}\Big|^{p} = r^{\frac{m-n}{n}} \times \overline{bb+\overline{2bd-cc} \times rr+ddr^4+\&c}\Big|^{p}$, & curva, cujus ordinata eſt r conditionem hic neceſſariam habebit. Erit enim $z = r^{\frac{m}{n}} \times \overline{bb+\overline{2bd-cc} \times rr+ddr^4+\&c}\Big|^{p+1} \times \overline{A+Brr+Cr^4+\&c}$, cujus ſeriei coefficientes A, B, C &c dantur per propoſitionem quintam Tractatus de Quadraturâ Curvarum. Manifeſtum autem eſt nec terminos hujus ſeriei nec quantitatem $\overline{bb+\overline{2bd-cc} \times rr+ddr^4+\&c}\Big|^{p+1}$ ſigna ſua mutare mutatione ſigni quantitatis r; quantitas autem $r^{\frac{m}{n}}$, ſi m, n numeri ſint impares, ſignum mutabit, quando ipſa r ſignum mutat; ideoque ordinata r & abſciſſa z ſigna ſimul mutabunt. Ordinata autem $a \times \frac{\dot{z}}{\dot{r}} \times \frac{b+cr+drr+\&c}{b-cr+drr-\&c} =$ erit $ar^{\frac{m-n}{n}} \times \overline{bb+\overline{2bd-cc} \times rr+ddr^4+\&c}\Big|^{p-1} \times \overline{b+cr+drr+\&c}\Big|^{2}$. Et hinc facile inveniri poſſunt curvæ rationales.

Pro exemplo ſimplici ponatur $p = 1 = m = n$, d, $\&c = 0$; unde de erit $\frac{\dot z}{\dot r}=bb-ccrr$, & $z=bbr-\frac{1}{3}ccr^3$. Ordinata autem curvæ metiendæ $=abb+2abcr+accrr$; ejuſdem igitur area eſt, ad $abbr+abcrr+\frac{1}{3}accr^3$, ut ſinus anguli ſub NMΨ, ad radium; ideoque erit PI $=bbr+bcrr+\frac{1}{3}ccr^3$. Hinc autem invenitur parabolam ſemicubicam problemati ſatisfacere, quam ita deſcribere oportet. Datâ (in *Fig.* 44.) lineâ rectâ AB, & in eâ puncto C, una cum lineâ rectâ CD angulum ſub BCD cum lineâ CB conſtituente æqualem angulo, in quo curva ſe interſecare requiritur. Ducatur ad libitum HGI ad CD parallela, ſumaturque in eâ GH $=2$ CG; deinde dividatur angulus ſub ACD in duas partes æquales lineâ rectâ CE, & denique ad diametrum HI & verticem H deſcribatur parabola ſemicubica KHL, quæ tranſeat per punctum C, ita ut CE ordinatim applicetur ad diametrum HI. Hæc parabola ad eandem lineam ſimiliter applicata, ſed ſitu inverſo, ſe interſecabit in angulo æquali angulo ſub BCD. *Fig.* 44.

Si placet curvas hac regulâ inventas theoremate præcedente conſtruere, ex iis, quæ hic tradita ſunt, curva huic negotio apta inveniri poteſt; erit enim curvæ illius ordinata æqualis areæ curvæ $\kappa\mu$ ad a applicatæ, quando angulus ſub MPγ rectus eſt. Verbi causâ, hujus areæ fluxio, nimirum $P\gamma\times\dot z$ in exemplo ſecundo prioris partis hujus regulæ erit $=\dot R R\times R^{\frac{m-n}{n}}\times\overline{c+dR^2+eR^4\ldots+fR^p}|$ $=\dot R R^{\frac{m}{n}}\times\overline{c+dR^2+eR^4\ldots+fR^p}|$; ideoque curvæ hic requiſitæ ordinata erit

$$=\frac{1}{a}R^{\frac{m+n}{n}}\times\overline{\frac{n}{m+n}c+\frac{n}{m+3n}dR^2+\frac{n}{m+5n}eR^4\ldots+\frac{n}{\overline{m+p+1}\times n}fR^p}.$$

In exemplo poſterioris partis hujus regulæ erit R $\left(=\frac{1}{2}a\times\overline{\frac{P\Phi}{a}-\frac{a}{P\Phi}}\right)=\frac{2bcr+2cdr^3+\&c}{bb+\overline{2bd-cc}\times rr+ddr^4+\&c}$; ideoque $R\times\dot z=\dot r r^{\frac{m}{n}}\times\overline{2bc+2cdr^2+\&c}\times\overline{bb+\overline{2bd-cc}\times rr+ddr^4+\&c}|^{p-1}$; hæc igitur eſt fluxio ordinatæ curvæ quæſitæ.

Si ſit $m = 1 = n = p$, d, &$c = 0$, erit $R \times \dot{z} = 2bcr\dot{r}$, & ordinata curvæ quæſitæ $= bcrr$; quoniam igitur $z =$ erit $bbr - \frac{1}{3}ccr^3$, erit curva quæſita in hoc caſu parabola divergens cum nodo, quæ definitur hac æquatione $3ezz = y^3 - 2eyy + eey$*. Et hac curvâ deſcribetur parabola ſemicubica ſupra inventa.

Fig. 45. Verbi causâ, ad rectam lineam (in *Fig.* 45.) A B ducatur perpendicularis C D, & ad illam ut axim deſcribatur ejuſmodi parabola divergens F E C E G. Deinde ducatur ad libitum H I angulum quemcunque datum cum rectâ A B conſtituens, & ducatur H K L M ad C D parallela; deinde ſumatur H N = H K + arc. C K, H O = H L + arc. C K L, & ab alterâ parte puncti H, H P = C E M − H M; & curva hac ratione deſcripta parabola ſemicubica erit.

Hinc apparet quomodo curvæ, quarum inveſtigationi regula hæc tertia aptatur, theoremate præcedenti conſtrui poſſunt, poſtquam earum formæ cognoſcuntur, ſed hæ curvarum formæ, a quibus rationales deriventur, regulâ hac tertiâ optime inveniuntur.

Hæ ſunt tres regulæ, quarum ſupra fit mentio. Ultima ſententia, *denique his regulis*, &c. exemplo ſequenti illuſtrari poteſt.

Sit y vel $= a + bx + \sqrt{c + 2dx + ex^2}$ vel $= \dfrac{a + bx + cxx}{d + ex}$, quæ duæ æquationes omnes complectuntur ſectiones conicas. Inde vero inveniemus $\dfrac{\ddot{y}}{\dot{y}}$ vel $= \dfrac{ec - dd}{\overline{d + ex + b\sqrt{c + 2dx + exx}} \times \overline{c + 2dx + exx}}\dot{z}$, vel $= \dfrac{2cdd + 2aee - 2bde}{\overline{d + ex} \times \overline{bd - ae + 2cdx + cexx}}\dot{z}$; quæ æquationes oſtendunt in nullâ ſectione conicâ, quomodocunque diſponatur, quantitatem $\dfrac{\ddot{y}}{\dot{y}}$ conditionem habere, quam hoc problema requirit; ideoque nullam ſectionem conicam problemati ſatisfacere. Quod comprobari etiam poteſt examinando rectangulum ſub fluxionibus primis ordinatarum æqualiter ad diverſas partes a principio abſciſſæ diſtantium.

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* Vid. Enumerat. linear. tert. ord. *Fig.* 73.

Hinc autem cognoſcitur nullam lineam curvam geometrice rationalem problema ſolvere, quæ parabolâ ſemicubicâ ſit ſimplicior.

Si vero talis inter quantitates a, b, c, d, e relatio ſtatui potuiſſet ut $\frac{\ddot{y}}{\dot{y}}$ conditionem in hoc problemate neceſſariam obtineret, nempe ut quantitas, quæ in $\dot{z}$ ducitur, eadem eſſe potuiſſet, & ſub eodem ſigno, pro eâdem magnitudine tam negativâ quam affirmativâ abſciſſæ z, quo eveniret ut $\frac{\ddot{y}}{\dot{y}}$ foret $= -\frac{\ddot{v}}{\dot{v}}$, ſi abſciſſa in oppoſitas partes a ſuo principio, æqualibuſque momentis fluere ponitur: tum profecto ſectio conica hinc determinanda vel problema ſolveret, vel ſectionis problemati ſatisfacientis ordinata ad ordinatam hujus rationem haberet datam.

Jam vero his regulis alias aliquot, quas ab amico accepi, ad problema ſolvendum adjungam.

Regula 4.] Iiſdem poſitis ac in regulâ primâ, ſit (in *Fig.* 46.) *Fig.* 46. N O ad A B, C D perpendicularis; ſint P I, Q R ordinatæ æqualiter a puncto M diſtantes, & ſit curva G H per punctum I ducta ſimilis & æqualis curvæ f E F. Ordinatis P I, Q R parallelæ & proximæ ducantur π j l, δ r, & lineæ rectæ I k, R s lineæ N O parallelæ. Angulus ſub s R r = eſt angulo ſub k I l; unde anguli ſub j I k, s R r ſimul ſumpti æquales ſunt angulo dato ſub j I l; & quantum angulus ſub j I k dimidium anguli ſub j I l ſuperat, tantum angulus ſub s R r ab eodem dimidio deficit. Si igitur (in *Fig.* 47.) *Fig.* 47. radio quolibet m n circuli arcus n o deſcribatur, & ſumatur angulus ſub n m p = dimidio anguli dati ſub j I l, angulus ſub n m q = angulo ſub j I k, & angulus ſub n m t = ei ſub s R r, ſectores q m p, p m t erunt æquales. Poſitâ autem I k = R s = 1, erit j k ut tangens anguli ſub j I k vel anguli ſub n m q, & r s erit ut tangens anguli ſub s R r vel anguli ſub n m t; ideoque & fluxio ordinatæ P I erit ut tangens anguli ſub n m q, nimirum ut n v; & fluxio ordinatæ Q R ut tangens anguli ſub n m t, nimirum ut n w; curvæ igitur Ψ Φ Ω, cujus areæ ordinata P I proportionalis eſt, ordinata P Φ poteſt eſſe æqualis tangenti n v, & ordinata Q χ ab alterâ parte puncti M = n w. Quoniam autem ſectores p m q, p m t ſunt æquales, conſtitui poteſt ſector p m q æqualis areæ M Π W P curvæ cujuſcunque K L conditionem habentis in regulâ primâ indicatam; & ſector p m t æqualis areæ M Π X Q ejuſdem curvæ. Denique ſi ducatur linea recta ι η ζ lineæ M Ψ parallela & proxima; cum angulus ſub ι M η = ſit dimidio anguli ſub j I l, vel angulo ſub nmz, erunt triangula $\varepsilon M\eta$, nmz ſimilia, & prima ratio $\varepsilon\eta$ ad εM eadem cum ratione zn ad nm; ideoque $\varepsilon\eta = \frac{M\Psi\zeta\varepsilon}{nm}$, propterea quod $\varepsilon M =$ eſt $\frac{M\Psi\zeta\varepsilon}{M\Psi}$, & $M\Psi = nz$. Hic autem habetur *ſeptimus modus*, quo problema ſolvi poteſt.

Si loco curvæ KL linea recta ſumatur, quicunque ſit angulus ſub nmz eadem deſcribetur curva; adeo ut hac ratione invenitur una eademque curva, quæ diverſis ſitibus in angulo quocunque dato problema ſolvit. Hæc autem curva a circuli & hyperbolæ quadraturâ dependet; ſi enim ducantur $m\tau$, $n\sigma$ ad mn perpendiculares, quarum $n\sigma =$ ſit mn, & aſymptotis mn, $m\tau$ hyperbola $\omega\sigma\psi$ deſcribatur, & deinde $q\phi\upsilon$, $p\theta\varsigma$ ducantur lineis $m\tau$, $n\sigma$ parallelæ; quando $MP =$ eſt arcui circuli pq, erit ordinata $PI = \frac{\theta\phi\upsilon\varsigma}{mn}$, ſi $mn =$ ſit $2M\Pi$ *.

Fig. 46. Fig. 48. *Regula* 5.] Deſcribatur (in *Fig.* 46.) curva $\varkappa M\mu$ ut in regulâ ſecundâ, & (in *Fig.* 48.) radio $= mn$ deſcribatur ſemicirculus $\alpha\beta\gamma$, cujus centrum δ, ſit autem $\delta ϐ$ diametro $\alpha\gamma$ perpendicularis. Sumatur $\delta\varepsilon = P\nu$, ducatur $\varepsilon\zeta$ ad $\delta ϐ$ parallela, jungaturque $\delta\zeta$. Deinde ſit curva KL ejus naturæ, ut area $M\Pi WP$ ſemper æqualis ſit ſectori $ϐ\delta\zeta$. Fig. 47. In circuli arcu (*Fig.* 47.) no ductis $p\eta$ ſinu arcus pq, & $p\theta$ ſinu arcus np, producatur mp ad z, ducaturque $z\xi$ ad $p\eta$ parallela. Porro dictis $mn = mp$, a; $m\theta$, b; nz, c; $p\eta$, R; nv, y; erit ut $mp : p\eta :: mz : z\xi$, ſed ut $m\theta : mn\ (mp) :: mn : mz$; ex æquo igitur ut $m\theta\ (b) : p\eta\ (R) :: mn : z\xi :: mv\ (\sqrt{aa+yy}) : zv\ (y-c)$ unde $by - bc = R\sqrt{aa+yy}$, & denique $y = nv = P\Phi = \frac{bbc + aR\sqrt{aa-RR}}{bb-RR}$.

Hinc autem *modo octavo* ſolvitur problema.

Regula 6.] Per propoſitionem nonam Tractatus de Quadraturâ Curvarum area curvæ, cujus abſciſſa eſt z & ordinata

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* Vid. Barrov. lect. geometr. pag. 110.

$\frac{bbc + aR\sqrt{\overline{aa - RR}}}{bb - RR}$, æqualis eſt areæ curvæ, cujus abſciſſa eſt R & ordinata $\frac{\dot{z}}{\dot{R}} \times \frac{bbc + aR\sqrt{\overline{aa - RR}}}{bb - RR}$. Unde habetur *modus nonus* problema ſolvendi.

Literæ *m* & *n* eadem denotent, ac in regulâ tertiâ, & fiat $\frac{\dot{z}}{\dot{R}}$

$$R^{\frac{m-n}{n}} \times \overline{bb - RR}|^{p}$$, & ordinata $\frac{\dot{z}}{\dot{R}} \times \frac{bbc + aR\sqrt{\overline{aa - RR}}}{bb - RR}$ fiet

$$= bbcR^{\frac{m-n}{n}} \times \overline{bb - RR}|^{p-1} + aR^{\frac{m}{n}} \times \overline{bb - RR}|^{p-1} \times \sqrt{\overline{aa - RR}}.$$

Unde ſi *n* unitatem denotet, & *p* numerum quemcunque integrum & affirmativum, curva geometrice rationalis invenietur, ſi modo *m* ſit numerus affirmativus, vel etiam ſi *n* ſit unitas, *m* numerus affirmativus & 2 *p* numerus impar negativus numero *m* major.

Regula 7.] Ducatur (in *Fig.* 48.) β λ ſemicirculum α β γ contingens *Fig.* 48.
in β, & producatur ε ζ ad μ, ductâ δ ν μ. Sit autem curva (in *Fig.* *Fig.* 46.
46.) K L ejus naturæ, ut area M Π W P = ſit ſectori δ β ν. Dictis igitur m n, *a*; n z, *c*; & tangente arcus p q, R; erit n v = P Φ = $\frac{aac + aaR}{aa - cR}$. Et hic eſt *decimus* problema ſolvendi modus.

Quando angulus interſectionis rectus eſt, & $c = a$, hæc regula ſub formulâ poſteriori regulæ ſecundæ comprehenditur.

Item ſi loco *x* M μ linea recta ſumatur, quicunque ſit interſectionis angulus, caſus ille curvæ logarithmicæ invenietur, quem in regulâ ſecundâ tradidimus.

Regula 8.] Ut antea, eſt area curvæ, cujus abſciſſa *z* & ordinata $\frac{aac + aaR}{aa - cR}$, æqualis areæ curvæ, cujus abſciſſa eſt R & ordinata $\frac{\dot{z}}{\dot{R}}$

$\frac{\dot z}{\dot R} \times \frac{aac + aaR}{aa - cR}$. Hic autem eſt *undecimus modus* problema exequendi.

Literis m, n eadem denotantibus, ac antea, ſi $\frac{\dot z}{\dot R} = R^{\frac{m-n}{n}} \times \overline{a^4 - ccRR}|^p$; & ordinata $\frac{\dot z}{\dot R} \times \frac{aac + aaR}{aa - cR}$ fiet $= R^{\frac{m-n}{n}} \times \overline{a^4c + a^4 + a^2cc \times R + aacRR \times a^4 - ccRR}|^{p-1}$: quæ formula curvas geometrice rationales facile præbet.

Si ſit $m = 1 = np$; eadem parabola ſemicubica atque ex regulâ tertiâ invenietur.

Fig. 49. *Regula* 9.] Si (in *Fig.* 49.) N O ad lineas A B, C D perpendicularis ſit, & ducatur curva K L, cujus ordinatæ P W, Q X, quæ æqualiter a puncto M diſtant, æquales ſint, & ab eâdem abſciſſæ parte poſitæ; radio ordinatæ P W æquali deſcribatur circuli ſegmentum a b c, quæ angulum comprehendat angulo æqualem, in quo curva ſe ipſam ſecare requiritur cujus ſegmenti centrum æqualiter diſtet a lineis A B, C D. Ducatur autem & alia curva $\varkappa M \mu$ cujus ordinatæ $P\nu$, $Q\varrho$ æqualiter a puncto M diſtantes ſint æquales & a contrariis partibus abſciſſæ N O poſitæ. Deinde ſumptâ M f $= P\nu$ ductâque f h lineæ N O ad perpendiculum, junctâque c h, manifeſtum eſt, ſi curva quæſita E F ejus ſit naturæ, ut contingens in puncto I ſemper ſit parallela lineæ c h, propoſito ſatisfaciet. Nam cum ſit W P = Q X, idem circuli ſegmentum ordinatis P W, Q X convenit; adeo ut ſi ſumatur M g = $Q\varrho$, ducatur g k ad N O perpendicularis, & jungatur c k, linea recta contingens curvam quæſitam E F in puncto R parallela erit lineæ c k. Quoniam igitur $Q\rho =$ eſt $P\nu$, ideoque M g = M f in ſitu hujus curvæ E F inverſo, & quando punctum R in punctum I cadit, contingens in puncto R lineæ puncta a, h conjungenti parallela erit, & cum contingente in puncto I angulum conſtituet æqualem ei ſub a h c, nimirum angulo in ſegmento a b c comprehenſo.

Invenitur igitur hujuſmodi curva, ſi fiat ut $\dot y : \dot z :: fh : fc$. Quamobrem ſi pro P W ponatur m; pro a M = M c ponatur n; pro intervallo inter punctum M & centrum ſegmenti ponatur p; & pro $P\nu =$ M f, q; habebimus $\dot y : \dot z :: \sqrt{mm - qq} \pm p : n + q$, & $\dot y =$

$\sqrt{mm} -$

$\frac{\sqrt{mm-qq}\pm p}{n+q}\cdot z$. Dantur autem rationes inter m, n, p ob datum ſegmenti a b c angulum, & invenietur y vel P I metiendo curvam, cujus abſciſſa eſt $\dot{z}$ & ordinata $\frac{\sqrt{mm-qq}\pm p}{n+q}$. Hic autem exhibetur *duodecimus modus* problema tractandi.

Si angulus ſub a h c ſit rectus, erit $p=0$, $n=m$, & ordinata curvæ metiendæ $\sqrt{\frac{m-q}{m+q}}$. Quam profecto ordinatam problemati ſatisfacere, intelligi quoque poteſt ex poſteriori regulæ ſecundæ formulâ.

Si loco linearum curvarum K L, κ M μ rectæ ſumantur, quando angulus ſub a h c rectus eſt, erit curva E F *cyclois*; quæ facile determinatur formâ undecimâ tabulæ curvarum ſimpliciorum, quæ cum circulo & hyperbolâ comparari poſſunt in Tractatu de Quadraturâ Curvarum *Newtoni.*

Regula 10.] Porro area curvæ, cujus abſciſſa eſt z & ordinata $\frac{\sqrt{mm-qq}\pm p}{n+q}$, æqualis eſt tum areæ curvæ, cujus abſciſſa eſt m & ordinata $\frac{\dot{z}}{\dot{m}}\times\frac{\sqrt{mm-qq}\pm p}{n+q}$; tum areæ curvæ, cujus abſciſſa eſt q & ordinata $\frac{\dot{z}}{\dot{q}}\times\frac{\sqrt{mm-qq}\pm p}{n+q}$. Unde habentur *duo alii modi*, quibus problema ſolvi poteſt; quorum poſteriori, ratione ſequenti, curvæ geometrice rationales inveniri poſſunt.

Sint δ, ι numeri impares, η numerus par, & ponatur $\frac{\dot{z}}{\dot{q}}=q^{\frac{\delta-1}{\iota}}\times\overline{n^{\eta}-q^{\eta}}$, item $m=1+\frac{1}{4}qq$. Unde erit $z=$ areæ curvæ, cujus abſciſſa eſt q & ordinata $q^{\frac{s-1}{s}} \times \overline{n^{s} - q^{s}}$, & ordinata $\frac{\dot{z}}{\dot{q}} \times \frac{\sqrt{mm - qq} \pm p}{n + q}$ fiet $= \overline{n^{s-1} - n^{s-2}q + n^{s-3}qq + \&c. \ldots - q^{s-1}} \times \overline{1 - \frac{1}{4}qq \pm p}$.

His *quatuordecim* diverſis *modis* generalibus amicus meus problematis ſolutionem abſolvit. Demonſtrationes autem illius ex compoſitione uſus in hoc problemate curvarum Geometris notarum ſic ſe habent.

Fig. 50. *De Caſu* 1 *Lin. Logarith.*] Sit (in *Fig.* 50.) A B linea logarithmica aſymptoton habens C D; eique ordinatim applicetur E F, quæ ſit ſubtangenti logarithmicæ æqualis. Ad lineam rectam E F & ad quodcunque in eâ punctum I conſtituatur alia linea logarithmica G H I priori ſimilis & æqualis, ſed ſitu inverſo diſpoſita. Deinde ſi contingentes H L, H M ducantur, dico angulum ſub L H M angulo ſub C E F eſſe æqualem.

Ordinatim applicetur H N, fiat E O = E N, ordinatim applicetur O P, & ducatur contingens P Q. Puncta P & H æqualiter diſtant a rectâ E I, unde punctum P in curvâ A B puncto H in curvâ G I reſpondet, & angulus ſub O P Q = eſt angulo ſub NHM, propterea quod curvæ A B, G I ſimiles ſunt & æquales. Quoniam vero curva A B eſt logarithmica & E N, E O æquales, erit N H × O P = E F q. Eſt autem E F = N L = O Q, unde ut N H : E F (N L) : : E F (O Q) : O P. Cum igitur anguli ſub H N L, Q O P ſint æquales, triangula H N L, Q O P ſunt ſimilia, & angulus ſub Q P O, qui æqualis eſt angulo ſub N H M, æqualis quoque erit angulo ſub N L H. Unde anguli ſub N H M & ſub N L H æquales erunt, & angulus ſub L H M angulo ſub C N H ſive angulo ſub C E F æqualis. Q. E. D.

Fig. 51. *De Caſu altero Lin. Log.*] Sint (in *Fig.* 51.) A B, C D duæ lineæ rectæ parallelæ, intra quas ~~quælibet~~ alia linea recta E F ducatur. Ad aſymptoton A B deſcribatur linea logarithmica G H, cujus ſubtangens ſit æqualis lineæ E F, & ordinatim applicatæ comprehendant cum aſymptoto angulos verſus contingentes æquales parti dimidiæ anguli ſub A E F. Quibus poſitis, ſi ad aſymptoton C D alia deſcribatur linea logarithmica I L M, priori ſimilis & æqualis, & ſi ducantur contingentes L N, L O, dico angulum ſub O L N angulo ſub B E F eſſe æqualem.

Ducatur N P, ut angulus ſub A N P angulo ſub A E F ſit æqualis, & erit N P = E F. Sumatur N Q lineæ E F ſive ſubtangenti lineæ logarithmicæ æqualis, jungaturque Q L. Quoniam igitur Q L punctum Q conjungit cum puncto contactûs L, Q L ordinatim ad aſymptoton A B applicabitur, ideoque angulus ſub L Q N verſus contingentem L N æqualis erit parti dimidiæ anguli ſub A E F vel anguli ſub A N P; eſt autem N P = E F = N Q; quoniam igitur N P, N Q ſunt æquales, & angulus ſub L Q N æqualis dimidio anguli ſub A N P, recta Q L producta tranſibit per P efficiens triangulum P N Q iſoſceles. Eâdem ratione ſi ducatur O S, ut angulus ſub C O S æqualis ſit angulo ſub A E F erit O S = E F; ſi vero ſumatur O R = E F, ducaturque R L, ordinatim ea applicabitur ad aſymptoton C D, & producta tranſibit per S, propterea quòd linea I M ſimilis eſt & æqualis lineæ G H. Erit autem angulus ſub P R L (= angulo ſub L S Q) = angulo ſub L Q S = angulo ſub N P Q. Unde erit angulus ſub L S Q = angulo ſub N P Q, & triangula S L Q, P N Q ſimilia ſunt, angulusque ſub S L Q = angulo ſub P N Q = angulo ſub B E F. Eſt autem & L S = L Q, O S = N Q, item angulus ſub O S L (= angulo ſub O R S) = angulo ſub N Q L. Triangula igitur O S L, N Q L æqualia ſunt, habentia baſes O L, N L æquales, & angulos ſub N L Q, O L S, etiam æquales: auferatur communis angulus ſub N L S, & relinquetur angulus ſub O L N = angulo ſub S L Q = angulo ſub B E F. Q. E. D.

Fig. 52. *De Cycloide.*] Sint (in *Fig.* 52.] A B, C D duæ rectæ lineæ parallelæ, quas E F ad perpendiculum ſecet. In diametrum E F deſcribatur ſemicirculus E G F, & eo ſemicirculo deſcribatur ſemicyclois F H. Jam ſi alia ſemicyclois I L Q priori ſimilis & æqualis ſed ſitu inverſo intra parallelas deſcribatur, & ſi contingentes L M, L N ducantur, dico angulum ſub M L N rectum eſſe.

Sit I O P ſemicirculus, quo deſcribitur ſemicyclois I Q, ejus diameter I P; ducatur L G O, lineis A B, C D parallela, & jungantur F G, G E, I O. Erit deinde contingens L M parallela rectæ F G, & contingens L N parallela rectæ I O, quæ parallela eſt rectæ E G. Angulus igitur ſub M L N = eſt angulo ſub F G E recto, ideoque angulus ſub M L N rectus eſt. Q. E. D.

Fig. 53. *De Parab. Semicub.*] Si (in *Fig.* 53.) rectam lineam A B alia recta linea C D interſecat in puncto D cum lineâ A B angulum quemcunque conſtituens; & ſi ſumatur D E = ½ D C; deinde ducatur E F, ut D F ſit = D E; & denique diametro C F & vertice C deſcribatur parabola ſemicubica G C H, quæ tranſeat per punctum E, habeatque ordinatim applicatas ad diametrum C F lineæ F E parallelas: his poſitis, ſi parabola hæc ad lineam A B in ſitu inverſo deſcripta ſit, ut eandem in ſitu jam dicto deſcriptam interſecet, & contingentes ad punctum interſectionis ducantur, illæ contingentes ſe interſecabunt in angulo æquali angulo ſub C D B.

Sumatur in parabolâ G C H punctum quodvis I, ducatur I L C, & ſumptâ E M = E L ducatur M N C. Deinde ordinatim applicentur O I P, N Q R, ducaturque C E V, item E X diametro C O parallela. His poſitis, erit V X : X E :: E F : F C, & X E : X P ::

DF : EF. Unde ex æquo ut VX : XP :: DF : FC, dividendoque ut VX : VP :: DF : DC. Quoniam igitur eſt DF = DE = $\frac{1}{2}$ DC, eſt etiam VX = $\frac{1}{2}$ VP. Porro ut IO*q* : EF*q* :: CO*c* : CF*c* :: VO*c* : EF*c*. Quatuor igitur ratione continuatâ proportionalium eſt VO ſecunda, quarum IO eſt prima & EF ultima. Eſt autem & IO : OV :: LF : EF. Ideoque ſunt IO, OV, FL, FE quatuor ratione continuatâ proportionales; unde ut VO : LF :: LF : EF :: VO — LF : LE, componendoque ut LF + EF : EF :: VO — EF (= VX) : LE. Demonſtratum autem fuit VX æqualem eſſe dimidio lineæ VP. Ut igitur 2 LF + 2 EF : EF :: VP : LE :: 2 LFE + 2 EF*q* : EF*q*. Jam vero ut IO : LF (:: IV : LE) :: LF*q* (2 LFE + LE*q* — EF*q*) : EF*q*, propterea quod lineæ IO, VO, LF, EF ſunt quatuor ratione continuatâ proportionales; quoniam igitur ut VP : LE :: 2 LFE + 2 EF*q* : EF*q*, erit ut PI : LE :: 3 EF*q* — LE*q* : EF*q*. Eodem modo demonſtratur ut NR : EM :: 3 EF*q* — EM*q* : EF*q*. Cum igitur EM = ſit EL, erit NR = PI; ſunt autem parallelæ, ideoque puncta N, I æqualiter diſtant a lineâ AB. Si igitur parabola ſemicubica GCH in ſitu inverſo ad lineam AB deſcribatur, punctum N incidere poteſt in punctum I. Parabolæ huic detur ille ſitus inverſus *bcg*, & ducantur contingentes IΓS, EW, ΔNT, *t*Iδ; item lineæ WLY, WZM. Erit ex naturâ parabolæ hujus OS = $\frac{2}{3}$ OC, FW = $\frac{2}{3}$ FC, & QT = $\frac{2}{3}$ QC. Eſt autem & FD = $\frac{1}{3}$ FC; unde FD, DE, & DW ſunt æquales, & angulus ſub FEW rectus: &, cum EL ſit = EM, erunt & anguli ſub EWL, EWM æquales. Quoniam autem LMF lineis IO, NQ parallela eſt, & lineæ OC, FC, QC ſimiliter dividuntur in punctis S, W, T, erit WLY contingenti IΓS parallela, & WZM contingenti ΔNT. Eſt igitur angulus ſub WYD = angulo ſub IΓ*t*, & angulus ſub WZD = angulo ſub NΔD = angulo ſub I*t*Γ. Porro cum anguli ſub EWL, EWM ſint æquales, & anguli ſub DEW, DWE etiam æquales propter linearum DW, DE æqualitatem, erit angulus ſub YWD = angulo ſub WZD. Ideoque angulus ſub CDB, qui æqualis eſt ſummæ angulorum ſub WYD & ſub YWD, æqualis erit ſummæ angulorum ſub IΓ*t*, & ſub I*t*Γ, nimirum angulo ſub ΓIδ æqualis. Q. E. D.

XIII. *Accounts of Books ommitted.*

I. *Harmonia Menſurarum*; ſive Analyſis & Syntheſis per rationum & angulorum menſuras promotæ; accedunt alia opuſcula Mathematica: per *Rogerium Coteſium.* Edidit. & auxit ROBERTUS SMITH, Coll. Trin. Cantab. & Reg. Soc. Socius; Aſtronomiæ & Experimentalis Philoſophiæ Profeſſor. Cantabrigiæ 1722, in Quarto.

II. *Geometria Organica,* ſive Deſcriptio linearum curvarum Univerſalis. Auctore *Colino Maclaurin* Matheſeos in Collegio Novo Abredonenſi Profeſſore, & R. S. S.

The

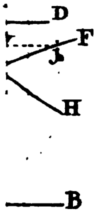


Fig. 35.

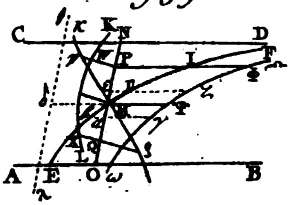
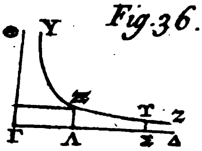


Fig. 36.



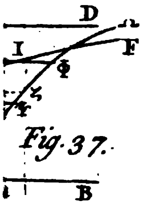


Fig. 37.

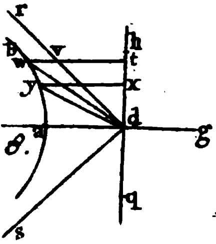


Fig. 38.

Fig. 39.

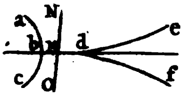


Fig. 40.

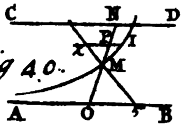


Fig. 4

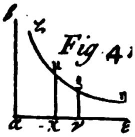
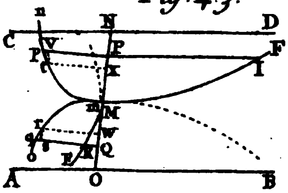


Fig. 43.



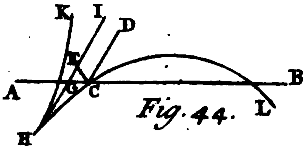


Fig. 44.

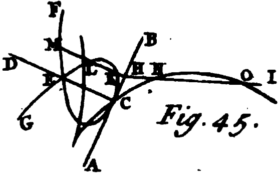


Fig. 45.

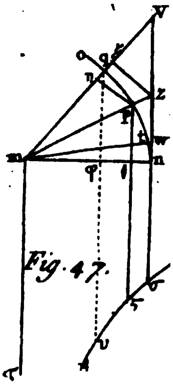


Fig. 47.

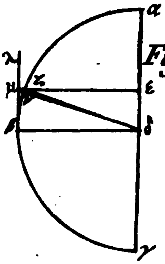


Fig. 48.

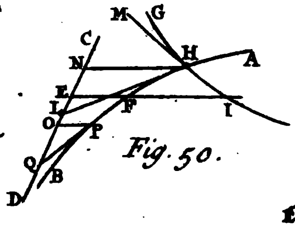
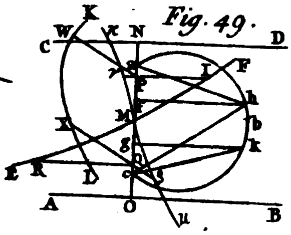
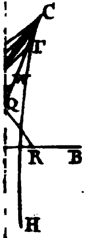


Fig. 50.

Fig. 49.





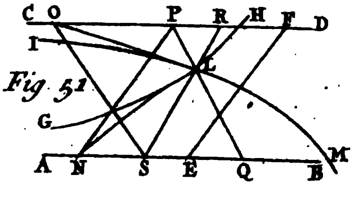
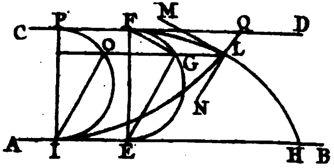


Fig. 51.

Fig. 52.



The Deſign of this Treatiſe is to examine various Methods propoſed by Mathematicians for deſcribing geometric Curves, and to demonſtrate a new one infinitely more General. 'Tis built upon the Theorems publiſh'd at the End of Sir *Iſaac Newton's* Enumeration of the Lines of the 3d Order. Theſe he demonſtrates in the 1ſt Section of the 2d Part, and in the laſt to ſhew the Uſe of Curves in Natural Philoſophy, ſolves two of the moſt conſiderable Problems that occur in it.

CHAP.

CHAP. II.

OPTICKS.

Experiments, occaſion'd by Sig. Rizzetti's *Opticks.* N° 406. p. 607. *by Dr.* Deſaguliers.

I. SIGNIOR *Rizzetti*, an *Italian* Gentleman, publiſhed a Book concerning the Affections of Light, in oppoſition to Sir *Iſaac Newton*, dedicated to Cardinal *Polignac*, 1727. In it he calls ſeveral of Sir *Iſaac Newton*'s Experiments into queſtion, becauſe they did not ſucceed in the Way he tried them, denies the Conſequences of others that he allows, and advances new Hypotheſes contrary to Experience. The Preſident being acquainted with this, deſir'd Dr. *Deſaguliers* to make ſome Experiments upon this Occaſion. How thoſe were made and ſucceeded, with Inferences from them, you have in the following Account.

N. B. Some of theſe Experiments are Sir *Iſaac*'s, but made after a different Manner; and ſome, as the Dr. informs us, altogether his own.

Fig. 54.

Experiment 1.] I prepared a Box of about three Foot high, and one Foot wide within (whoſe Shape was a truncated Pyramid) in the following Manner. I painted the Inſide of it black, and in the back Part, one Foot above the Baſe, made a ſquare Hole of three Inches in Width (whoſe Section is *r r*) to receive a Piece *R* ſhutting cloſe with a Rabbet or Shoulder, whoſe Surface coming through the Hole was wholly covered with the painted Paper, on which the Experiment was to be made. Over againſt *r r*, in the fore Part of the Box, was a Door to open with a Tube in it, four Inches wide and five Inches long, whoſe Section is *e*, *f*, *g*, *b*, that two Candles ſet on the Places *i*, *k*, to enlighten the Paper at *r r*, might throw no direct Light out of the Box, whoſe Section is repreſented at *a b c d*. Then having made the Room perfectly dark, I fixed the Box upon a Table, that it might remain in one Place; at the Diſtance of eight Foot from *r r*, I fixed the Lens L L, of four Foot Focus, in a Frame upon another Table, with its Axis going through the Middle of *r r*: At the Diſtance of about eight Foot beyond the Lens, I ſet up the Skreen or Square of white Paper S. Having put into the Hole *r r* a ſtiff Paper, painted with Vermillion, and wrapped four Times and an half with black Silk (as repreſented by R), that Paper enlighten'd by the Candles at *i*, *k*, the Image of the red Paper was projected upon the Skreen at *p*, and when the moſt diſtinct Place was found, the Skreen was fixed: Then a Paper painted with Ultramarine being fixed in the Hole *r r*, the Image of it was ſo indiſtinct at *e*, that the Images of the black Silks could not be ſeen; but holding a Piece of Paper cloſe to the Skreen, and bringing it forward, at about ¼ of an Inch from the Skreen, the Repreſentation of the Silks began to appear on the blue Image; but it was moſt diſtinct at an Inch and ¼, or at ZZ; ſo that there was 1¼ Inch between the diſtinct Baſe of the *red*, and that of the blue Paper. But what has led ſeveral People into an Error in making this nice Experiment, is the Depth of the Focus of the Rays in both Caſes; for though the red Image was moſt diſtinct at ϱ, yet the Repreſentation of the black Silks might juſt be perceived by a good Eye when the Skreen was moved backwards or forwards ¼ of an Inch: The blue Image which was ſtronger had its Silks viſible an Inch on either Side of ZZ; ſo that in a Paper half red and half blue, painted with theſe Colours, one might have ſeen the Silks (though faintly) upon the two Images at once, and have been thereby deceived: But ¼ of an Inch beyond the Place common to both, the *red* alone would have appeared diſtinct; and an Inch ſhort of the ſaid Place, the blue Image moſt diſtinct, and diſtinct alone; that is an Inch and ¼ nearer the Glaſs. Inſtead of *Vermilion* the red Paper may be painted with *Carmine* or *Lake*, but it will not do ſo well, as was then tried; nor does *Pruſſian Blue* do ſo well as *Ultramarine.* The beſt Way is to heighten the *Vermillion* with a little *Carmine*, and the *Ultramarine* (which has too much *white*) with *Indigo*; and then there will be a Space between the two diſtinct Baſes where both the Images will be indiſtinct.

N.B. *I made the Experiment with ſuch Colours, in the Year* 1722; *but now I uſed no Mixtures, that any Body elſe might repeat the Experiment.*

The 55th Figure repreſents the Box with one Side out, whoſe Place is *δ d b β*; *e g* is the Hole for the Tube in the Door of the Foreſide, *κ δ c d*; *r n* the Hole in the Back to receive the Piece R with its painted Paper. *Fig.* 55.

The 56th Figure is the Box open before, with the Candles and Paper in it, the ſame Parts being marked with the ſame Letters as in the other Figures.

N. B. *I made the Experiment in this Manner, becauſe Signior* Rizzetti *attributed the different* Foci *of the Colours to different Inclinations, which could not be alledged here; the* red *and* blue *being, as he had deſired, ſucceſſively fixed in the very ſame Place:* And he ſays, *p.* 64. *addidi permanentes colores a lumine directo diverſâ inclinatione illuſtratos conſtante Inclinatione in lentem incidere. Nay, more than this was performed in the Experiment; for as the Candles were fixed, the Light fell upon the painted Paper always with the ſame Incidence.*

Exper.

Fig. 54, 55. *Exper.* 2.] Inſtead of the red or blue Paper at r r (*Fig.* 54, 55.) I fixed upon the Piece R, a Paper half *red*, and half *blue*, as *Fig.* 57. R B (*Fig.* 57.) then over the Hole in the fore Part of the Box re- *Fig.* 55, 57. preſented by *e g* (*Fig.* 55.) I fixed a ſquare Plate *x δ c d* (*Fig.* 57.) with an oblong Hole in it four Inches long in its Horizontal Poſition, and one Inch deep, through which one might ſee the parti-coloured Paper, as if it was only of the Bigneſs and Figure of this Aperture, and ſtrongly enlighten'd by the Candles hid in the Box; the reſt of the Room being very dark.

N. B. *I made this Preparation, becauſe* Rizzetti *objects to Sir* Iſaac Newton'*s firſt Experiment of the firſt Book, that the black Cloth beyond the parti-coloured Paper was not colourleſs, and therefore the Experiment was not deciſive as particularly relating to the Paper.*

Fig. 58. R B (*Fig.* 58.) is the Paper contracted in Length and Breadth by the Aperture of the Plate, which Paper being looked at, at the Diſtance of five Foot, by the Priſm 1, appeared as drawn at r b. The Priſm being removed to 2, at the Diſtance of ten Foot, ſhewed the Paper as at *r b*. And when it was at 3 (at the Diſtance of fifteen Foot) the Paper appeared as ϱ β. In theſe three Caſes the blue b, *b*, and β appeared lower than the red r, *r*, ϱ, the refracting Angle of the Priſm being downwards. When the refracting Angle was held upwards, as at 5, then the *blue* B was raiſed higher than the *red* R; but if due Care be not taken, in turning the Priſm, a Reflection may be miſtaken for a Refraction, as at 4; and then indeed the *Red* and *Blue* will be equally raiſed as at T. This muſt have been Signior *Rizzetti*'s Miſtake, when (in *Pag.* 38.) he ſays that one Colour was raiſed higher than the other by two Lines, at ten Foot Diſtance, but not at all at five Foot; for ſeveral of the Perſons preſent at my Experiments, made the ſame Miſtake at firſt before they could perform the Experiment in manner abovementioned; which they at laſt did, and found the Colours ſeparated moſt at the greateſt, and leaſt at the leaſt Diſtance.

This miſtaking a Reflection for a Refraction, has been the Occaſion of ſeveral more Errors, and Difficulties to be met with in Signior Rizzetti'*s Book.*

Exper. 3.] A Candle K, reflected from the Surface A B of the *Fig.* 59. Priſm A B C, appeared very faintly to the Eye at E, as a weak Image at k; becauſe the Rays incident at I, paſs moſt of them through the Priſm, and go on to R, ſeparating from one another according to their different Degrees of Refrangibility; whilſt a few of them are reflected to the Eye in the Direction I E.

But if the Priſm be in the Poſition A C B (*Fig.* 60.) moſt of the *Fig.* 60. Rays of the Candle K, incident at I, on the Plane A B (after having paſſed perpendicularly through the Plane B C) are reflected, and paſſing perpendicularly through A C, go into the Eye at E, which ſees a very ſtrong Image of the Candle at k, whilſt very few Rays go down to R to produce Colours.

Corol.] *This ſhews that the Rays of Light paſs with more Facility through Glaſs (a denſe) than through the Air (a rare) Medium; contrary to* Rizzetti's *Aſſertion.*

Exper. 4.] To make this more evident, and compare together the Facilities with which Light paſſes through the two Mediums, I took a Cube of Glaſs of three Inches the Side, A *a b* B *d* D C, whoſe Section is A B C D, and looking upon it from E to ſee by Reflection the Candle K, I ſaw two Images of it; one at *k* *Fig.* 61. very faint, and reflected from the upper Surface A B, and the other at κ very ſtrong, and reflected from the lower Surface C D. Now it is evident, that the Vividneſs (or Brightneſs) of the Image κ, is to the Vividneſs of the Image *k*; as the Facility with which the Rays in theſe Circumſtances paſs through the Glaſs, or through the Air: And thoſe are eaſily compared, becauſe both the Images are ſeen at once.

Exper. 5.] The Line P I being perpendicular to the reflecting Plane A B of the Triangle A C B, I brought the Candle K by Degrees ſo near to P, as to diminiſh very much the Angle of Incidence K I P, which made the Image or Appearance of the Candle *Fig.* 62. at *k*, become fainter by Degrees, and at laſt as faint as in *Fig.* 59. *Fig.* 59.

Exper. 6.] Having made the Experiment as at *Fig.* 60, I preſſed *Fig.* 60. another Priſm D F G, cloſe to the Priſm A B C, and when I ſqueezed them together but gently, ſome of the Rays from the Candle R, paſſed through the lower Priſm, and falling upon a Paper at R, made a reddiſh Spot; but when I ſqueezed them very hard, the Spot became much wider, white in the Middle, and only tinged with Red about the Edges: At the ſame Time the Eye ſaw a black Spot in the Image of the Candle at *k*; and a Stander-by looking obliquely at the Place I (where the Glaſſes touched) ſaw, as it were, a little Hole through the Priſms as big as the Spot *k*. But if the Priſms be preſſed together but gently, then all the other Phænomena diſappear, except the firſt little Spot at R, as in *Fig.* 64. *Fig.* 64.

When the Candle is ſeen by Reflection from the lower Surface of a Priſm, as in the 60th, 62d and 63d Figures, the Rays paſs quite *Fig.* 60, 62, 63. through that Surface, and are turned up again by the Attraction of it in Curve Lines ſo as to re-enter the Priſm, and then (going out again through the Surface A C) go up to the Eye at E. In this Caſe the moſt refrangible Rays, being the moſt eaſily inflected, make the leaſt Curves, whoſe Vertices are nearer the Glaſs than thoſe of the greater Curves made by the leaſt refrangible Rays.

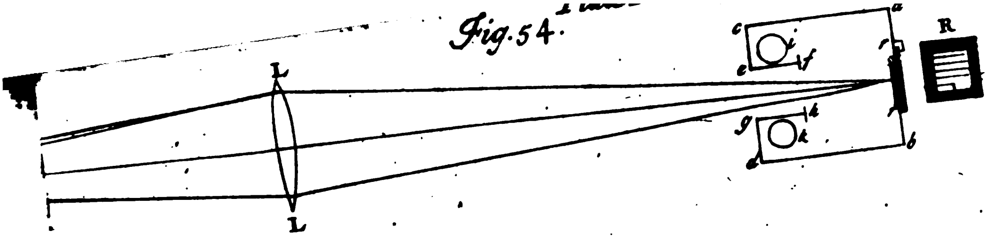
This is proved by Experiment 6, where the under Priſm only attracts down from the Reflection of the upper Priſm, the Red making Rays as in *Fig.* 64. where the Plate of Air between the Priſms is of ſome ſmall Thickneſs. But when the Priſms, whoſe Surfaces are a little convex, are preſſed hard together, the lower Priſm is near enough to attract Rays of a great Degree of Refrangibility; and therefore the Spot then becomes white in the Middle, and only red about the Edges, which are produced by ſuch Parts of the lower Priſm as are not ſo near the upper Priſm. *Fig.* 64.

There are two Circumſtances in the 6th Experiment, which diſprove *Rizzetti*'s Aſſertion (Page 125) viz. *That there is a ſenſible Reflection even where Glaſſes touch*; for when the Priſms touch at I *Fig.* 63, the black Spot appearing in the Image of the Candle *k* ſhews that there is at I a Deficiency of thoſe Rays, which, coming from the Middle of the Candle, uſed to be reflected up to the Eye at E, and therefore that A B the reflecting Surface of the upper Priſm ceaſes to reflect in a little Space round about I where the upper Surface D F of the under Priſm touches it; the Rays, which before were reflected, now going down to make the Spot at R. The other Circumſtance is this; that whereas a Paper *k* is inviſible to an Eye at E by the Interpoſition of the Priſm D F G; when another Priſm A C B is laid over it and preſſed hard, there appears to be an Hole of about $\frac{1}{6}$ of an Inch (more or leſs in Diameter as the priſmatical Surfaces are more or leſs flat) thro' which the Paper at *k* becomes viſible; this being the Place of Contact where the Reflection downwards (of the Surface D F) ceaſes. *Fig.* 63.

This happens becauſe thoſe Rays, which (coming from the Candle K) were bent in Curves under the Surface A B of the upper Priſm about ſeveral Points near I, are by the Nearneſs of the Surface D F of the lower Priſm brought down to R, inſtead of being turned up again to the Eye at E; whilſt thoſe Rays, which (coming from the Paper at *k* thro' the Surface G F of the lower Priſm, and paſſing thro' the upper Surface of it, F D) were bent in Curves about ſeveral Points near I, are prevented from turning down again to R, and are brought up to the Eye at E, which conſequently muſt ſee a round Part of the Paper at *k*, juſt as big as the Place of Contact, which appears like an Hole; or as if the two Priſms being changed to a Parallelopiped, were covered with a dark Paper that had only a ſmall Hole in it.

But to make this more evident, eſpecially to ſuch as are not well acquainted with Sir *Iſaac Newton*'s Opticks, I beg Leave to explain the Manner of the Bending of Rays, where they are refracted or reflected.

Fig. 54.



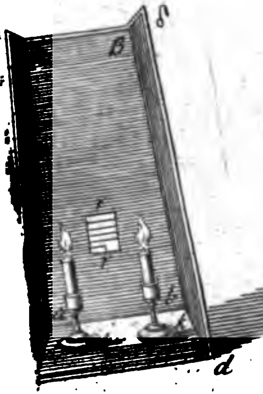


Fig. 57.

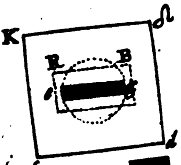


Fig. 58.

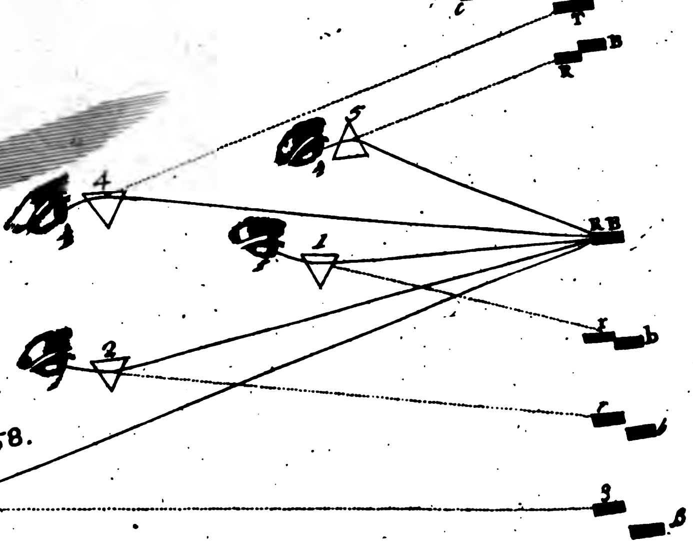


Fig. 60.

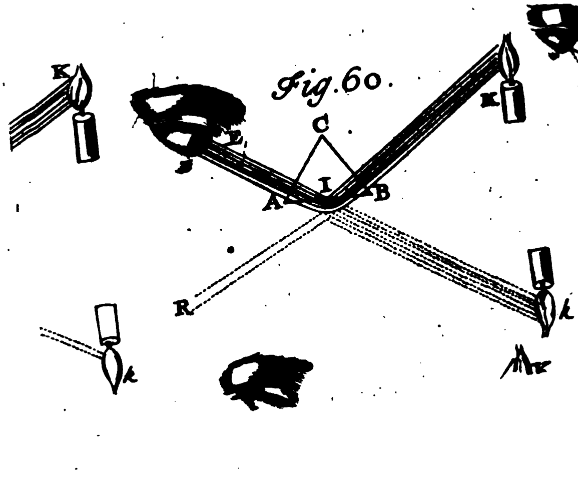
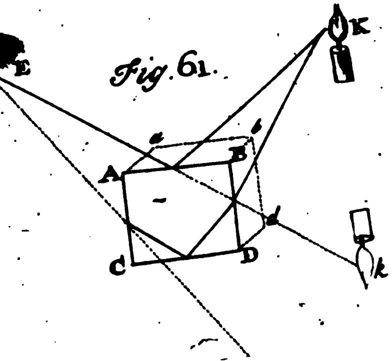


Fig. 61.



Of the Bending of the Rays in their Refraction.

Fig. 65.

Schol. 1.] Let D D (*Fig.* 65.) repreſent a denſe Medium (as Glaſs) whoſe Surface is G G, and A A a rare Medium (as Air). Now let us ſuppoſe a Power to extend all over the Surface G G, acting from A A towards D D in Lines perpendicular to the Surface G G, very ſtrong in Contact, but inſenſible at a very ſmall Diſtance from the ſaid Surface, which we will call the *Attraction of the Surface* G G, without conſidering whether it be any real Virtue in the ſaid Surface, or the Action of a Medium impelling towards it. Let Lines 11, 22, 33, ſuch as expreſs the Lines in which the Attraction exerts it ſelf, and the Line M M (extremely near to G G) the Limits of the Attraction, beyond which it cannot affect a Ray of Light. Let the Ray of Light R *a* moving from a rare Medium into a denſe in the Direction R r, come towards the Surface G G in ſuch an Angle that it may be refracted. When the Ray comes to *a*, by the Attraction at *a* it will be acted upon in the Line *a* b, and (by the known Laws of Mechanicks) be turned out of the Way into the Direction *a* a, inſtead of *a* r: When it is got to *b*, being acted upon in the Direction *b* 4, its new Direction will become *b* b: At *c*, by the Power acting in the Line *c* 5, it will change its Direction to *c* c; and laſtly, at *d* it will go into the Glaſs in the Line *d* d, continuing in that ſtreight Line whilſt it moves in that Medium.

Now if the Lines 11, 22, 33, *n*, *c*, *b*, *a*, be infinitely near (as they muſt be ſuppoſed to be) the Ray, inſtead of being broken into the ſeveral ſtreight Lines *a b*, *b c*, and *c d*, will be bent into the Curve *a b c d*; and the emergent Ray *d* d will make the ſame Angle with the Incident Ray R r as if the Refraction had been made at once at the Point *n*, which Point may be conſidered as in the Surface G G, becauſe M M has been ſuppoſed extremely near that Surface: Then alſo may Refractions be conſidered in groſs, and Rays trac'd, in all Optical Propoſitions, as if there were no ſuch Curve as what we have been deſcribing.

Fig. 66.

Again, let D (*Fig.* 66.) repreſent the denſe Medium or Glaſs, and A the rare Medium or Air; R *a* a Ray of Light coming out of the denſe Medium into the rare, in the Direction R *r*, in which it may be refracted (as for Example, in an Angle of 30 Degrees with the Perpendicular P *a*). Let M M be the Line which limits the Attraction of the Surface G G, which Attraction is exerted in Lines tending perpendicularly from M M to G G. As ſoon as the Ray of Light hath emerged at *a*, it is attracted in the Direction *a* P, and therefore diverted from the Line *a* r, into the new Direction *a* a; at *b*, it is turned into the Line *b* b; at *c*, into the Line *c* c; and at *d*, into the Line *d* d; ſo that the emergent Ray will be *d* d, as if the Refraction had been performed in the Point *n*, and that Point was in the Surface G G, without any Curve at *a b c d*; and all the reſt as we conſidered it before, with this Difference only, *viz.* That the Ray is bent juſt as it comes out (or rather when it is come out) of the denſe Medium; whereas before we conſidered its Bending before it came into it.

Of the Bending of Rays in Reflection.

Fig. 67. *Schol.* 2.] But if the Ray R *a* (*Fig.* 67.) coming out of Glaſs into Air, ſhould come in ſuch a Direction as to be wholly reflected, as it will do when the Angle R *a* P is of 45 Degrees; I ſay the Reflection will not be made at the Surface G G, nor above it in the Glaſs; but under the ſaid Surface, in the Air, or even in a Vacuum, or any Medium leſs denſe, or rather leſs refractive than Glaſs.

M M repreſents the Limits of the Attraction of the Glaſs exerted in a Direction from M M to G G perpendicularly, as we ſaid before.

The Ray R *a*, moving in the Direction R r, at its Emerſion at *a*, is, for the Reaſons before given, turned into the Direction *a* a; then at *b*, into the Direction *b* b; at *c*, into the Direction *c* c; at *d*, into the Direction *d* d; at *e*, into the Direction *e* e; and at *f*, into the Direction *f* f parallel to G G; then at *g*, the Ray is again turned towards the Glaſs, by whoſe Attraction changing ſucceſſively into all the Directions *g* g, *i* i, *k* k, and *l* l; at laſt it re-enters the Glaſs in the Direction *m* m making the ſame Angle with the Perpendicular *m* p that R *a* made with *a* P. Now as the Lines perpendicular to G G drawn from M M are infinitely near, the Line *a b c d e f g h i k l m* muſt be a Curve; and as M M and G G are extremely near, the Vertex of the Curve (whoſe Tangent is *f* f parallel to G G) will be ſo near the Point I, as to be conſidered as co-inciding with it, when we compare the Angle of Incidence with that of Reflection; then alſo will the Space between the Parallels p *m* and P *a* be ſo far diminiſhed, that thoſe two Lines may be looked upon as co-inciding, the Angles m *m* p and R *a* P being equal, whether the three Points *m*, I, *a*, co-incide or not.

For theſe Reaſons, for common Uſe, one may conſider the Reflection from the under Surface of the Glaſs as made at once in that Surface at the Point I. But when we examine Things ſtrictly, Experiments as well as the above Reaſoning, will ſhew, that there is ſuch a Curve as we have mentioned. *See* Experiment VI. *Fig.* 63,
Fig. 63, 64: and 64: and we ſhall mention others.

N. B. If any Point of the Curve *a b c*, &c. between *a* and *f*, fall below (or beyond the Line M M,) the Ray will then go on in a ſtraight Line Tangent to the Curve in that Point where it leaves the Line M M.

Now let us ſuppoſe M e d c b a r M (in the ſame *Fig.*) to be Glaſs; or any other denſe Medium, and m p P R Air, or any other rare Medium, and R *a* a Ray of Light moving in the rare Medium towards the denſe Medium in the Direction R *a* towards r; if inſtead of an Attraction at the Surface of the Glaſs M M, there be ſuppoſed a repellent Force, whoſe Limits are G G; then will the Ray by the Repulſion of the Surface M M be bent into the Curve *a b c d e f g h i k l m* in the ſame Manner as we ſhewed it would be under the Surface G G, when G p P G was conſidered as a denſe Medium. Hence it follows that a Ray moving in the Air, is reflected from a ſpecular Surface of Glaſs, or any other Mirrour, opake or diaphanous, without touching the ſaid Surface.

N. B. *That the ſame Power may, under different Circumſtances, attract to and repel from the ſame Surface, ſhall be made out in the remaining Part of this Paper; but now taking ſuch a Power for granted, we will proceed in conſidering the* Flexure *of Rays of Light.*

Let us ſuppoſe a Priſm A C B (*Fig.* 68.) to have the attracting Power of its inferior Surface extend as far as the Line *m m*; if another Priſm G D F (the attracting Force of whoſe upper Surface extends as far as *n n*) be brought very near to the firſt Priſm; where the attracting Powers of the Priſms interfere, they will deſtroy one another, becauſe they act in contrary Directions; and thereby the Limits of Attraction of each of the Surfaces will be contracted; the Power of A B extending no farther than *n n*, and that of D F no farther than *m m*, whilſt the Space *n n m m* loſes all the Force that it had (and would have upon the Removal of either Priſm) to turn a Ray of Light, moving obliquely, out of its Direction. *Fig.* 68.

Now in this Situation of the Priſms, a Ray of Light entring the Surface C B at right Angles, will go through the ſecond Priſm alſo at right Angles (not exactly in the ſame Line, but) in a Line parallel to the Direction of the incident Ray: For Example, let the Ray R *a* (not refracted at, becauſe perpendicular to, the Surface C B) emerge from the firſt Priſm at *a*, in the Direction *a r*; its changed Direction at *a* will become *a* a, and at *b*, *b* b, or rather the Ray will be inflected in the Curve *a b*; and at *b* getting out of the Power of the Attraction of the Surface A B, it will (for the Reaſons before given) move in a ſtraight Line from *b* to *c*, where it will be bent again the contrary Way in the Curve *c d* of the ſame kind as *a b*, and laſtly emerge in the Direction *d* d parallel to the firſt Direction R r. From hence it follows, that when the Priſms are brought ſo near as to touch, their mutual Attractions deſtroying each other, the Rays of Light will not be bent, but paſs through the two Priſms (which in this Caſe perform the Office of a Parallelopiped) in the ſame Direction with which they came into the firſt Priſm, and conſequently produce no Colours; contrary to what is affirmed by *Rizzetti* (*Page* 78, 79, &c.)

and when the Rays R *a* fall obliquely upon the Surface C B, the Effect of their Refraction at their Immerſion at S to produce Colours, is taken off by the Refraction which they ſuffer at their Emerſion at z.

Fig. 61. *Exper.* 7.] I took the Cube of *Fig.* 61. and looking obliquely thro' it at the Hole of the Window of my dark Chamber (the Sun ſhining or not ſhining) the Hole appeared entirely colourleſs, as did alſo a Candle, both appearing fringed with Colours when ſeen through the Priſm. Then holding two Priſms together, as in *Fig.* 63. if the Hole of the dark Chamber be at k, it appears white to the Eye at E; but if the Angles of the Priſms at B F be a little ſeparated, whilſt the Points A D touch, the Hole will appear coloured: When the Surfaces are ſeparated at A D, and touch at B F, the Colours appear in an inverted Order; but if the Surfaces A D and B F are parallel, whether they touch or not, the Hole will appear white. Fig. 63.

N. B. *In this Caſe the Priſms muſt be ſimilar, that the Surface* F G *may be parallel to* A C; *otherwiſe* A B *and* D F *muſt be ſo inclined to one another as to render* A C *and* F G *parallel. Indeed if one of the Priſms be very far removed from the other, the beterogeneous Light which entered in at* F G, *may be ſo far ſpread by the Separation of the differently refrangible Rays, that the Priſm* A B C *will not take it all in; then the Eye behind the ſecond Priſm may ſee Colours, as I ſuppoſe* Rizzetti *did.* See *Page* 79 of his Book.

Fig. 68. If the Ray of Light R *a b c d* d (*Fig.* 68.) changing its Direction in the Manner above-mentioned, makes an Angle of about 45 Degrees with the Perpendicular P *a*; upon the removal of the lower Priſm, the Ray will be turned up again, as in *Fig* 67. But if the Angle P *a* R be greater, the Ray will ſtill be turned up again in a Curve, as *a b c d e f*, *Fig.* 59. notwithſtanding the lower Priſm is at D F G; but if that Priſm be brought up cloſer to the Surface A B, the Curves will be deſtroyed where the Priſms touch, and all the Rays in the Place of Contact brought down through the lower Priſm. Fig. 67. Fig. 59.

The moſt refrangible Rays conſiſt of ſmaller Particles than the leaſt refrangible Rays, and therefore muſt have leaſt *Momentum*, the Velocity of all the Rays being the ſame; and conſequently are more eaſily turned out of the Way by Attraction or Repulſion, which makes the Curves made by the purple and violet Rays under the Surface A B, to be leſs and nearer the ſaid Surface than the Curves made by red and orange Rays.

Fig. 69. Suppoſe a Violet R *a* moving in the Direction R r, *Fig.* 69. to be ſo bent under the Surface A B, that at the Vertex of the Curve, or where its Tangent *c* c is parallel to A B, there ſtill remains a ſmall Space between the Curve and the Line *n n*, where the Limits of Attraction (contracted by the Proximity of the undermoſt Priſm

D F G

DF G end) that Ray will be turned up again in the Curve *d e f*, and ſo reflected in the Line *f f*, the Directions having been ſucceſſively changed, as in *Fig.* 67. *Fig.* 67. But a red Ray with the ſame Inclination, would paſs on into the lower Priſm, as was explained in *Fig.* 68. *Fig.* 68. Becauſe the Momentum of the red Ray being greater than that of the violet, the ſame Degree of Attraction could not give it the ſame Flexure.

This is confirmed by Experiment; for when the lower Priſm is not preſſed hard againſt the upper (as in *Fig.* 64.) *Fig.* 64. the Rays brought down to R make a Spot of a Colour made up chiefly of red and orange Rays; but when the Priſms are preſſed cloſer, the Spot grows bigger and perfectly white in its Middle, becauſe all Sorts of Rays are brought down to the Spot; but it is incloſed round with a reddiſh Border, occaſioned by the Parts of the Priſm which are very near, but not in Contact, or at leaſt not near enough to bring down the green, blue, purple and violet Rays.

This ſhews that the Reflection is not made from the interior ſolid Parts of the Glaſs, nor from the Parts in the Surface, as Rizzetti *affirms. But this is made more evident by*

Exper. 8.] A Candle being in the Poſition K, the Eye at E, and *Fig.* 70. the Priſm at A B C; a ſtrong Image of the Candle was ſeen at k as in *Fig.* 60. *Fig.* 60. But lifting up a Veſſel of Water V S S V till the Surface of Water V V touched A B the lower Surface of the Priſm, the Image of the Candle became almoſt inſenſible, as the Eye loſt all thoſe Rays which now were attracted into the Water. And for a farther Proof, that the Reflection is made under the Surface and not in it, when the Priſm was taken out of the Water, being wet at its lower Surface, or having a *Stratum* of Water (whoſe Surface was V V *Fig.* 71.) *Fig.* 71. under A B, the Image of the Candle did again become vivid, the Rays being turned up again under V V. Indeed the Image, in this Caſe, though ſtrong, did not appear well defined, by Reaſon of the Unevenneſs of the watry Surface V V *Fig.* 71.

I am very well aware that *Rizzetti* may anſwer here, that what I have ſaid above, does in ſome Meaſure favour his Notions; and that the Rays which (in *Fig.* 60. *Fig.* 60. having paſſed through A B, the lower Surface of the Priſm) are turned up again to the Eye at E, do not ſuffer a Reflection but a new Immerſion; for he ſays, in *Page* 125.—— " Anglus (meaning Sir *Iſaac Newton*) ſecundo ſub-
" jungit, *quod ſi lumen in tranſitu è vitro in aerem obliquiùs incidat,*
" *quam in angulo graduum* 40, *illud in totum reflectitur.* Ego verò
" reſpondeo, quod ex iis, quæ docui in *Prop.* 4. *Cap.* 1. elicitur
" hanc non eſſe veram luminis reflectionem, ſed potius novam
" Immerſionem; & ideo nego quod ex iſto Phænomeno ſequatur
" lumen a partibus corporum ſolidis, aliquo interjecto intervallo,
" reflecti." And a little lower, having quoted what Sir *Iſaac*

Newton ſays, concerning the blue Light, which coming from one Priſm obliquely upon the farther Surface of another, is wholly reflected, at the ſame Inclination that the red Light is wholly tranſmitted. — He ſays, " Satis ſit iterum reſpondere, quod in hoc etiam " caſu eſt nova luminis immerſio, quæ dicitur ab auctore reflectio.

But this is only cavilling about Words; for if the Ray of Light, which moving in a denſe Medium falls obliquely on the Surface common to that and a rarer Medium, be turned back again in the denſe Medium, ſo as to make the Angle in which it returns from the ſaid Surface equal to that in which it came to it; this Return of the Ray may properly be called a *Reflection*, whether the Ray be turned back at the Point of the Incidence in the Surface, or be carried about the Point of Incidence in a ſmall Curve, whoſe Conſideration may be omitted in tracing the Way of a Ray of Light in its Paſſage, for making of optical Machines. Whoever reads the 8th *Prop.* of the 2d *Part*, Book II. of Sir *Iſaac*'s Opticks, may very eaſily find that he was not ignorant of the turning back of the Ray under the Surface of the Glaſs before it returned into it: And though the Reflection in that Caſe be not made by impinging on the ſolid Parts of the Glaſs, yet it is owing to them that the Light (acted upon at a Diſtance) is turned up again, as has been ſhewn by ſeveral of the Experiments abovementioned.

Now let us ſee how *Rizzetti*'s Account of the new Immerſion agrees with *Phænomena.*

Fig. 72. Let all above the Line P p (*Fig.* 72.) be a denſe Medium, as Glaſs; and all below it a rare Medium, as Air; A B C D is a Beam of Light inſenſible in Thickneſs, but of ſome Breadth, whoſe Rays cohere to one another, and whoſe Section or firſt Line is B C. If the Medium in which B C is, did not change, B C would move parallel to it ſelf in the Lines B *a* and C *d*; but as the End C of the Line B C comes out into a rare Medium, which being of leſs Reſiſtance to Light (for ſo he ſuppoſes) the Point C moving with more Facility than the Point B deſcribes the Curve C F H, whilſt B moving in the denſe Medium with more Difficulty, deſcribes the leſſer Curve B E G; then the Point C being got to H is re-immerſed, and the Line B C being got to H G goes on in the Direction H K G L parallel to itſelf, drawing the Beam after it in a rectilinear Direction, after Part of it has been bent within the Glaſs and Part of it without.

Now if this be true, and P p π be a Priſm, I beg to know what becomes of the Line at E F which unites the Rays of the Beam about the Point of Incidence I, when Water is brought to touch the Surface P p, as at A B *Fig.* 70? If it be ſaid that Water *Fig.* 70. making a great Reſiſtance, though not ſo great as Glaſs, the Curve B E G deviates ſo little from the Line B *a* that the Point E comes below I, and the Beam is wholly refracted; I aſk whence comes the faint Image at k? If it be anſwered, that ſome Part E I of the Line E F *Fig.* 72. is turned up to the Eye at E (*Fig.* 70.) *Fig.* 72, 70. what becomes of the lateral Coheſion of Light on which *Rizzetti* founds his chief Propoſition, and from which he draws his Conſequences?

It would be tedious as well as uſeleſs to be particular in ſhewing all *Rizzetti*'s Miſtakes; therefore I ſhall only mention one more Experiment from Sir *Iſaac Newton*, which I repeated on Account of what is ſaid in *Rizzetti*'s Preface, *Page* 16, *viz. that if (according to Sir* Iſaac) *Rays were differently reflexible, Colours muſt be produced by Reflection from a plane Surface*; *but this*, ſays our Author, *is contrary to Experience.* Now this his Aſſertion is diſproved by

Exper. 9.] As this Experiment was made exactly in Sir *Iſaac Newton*'s Manner, and with the ſame Succeſs, I refer you to his own Account. *Vid. in Book* I. *Part* II. *Exper.* 16.

If this Account needs any farther Explanation, let us ſuppoſe C A B the Section of the Priſm in *Fig.* 13 of Sir *Iſaac Newton*, transferred to *Fig.* 73. at A C B. *Fig.* 73. If R *o* be a red Ray inclined to a Perpendicular to A B in an Angle of more than 41 or 42 Degrees, it will at its Emerſion under the Surface A B be turned into the Curve *o n m i*, and ſo go up again to the Eye at E; but another red Ray coming in the Direction *r n* making an Angle with the Perpendicular ſufficiently leſs, will after its Emerſion at *n*, be only bent ſo much as to be turned out of the Way, and refracted to *q*, in an Angle of Refraction agreeable to the Refrangibility of red Light. But V *m* a violet Ray with the ſame Inclination as the laſt red one *r n* ſhall not be refracted, but turned up in the Curve *m i* P, and ſo go to the Eye at E. Another violet Ray v *m* making an Angle ſomething leſs with the Perpendicular, will paſs through the Glaſs, and be refracted in the Line *m* S. Upon this Account all that Part of the Baſe of the Priſm (of which A B is the Section) between A and *p* will be dark or faint, all that Part between *p* and *n* be tinged with a bluiſh Colour, and all between *o* and B of a bright White.

P. S.] The Bending of Rays of Light juſt as they come to be reflected or refracted, may be eaſily underſtood by ſuch as are well acquainted with thoſe Properties of Light, which Sir *Iſaac Newton* calls their *Fits of eaſy Reflection*, and *Fits of eaſy Tranſmiſſion*; without any Hypotheſis, by Conſequences fairly drawn from Experiments and Obſervations. But as Signior *Rizzetti* does not ſeem to have the leaſt Notion of thoſe Properties of Light, and the nice Obſervations on which they are founded; and ſeveral other Perſons have not Time to read thoſe Parts of the Opticks with ſufficient Application; to ſhew how the ſame Power of the Surface of a denſe Medium may both attract and repel under different Circumſtances —— I content myſelf here with giving the Hypotheſis, which Sir *Iſaac* does before he comes to that Part of his Book where he demonſtrates the Fits abovementioned.

If G G be the Surface of a denſe Medium G D D G, on which a Tremor is cauſed by the Warmth communicated to it by the Rays of Light, ſo as to give a Wave-like Motion to the Medium immediately next to the Surface G G; as that vibratory Motion is performed, the Medium alternately puſhes from the Surface, and returns towards it (as is repreſented by the Poſition of the Darts in the Figure,) and puſhes back the Light, ſo as to reflect it when the Vibration is contrary to its Direction, but brings it down to be refracted when the Vibration conſpires with the ſame Motion.

See Book II. *Part* III. *Prop.* 12. *of Sir* Iſaac Newton'*s Opticks*.

Of coloured Arches within the common Rainbow, by Dr. Langwith. N°375. *p.* 241.

II. When the Primary Rainbow has been very vivid, I have obſerv'd in it, more than once, a ſecond Series of Colours within, contiguous to the firſt, but far weaker, and ſometimes a faint Appearance even of a third. Theſe increaſe the Rainbow to a Breadth much exceeding what has hitherto been determin'd by Calculation. I remember, I had once an Opportunity of making an ingenious Friend take notice of this Appearance, who was much ſurpriz'd at it, as thinking it not to be reconciled with the Theory.

Pag. 242. Since my laſt, I have obſerv'd ſomething of the ſame Nature, though not in the ſame degree of Perfection, the loweſt Arch of a faint Red inclining to Purple, appear'd and vaniſh'd ſeveral times while I was obſerving it.

Pag. 243. I begin now to imagine, that the Rainbow ſeldom appears very lively without ſomething of this Nature, and that the ſuppoſed exact Agreement between the Colours of the Rainbow, and thoſe of the Priſm, is the reaſon that it has been ſo little obſerv'd.

I have ſeen thoſe Phænomena in ſuch perfection lately, that *Pag.* 244. I can't help being particular in my Account of it.

Auguſt the 21ſt, 1722, about half an hour paſt 5 in the Evening, Weather temperate, Wind at N. E. the Appearance was as follows, *viz.*

The Colours of the Primary Rainbow were as uſual, only the purple very much inclining to red, and well defin'd: Under this was an Arch of green, the upper Part of which inclin'd to a bright yellow, the lower to a more dusky green: Under this were alternately two Arches of reddiſh purple and two of green: Under all a faint Appearance of another Arch of purple, which vaniſh'd and return'd ſeveral times ſo quick, that we cou'd not ſteadily fix our Eyes upon it. Thus the Order of the Colours was

I. Red, Orange-Colour, Yellow, Green, Light-Blue, Deep-Blue, Purple.

II. Light-Green, Dark-Green, Purple.

III.

Fig. 66.

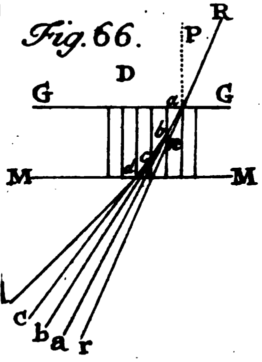
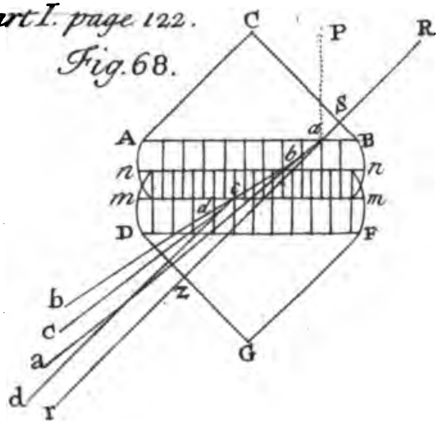


Fig. 68.



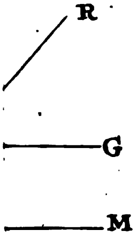


Fig. 69.

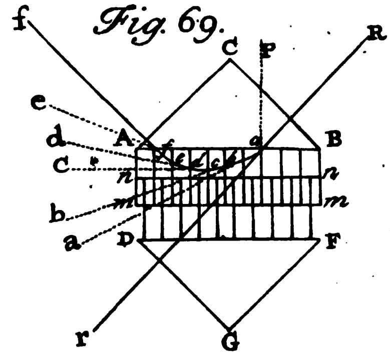


Fig. 71.

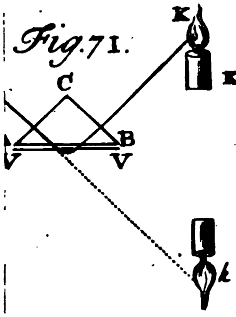


Fig. 72.

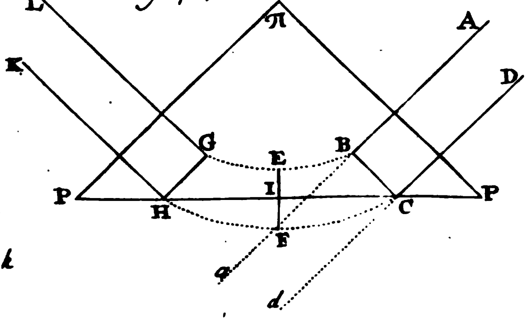


Fig. 73.

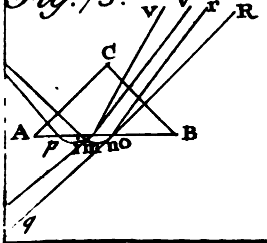
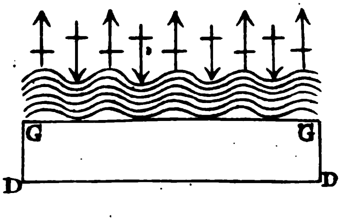


Fig. 74.



III. Green, Purple.

IV. Green, faint vaniſhing Purple.

You ſee we had here four Orders of Colours, and perhaps the beginning of a fifth, for I make no queſtion but that what I call the Purple, is a Mixture of the Purple of each of the upper Series with the Red of the next below it, and the Green a Mixture of the intermediate Colours. I ſend you not this Account barely upon the Credit of my own Eyes; for there was a Clergyman and four other Gentlemen in Company, whom I deſir'd to view the Colours attentively, who all agreed, that they appear'd in the manner that I have now deſcrib'd.

There are two things, which well deſerve to be taken notice of, as they may perhaps direct us in ſome meaſure to the Solution of this curious Phænomenon.

The 1ſt is,

That the Breadth of the firſt Series ſo far exceeded that of any of the reſt, that as near as I could judge, it was equal to them all taken together.

The 2d is,

That I have never obſerv'd theſe inner Orders of Colours in the lower Parts of the Rainbow, tho' they have often been incomparably more vivid than the upper Parts, under which the Colours have appear'd. I have taken notice of this ſo very often, that I can hardly look upon it to be accidental, and if it ſhould prove true in general, it will bring the Diſquiſition into a narrow Compaſs; for it will ſhew that this Effect depends upon ſome Property, which the Drops retain, whilſt they are in the upper part of the Air, but loſe as they come lower, and are more mix'd with one another.

The abovementioned Appearances accounted for; by Dr. Pemberton. N°375.*p.*245.

III. Let A B repreſent a Drop of Rain, B the Point from whence the Rays of any determinate Species being reflected to C, and afterwards emerging in the Line C D, do proceed to the Eye, and cauſe the Appearance of that Colour in the Rainbow, which appertains to this Species. It is obſerved by Sir *Iſaac Newton**, that in the Reflection of Light, beſides what is reflected regularly, ſome ſmall part of it is irregularly ſcattered every way. So that from the Point B, beſides the Rays that are regularly reflected from B to C, ſome ſcattered Rays will return in other Lines, as in B E, B F, B G, B H, on each Side the Line B C. Further it muſt be noted from Sir *Iſaac Newton* †, that the Rays of Light in their

Fig. 75.

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* Optics, *Book* II. *Part* 4.

† Ibid. *Part* III. *Prop.* xii.

Paſſage from one Superficies of a refracting *Medium* to the other undergo alternate Fits of eaſy Tranſmiſſion and Reflection, ſucceeding each other at equal Intervals; inſomuch that if they reach the further Superficies in one ſort of thoſe Fits, they ſhall be tranſmitted; if in the other kind of them, they ſhall rather be reflected back. Whence the Rays that proceed from B to C, and emerge in the Line C D, being in a Fit of eaſy Tranſmiſſion, the ſcattered Rays that fall at a ſmall Diſtance without theſe on either ſide, (ſuppoſe the Rays, that paſs in the Lines B E, B G) ſhall fall on the Surface in a Fit of eaſy Reflection, and ſhall not emerge; but the ſcattered Rays, that paſs at ſome Diſtance without theſe laſt, ſhall arrive at the Surface of the Drop in a Fit of eaſy Tranſmiſſion, and break through that Surface. Suppoſe theſe Rays to paſs in the Lines B F, B H; the former of which Rays ſhall have had one Fit more of eaſy Tranſmiſſion, and the latter one Fit leſs, than the Rays that paſs from B to C. Now both theſe Rays, when they go out of the Drop, will proceed by the Refraction of the Water in the Lines F I, H K, that will be inclined almoſt equally to the Rays incident on the Drop, that come from the Sun, but the Angles of their Inclination will be leſs than the Angle, in which the Rays emerging in the Line C D are inclined to thoſe incident Rays. And after the ſame manner Rays ſcattered from the Point B, at a certain Diſtance without theſe, will emerge out of the Drop, while the intermediate Rays are intercepted; and theſe emergent Rays will be inclined to the Rays incident on the Drop in Angles ſtill leſs than the Angles, in which the Rays F I and H K are inclined to them; and without theſe Rays will emerge other Rays, that ſhall be inclined to the incident Rays in Angles yet leſs. Now by this means will be formed of every kind of Rays, beſides the principal Arch which goes to the Formation of the Rainbow, other Arches, within every one of the Principal, of the ſame Colour, though much more faint: and this for divers Succeſſions, as long as theſe weak Lights, which in every Arch grow more and more obſcure, ſhall continue viſible. Now as the Arches produced by each Colour will be variouſly mixed together, the diverſity of Colours obſerved by Dr. *Langwith* may well ariſe from them.

The preciſe Diſtances between the principal Arch of each reſpective Colour and theſe fainter correſpondent Arches depend on the Magnitude of the Drops of Rain. In particular, the ſmalleſt Drops will make the ſecondary Arches of each Species at the greateſt Diſtance from their reſpective Principal, and from each other. Whence, as the Drops of Rain increaſe in falling, theſe Arches near the Horizon by their great Nearneſs to their reſpective principal Arches become viſible.

Of Refracting Circles in two Poriſms.

And now, Sir, we are upon the Rainbow, I ſhall here take the Freedom of ſetting down two Propoſitions, which I have formerly conſidered, relating to this Subject. For the greater Brevity I ſhall deliver them under the Form of Poriſms; as, in my Opinion, the Ancients called all Propoſitions treated by Analyſis only.

Prop. I.] *In a given refracting Circle, whoſe refracting Power is given, the Ray is given in Poſition, which paſſing parallel to a given Diameter of the Circle is refracted by that Circle to a Point given in the Circumference of it.*

Let A B C D be the given Circle, the given Diameter A C, and given Point G; and let the Ray E F, parallel to A C, be refracted to G. I ſay E F is given in Poſition. *Fig.* 76.

Produce E F to H, and draw the Diameter F I, drawing likewiſe I K H, I G. Then is H F I the Angle of Incidence, and G F I the refracted Angle; ſo that I H being perpendicular to F H and I G perpendicular to F G, I H is to I G as the Sine of the Angle of Incidence to the Sine of the refracted Angle, and the *Ratio* of I H to I G is given, as likewiſe the *Ratio* of I K to I G. Therefore I K being perpendicular to A C, the Point I is in a Conic Section given in Poſition, whoſe Axis is perpendicular to A C, and one of its *Foci* is the Point G[*]. Conſequently the Points I and F are given, and laſtly the Ray E F given in Poſition.

DETERM. I.] It is evident, that this conic Section, may either cut the Circle in two Points, touch it in one Point, or fall wholly without it. *Fig.* 77. Therefore let the Section touch the Circle in the Point I, and let I L touch both the Section and the Circle in the ſame Point I. Then G L being joined, the Angle under I G L on account of the conic Section is a right one[†], ſo that F G L is one continued right Line, and I F is to I L as F G to G I; as likewiſe, M being the Center of the Circle, M I to I L, or F H to H I, as F G to twice G I, becauſe M I is to I F as G I to twice G I. Hence by Permutation F H is to F G as H I to twice G I; that is, as the Sine of the Angle of Incidence to twice the Sine of the refracted Angle.

Moreover F H being to H I as F G to twice G I, the Square of F H will be to the Square of H I, as the Square of F G to four times the Square of G I. Therefore, by Compoſition, as the Square of F H to the Square of F I or of A C, ſo is the Square of F G to the Square of F I together with three times the Square of G I, and ſo likewiſe is the Exceſs of the Square of F G above the Square of F H, which equals the Exceſs of the Square of I H above the Square of I G, to three times the Square of G I; for as one Antecedent to one Conſequent, ſo is the difference of the Antecedents to the difference of the Conſequents. Hence in the laſt Place, the Square of half F H will be to the Square of A M, as the Exceſs of the Square of I H above the

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[*] *See* Papp. l. 7. prop. 238. Milnes Conic. part. 4. prop. 9.

[†] Milnes Conic. lib. 8. prop. 23.

Square of I G to three times the Square of I G, or as the Exceſs of the Square of the Sine of Incidence above the Square of the Sine of Refraction, to three times the Square of the Sine of Refraction.

Fig. 78. DETERM. 2.] Draw the Diameter G O and the Tangent O P, meeting G F produced in Q: then the Angle under I F G is equal to the Angle under O G F, the Angle under F I L equal to that under G O Q, both being right, and F I is equal to G O; whence the Triangles G O Q, F I L are ſimilar and equal; ſo that G Q is equal to F L, and the Point F in an *Hyperbola* paſſing through G, whoſe Aſymptotes are A C and O P*.

Prop. 2.] *A refracting Circle and its refracting Power being given, the Ray is given in Poſition, which, paſſing parallel to a given Diameter of the Circle, after its Refraction, is ſo reflected from the farther Surface of the Circle, as to be inclined to its incident Courſe in a given Angle.*

Fig. 79. Let A B C D be the given Circle; let A C be the given Diameter, E F the incident Ray parallel to it, which being refracted into the Line F G ſhall ſo be reflected from the Point G in the Line G H, that E F and H G being produced, till they meet in I, the Angle under E I H ſhall be given.

Let K be the Center of the Circle, and K F, K G be joined; let the Semidiameter L K be parallel to the refracted Ray F G, and M K being taken to the Semidiameter of the Circle in the *Ratio* of the Sine of Incidence to the Sine of Refraction; let L M be joined, and laſtly make the Angle under K M N equal to half the given Angle under E I H. This being done, if F G be produced to O, F O ſhall be to K O as the Sine of the Angle of Incidence to the Sine of the refracted Angle, that is, as M K to K L; in ſo much that K L being parallel to F O, and the Angle under M K L equal to that under F O K, the Angle under M L K ſhall be equal to that under F K O, and the Angle under K L M equal to that under K F O equal to that under F G K or half that under F G H, whence the Angle under K M N being equal to half the Angle under F I H, the reſiduary Angle under N M L will be equal to half the Angle under I F G, or to half that under M K L. Therefore L C being drawn, the Angle under L M N will be equal to that under M C L; and in the laſt Place, if M C be divided into two equal Parts in P, and P Q R be drawn parallel to C L, the Angle under Q M R will be equal to that under R P M, and the Triangles Q M R, M P R ſimilar, ſo that the Rectangle under P R Q ſhall be equal to the Square of M R. Whence R L being equal to M R, the Point L ſhall be in an equilateral *Hyperbola*, touching the Line M N in the Point M, and having the Point P for its Center †. But this *Hyperbola* is given in Poſition, and conſequently the Point L, the Angle under M L K, and the equal Angle under C K F will be given, and therefore the Ray E F is given in Poſition.

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* Apoll. Conic. l. 2. prop. 8.

† Apoll. Conic. lib. 1. prop. 37. *compared with* lib. 7. prop. 23.

Fig. 80.

DETERM. 1.] Let the *Hyperbola* touch the Circle in the Point L, and let their common Tangent be L S; draw L T parallel to M N, ſo as to be ordinately applied in the *Hyperbola* to the Diameter C M. Whence L S touching the *Hyperbola* in L, P T will be to T L as T L to T S *, and the Angle under T S L equal to that under T L P, but as the Angle under S C L is equal to that under N M L, the ſame is equal to the Angle under T L M; therefore the Angle under S L C is equal to the Angle under M L P. Farther, M L being produced to V and V C joined, the Angle under L V C is equal to that under S L C, by reaſon that LS touches the Circle in L; hence the Angles under L V C and under M L P are equal, L P, V C are parallel, and M P being equal to P C, M L is equal to L V; and K W being let fall perpendicular to L V, M W is equal to three times L W. But now if the incident Ray E F be produced to X, the Angle under M L K being equal to that under C K F, or to that under E F K, F X ſhall be equal to L V, equal to twice L W; and the Angle under K M L being equal to that under K F G; ſince K W is perpendicular to M W, F G ſhall be to twice M W as M K to K F, or as the Sine of Incidence to the Sine of Refraction: whence M W being equal to three times L W, F X ſhall be to F G as the Sine of Incidence to three times the Sine of Refraction.

Moreover, M W being equal to three times L W, the Square of M W will be equal to nine times the Square of L W, and the Rectangle under V M L, or the Rectangle under C M A, that is, the Exceſs of the Square of K M above the Square of K A, will be equal to eight times the Square of L W; therefore the Square of L W or the Square of half F X will be to the Square of K L, or of K A, as the Exceſs of the Square of K M above the Square of K A to eight times the Square of K A, that is, as the Exceſs of the Square of the Sine of Incidence above the Sine of Refraction, to eight times the Square of the Sine of Refraction.

DETERM. 2.] Draw A Y parallel to M N, and A Z parallel to M V: then is the Angle under Y A Z, equal to that under L M N, which is equal to that under L C A; whence the Arches A L, Y Z are equal; but the Arches A L, V Z are likewiſe equal, becauſe L V, A Z are parallel, therefore Y V being joined, and L Γ drawn perpendicular to A C, the Chord V Y ſhall be the double of L Γ; but V Δ being likewiſe let fall perpendicular to A C, becauſe M V is the double of M L, V Δ ſhall be the double of L Γ; and therefore V Δ and V Y ſhall be equal; whence the Point V ſhall be in a *Parabola*, whoſe *Focus* is the Point Y, its Axis perpendicular to A C, and

Fig. 81.

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* Apoll. Conic. lib. 1. prop. 37. *compared with* lib. 7. prop. 23.

the *Latus rectum*, belonging to that Axis, equal to twice the perpendicular let fall from Y upon A C *. But if K V be joined, the Angle under L K V is equal to twice the Complement to a right Angle of the Angle under K L V, which is equal to the Angle of Incidence, and exceeds the refracted Angle by the Angle under A K L.

The Determinations of theſe two Propoſitions, have relation to the firſt and ſecond Rainbow; thoſe of the firſt Propoſition reſpecting the interior, and thoſe of the ſecond the exterior. The firſt Determinations of theſe two Propoſitions aſſign the Angles, under which each Rainbow will appear in any given refracting Power of the tranſparent Subſtance, by which they are produced; the latter Determinations of theſe Propoſitions teach how to find the refracting Power of the Subſtance, from the Angles under which the Rainbows appear; the Angle under C M G, in the Determinations of the firſt Propoſition, being half the Angle which meaſures the Diſtance of the interior Bow from the Point oppoſite to the Sun; and in the Determinations of the ſecond Propoſition, the Angle under C M N is half the Complement to a right Angle of half the Angle that meaſures the Diſtance of the exterior Bow, from the Point oppoſite to the Sun. But whereas theſe latter Determinations require ſolid Geometry, it may not be amiſs here to ſhew how they may be reduced to Calculation, ſeeing the Obſervation of theſe Angles, as the learned Dr. *Halley* has already remark'd †, affords no inconvenient Method of finding the refracting Power of any Fluid, or indeed of any tranſparent Subſtance, if it be formed into a ſpherical or cylindrical Figure. For this purpoſe therefore I have found, that in the latter Determination of the firſt Propoſition, if the Sine of the Angle under C M G be denoted by a, the Tangent of the Complement of this Angle to a right one be denoted by b, and the Secant of this Complement by c; the Root of this Equation $z^3 - 3aaz = 2aa \times \overline{2c - a}$ will exceed the Sine of the Angle under F M A, that is, the Sine of the Angle of Incidence, by the Sine of the Angle under C M G; and the Sine of the Angle under F M O, which is double the refracted Angle, will be the Root of this Equation $x^3 + 3aax = 4aab$; this Angle being acute, when the Tangent of the Angle under C M G is leſs than half the *Radius*, or when the Angle itſelf is leſs than 26 *Deg.* 33′. 54″. 11‴, and when this Tangent is more than half the *Radius*, the Angle under O M F is obtuſe. Fig. 82.

The Roots of theſe cubic Equations are found by ſeeking the firſt of two mean Proportionals, between each of the verſed Sines appertaining to the Arches C G, A G, and the Sine of thoſe Arches, counting from the verſed Sines; for the Sum of theſe two mean Proportionals is the Root of the former Equation, and the Difference between them the Root of the latter; as may be collected from *Cardan*'s Rules.

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* Vide de la Hire, Sect. Conic. lib. 8. prop. 1, 3.

† *Philoſoph. Tranſact.* N°. 267. *pag.* 722.

And hence likewiſe if the firſt and laſt of the five mean Proportionals, between the Sine and Coſine of half the Angle under C M G be found, twice the Sum of the Squares of theſe mean Proportionals applied to the *Radius* exceeds the Sine of the Angle of Incidence by the Sine of the Angle under C M G; and twice the Difference of the Squares of the ſame mean Proportionals applied to the *Radius* is equal to the Sine of double the refracted Angle. Moreover this double of the refracted Angle exceeds the Angle of Incidence by the Angle under C M G.

Fig. 83.

In the latter Determination of the ſecond Propoſition draw K Y, and A Y being parallel to M N, the Angle under C K Y will be equal to twice the Angle under C M N, that is equal to the Complement of half the Diſtance of the exterior Rainbow from the Point oppoſite to the Sun. Then putting a for the Radius A K, and b for the Sine of the Angle under C K Y, the Sine of the Angle under A K V will be the Root of this Equation, $y^4 + 4by^3 - 8aaby + 4aabb = 0$. But the Angle of Incidence and Refraction may alſo be found as follows.

Let two mean Proportionals between the *Radius* and the Sine of the Angle under C K Y be found, then take the Angle, whoſe Coſine is the firſt of theſe mean Proportionals, counting from the *Radius*; and alſo the Angle whoſe Sine together with the ſecond mean Proportional ſhall be to the *Radius*, as the Coſine of the Angle under C K Y, to the Sine of the Angle before found. The Sum of theſe three Angles is double the Complement to a right one of the Angle under A K L, the Angle under K M L, or the refracted Angle, being equal to half the Sum of this Angle under A K L and the Angle under C K Y; as in the laſt Place the Angle under K I V, *i. e.* the Angle of Incidence, equal to the Sum of the Angles under K M L and under M K L.

I need not obſerve, that the geometrical Methods of deducing theſe Angles of Incidence and Refraction, from the Angle meaſuring the Diſtance of each Rainbow from the Point oppoſite to the Sun, afford very expeditious mechanical Conſtructions.

An Account of Mr. Leeuwenhoek's *Microſcopes, by Mr.* Folkes, V. P. N° 380. *p.* 446.

IV. This curious and valuable Legacy left by the laſt Will and Teſtament of the late Mr. *Leeuwenhoek* to the *Royal Society*, conſiſts of a ſmall *Indian* Cabinet, in the Drawers of which are 13 little Boxes or Caſes, each containing two Microſcopes, handſomely fitted up in Silver, all which, not only the Glaſſes, but alſo the *Apparatus* for managing of them, were made with the late Mr. *Leeuwenhoek*'s own Hands: Beſides which, they ſeem to have been put in Order in the Cabinet by himſelf, as he deſign'd them to be preſented to the *Royal Society*, each Microſcope having had an Object placed before it, and the Whole being accompany'd with a Regiſter of the ſame, in his own Hand-Writing, as being deſirous the Gentlemen of the Society ſhould, without Trouble, be enabled to examine many of thoſe Objects, on which he had made the moſt conſiderable Diſcoveries.

Several of theſe Objects yet remain before the Microſcopes, tho' the greater Number are broken off, which was probably done by the ſhaking of the Boxes in the Carriage. I have, nevertheleſs, added a Tranſlation of the Regiſter, as it may ſerve to give a juſter Idea of what Mr. *Leeuwenhoek* deſign'd by this Legacy, and alſo be of Uſe, by putting any curious Obſerver in Mind of a Number of Minute Subjects, that may in a particular Manner deſerve his Attention.

The 13 Caſes abovemention'd are numbered from 15 to 27 incluſively, correſponding to which is the Regiſter of the Objects, two to every Caſe, as follows.

Caſe 1. N° 15. { 1 Globules of Blood, from which its Redneſs proceeds.
2 A thin Slice of Wood of the Lime-Tree, where the Veſſels conveying the Sap are cut tranſverſely.

Caſe 2. N° 16. { 1
2 The Eye of a Gnat.

Caſe 3. N° 17. { 1 A crooked Hair, to which adheres a Ring-Worm, with a Piece of the Cuticle.
2 A ſmall Hair from the Hand, by which it appears thoſe Hairs are not round.

Caſe 4. N° 18. { 1 Fleſh of the Codfiſh (*Cabeljaeuw*) ſhewing how the Fibres lie oblique to the Membranes.
2 An Embrio of Cochineal, taken from the Egg, in which the Limbs and Horns are conſpicuous.

Caſe 5. N° 19. { 1 Small Pipes, which compoſe the Elephant's Tooth.
2 Part of the Cryſtalline Humour, from the Eye of a Whale.

Caſe 6. N° 20. { 1 A Thread of Sheeps-Wool, which is broken, and appears to conſiſt of many leſſer Threads.
2 The Inſtrument, whence a Spider ſpins the Threads, that compoſe his Web.

Caſe 7. N° 21. { 1 A Granade, or Spark made in ſtriking Fire.
2 The Veſſels in a Leaf of Tea.

Caſe 8. N° 22. { 1 The *Animalcula in Semine Maſculino*, of a Lamb taken from the Teſticle, *July* 24. 1702.
2 A Piece of the Tongue of a Hog, full of ſharp Points.

Caſe 9. N° 23. { 1 A Fibre of Codfiſh, conſiſting of long ſlender Particles.
2 Another of the ſame.

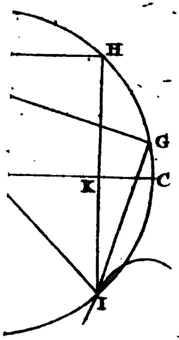
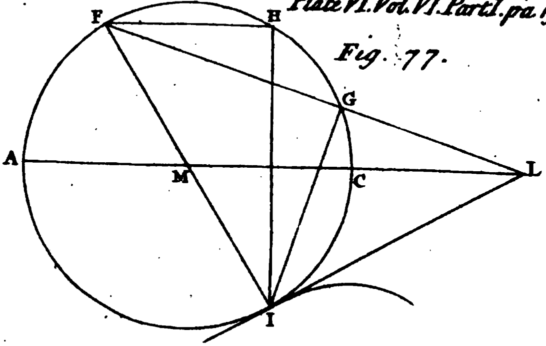


Fig. 77.



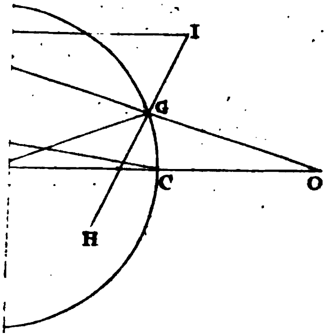
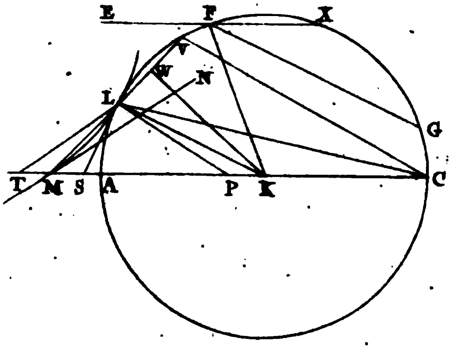


Fig. 80.



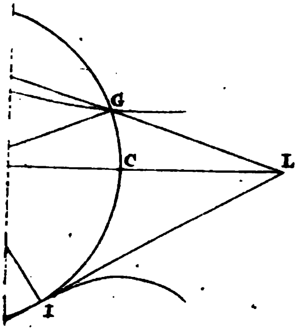
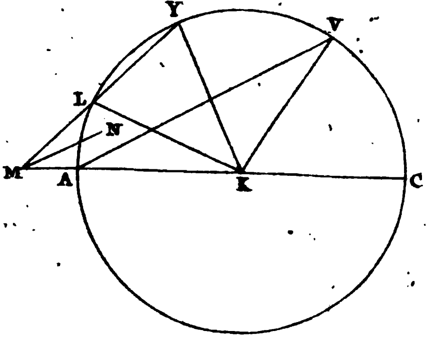


Fig. 83.



Toms Sculp.

Caſe 10. Nº 24.	1 A Filament, conveying Nouriſhment to the Nutmeg, cut tranſverſly. 2 Another Piece of the ſame, in which the Figure of the Veſſels may be ſeen.
Caſe 11. Nº 25.	1 Part of the Bone or Tooth abovementioned, conſiſting of hollow Pipes. 2 An exceeding thin Membrane, being that which cover'd a very ſmall Muſcle.
Caſe 12. Nº 26.	1 Veſſels by which Membranes receive Nouriſhment and Increaſe. 2 A Bunch of Hair from the Inſect call'd a Hair-Worm.
Caſe 13. Nº 27.	1 The double Silk, ſpun by the Worm. 2 The Organ of Sight of a Fly.

It were endleſs to enter into any Particulars, of what is to be obſerved in any of theſe Objects, or indeed to give any Account of Mr. *Leeuwenhoek*'s Diſcoveries; they are ſo numerous as to make up a conſiderable Part of the *Philoſophical Tranſactions*, and when collected together, to fill four pretty large Volumes in Quarto, which have been publiſh'd by him at ſeveral Times: And of ſuch Conſequence, as to have opened entirely new Scenes in ſome Parts of *Natural Philoſopy*, as we are all ſenſible, in that famous Diſcovery of the *Animalcula in Semine Maſculino*, which has given a perfectly new Turn to the Theory of Generation, in almoſt all the Authors that have ſince wrote upon that Subject.

For the Conſtruction of theſe Inſtruments, it is the ſame in them all, and the *Apparatus* is very ſimple and convenient: They are all ſingle Microſcopes, conſiſting each of a very ſmall double Convex-Glaſs, let into a Socket, between two Silver Plates rivetted together, and pierc'd with a ſmall Hole: The Object is placed on a Silver Point, or Needle, which, by Means of Screws of the ſame Metal, provided for that Purpoſe, may be turn'd about, rais'd, or depreſs'd, and brought nearer, or put farther from the Glaſs, as the Eye of the Obſerver, the Nature of the Object, and the convenient Examination of its ſeveral Parts may require.

Mr. *Leeuwenhoek* fix'd his Objects, if they were ſolid, to this Silver Point, with Glew; and when they were Fluid, or of ſuch a Nature as not to be commodiouſly view'd unleſs ſpread upon Glaſs, he firſt fitted them on a little Plate of Talk, or exceſſively thin-blown Glaſs, which he afterwards glewed to the Needle, in the ſame Manner as his other Objects.

The Obſervation, indeed, of the Circulation of the Blood, and ſome others, require a ſomewhat different *Apparatus*, and ſuch a one he had, to which he occaſionally fix'd theſe ſame Microſcopes; but as it makes no Part of this Cabinet, I ſhall omit giving any farther Account of it, only taking Notice that it may be ſeen in a Letter to the *Royal Society*, of the 12th of *January*, 1689, and printed in his *Arcana Naturæ Detecta*, N°. 69. But I was willing to mention juſt ſo much, as it may ſerve to ſhew the univerſal Uſe of theſe Microſcopes, and as it induces me (among other Things) to believe, theſe were the Kind of Microſcopes generally, if not ſolely, us'd by this curious Gentleman in all his Obſervations, and to which we are oblig'd for his moſt ſurprizing Diſcoveries.

Another Particular, to the ſame Purpoſe, I would not omit, and that is, That upon the late Queen *Mary*'s doing Mr. *Leeuwenhoek* the Honour of a Viſit at *Delft*, and viewing his Curioſities with great Satisfaction, he preſented her with a Couple of his Microſcopes, which, as I have been inform'd by one who had them a conſiderable Time in his Hands, were of the ſame Sort as theſe, and did not any ways differ from one of the 13 Caſes contain'd in the Drawers of this Cabinet.

The Glaſſes are all exceedingly clear, and ſhew the Object very bright and diſtinct, which muſt be owing to the great Care this Gentleman took, in the Choice of his Glaſs, his Exactneſs in giving it the true Figure; and afterwards, amongſt many, reſerving ſuch only for his Uſe, as he, upon Tryal, found to be moſt excellent. Their Powers of magnifying are different, as different Sorts of Objects may require; and, as on the one Hand, being all ground Glaſſes, none of them are ſo ſmall, and conſequently magnify to ſo great a Degree, as ſome of thoſe Drops, frequently us'd in other Microſcopes; yet, on the other, the Diſtinctneſs of theſe very much exceeds what I have met with in the Glaſſes of that Sort; and this was what Mr. *Leeuwenhoek* ever principally propos'd to himſelf, rejecting all thoſe Degrees of magnifying in which he could not ſo well obtain that End; for he informs us in one of his Letters, where he is ſpeaking of the exceſſive Praiſe ſome give to their Glaſſes on this Account, that although he had above Forty Years had Glaſſes by him of an extraordinary Smallneſs, he had made but very little Uſe of them; as having found, in a long Courſe of Experience, that the moſt conſiderable Diſcoveries were to be made with ſuch Glaſſes as, magnifying but moderately, exhibited the Object with the moſt perfect Brightneſs and Diſtinction.

But however excellent theſe Glaſſes may be judg'd, Mr. *Leeuwenhoek*'s Diſcoveries are not entirely to be imputed to their Goodneſs only: His own great Judgment, and Experience in the Manner of uſing them, together with the continual Application he gave to that Buſineſs, and the indefatigable Induſtry with which he contemplated often and long upon the ſame Subject, viewing it under many and different Circumſtances, cannot but have enabled him to form better Judgments of the Nature of his Objects, and ſee farther into their Conſtitution, than it can be imagined any other Perſon can do, that neither has the Experience, nor has taken the Pains this curious Author had ſo long done.

Nor ought we to forget a Piece of Skill, in which he very particularly excell'd, which was that of preparing his Objects in the beſt Manner, to be view'd by the Microſcope; and of this I am perſwaded, any one will be ſatisfied, who ſhall apply himſelf to the Examination of ſome of the ſame Objects as do yet remain before theſe Glaſſes; at leaſt, I have my ſelf found ſo much Difficulty in this Particular, as to obſerve a very ſenſible Difference between the Appearances of the ſame Object, when apply'd by my ſelf, and when prepared by Mr. *Leeuwenhoek*, tho' view'd with Glaſſes of the very ſame Goodneſs.

I have the rather inſiſted upon this, as it may be a Caution to us, that we do not raſhly condemn any of this Gentleman's Obſervations, tho' even with his own Glaſſes, if we ſhould not immediately be able to verify them our ſelves. We are under very great Diſadvantages for want of the Experience he had, and he has himſelf put us in Mind, more than once, that thoſe who are the beſt ſkill'd in the Uſe of magnifying Glaſſes, may be miſled, if they give too ſudden a Judgment upon what they ſee, or till they have been aſſured from repeated Experiments. But we have ſeen ſo many, and thoſe of his moſt ſurprizing Diſcoveries, ſo perfectly confirmed, by great numbers of the moſt curious and judicious Obſervers, that there can ſurely be no reaſon to diſtruſt his accuracy in thoſe others, which have not yet been ſo frequently or carefully examined.

V. The Inſtrument conſiſts of a metalline *Speculum*, about ſix Inches in Diameter. The *Radius* of the Sphere, on which its concave Surface was ground, is ten Feet, five Inches and one quarter, and conſequently its focal Length is 62 $\frac{5}{8}$ Inches. The Back has a hollow Screw made at its Centre, to receive the End of a Handle, which is ſcrew'd on, whenever the Metal is to be moved, in order to avoid ſullying its poliſh'd Surface by handling.

An Account of a Reflecting Teleſcope, made by Mr. J. Hadley. V. P. N° 376. *p.* 303.

This Object-Metal A, *Fig.* 84. is placed in one End of an octangular Tube, B B, about ſix Feet long, and ſomething wider than what is ſufficient to receive the Metal, dyed black on the Inſide. About ſix or ſeven Inches in Length of the three uppermoſt Sides of the Tube C (toward that End, at which the Metal is plac'd) are ſeparated from the reſt, and open with two Hinges, to make room for the Metal to be put in and taken out. The End of the Tube is cloſed by an octangular Piece of Board D, which has an opening *d*, about $\frac{2}{3}$ of an Inch broad, from the Top down to a little below the Centre, to give room for the beforemention'd Handle, when the Object-Metal is lifted into or out of the Tube; at other times it is cloſed with a ſliding Shutter. The Metal is placed ſo, as to have its *Axis* coincide with that of the

Fig. 84.

Tube, by the means of three ſmall Buttons fix'd to the Inſide of the Tube, having their hinder Ends all in the ſame Plane, to which this *Axis* is perpendicular. Two of theſe appear at *a a*, the third being at the middle of the Bottom of the Tube, is not ſeen. The foreſide of the Metal reſts againſt theſe Buttons in three Points of its Circumference, nearly equidiſtant from each other, and is held to them by three Screws, (one of which appears at *b*) which run through the octangular Board at the End of the Tube, and bear againſt the Back of the Metal, (in three Points, which directly anſwer thoſe three on the foreſide) with juſt ſo much Force, as is requiſite to keep it ſteady in its Place. They muſt not be ſcrew'd harder againſt the Metal for Fear of bending it, which (tho' it is half an Inch in Thickneſs) a very little Force is ſufficient to do. When the Inſtrument is not uſed, theſe Screws are looſen'd, and the Object-Metal is taken out and laid by, to prevent its tarniſhing.

The oval Plane is compos'd of a Plate of the ſame Metal with the great *Speculum*, about $\frac{1}{5}$ or $\frac{1}{6}$ of an Inch in Thickneſs, ſolder'd on the Back to another Braſs. Its Breadth is ſomething leſs than half an Inch, and is in Proportion to its Length as 1 to $\sqrt{2}$. At one End of the Oval, the Braſs Plate projects a little beyond the other, and has a Screw cut through it in that Part, as likewiſe another directly againſt the Centre of the foreſide. The other End is cypher'd away on the Backſide, that it may intercept as few of the Rays, in their Paſſage towards the Object-Metal, as is poſſible. The two Screw-holes in the Back ſerve to fix this Oval A, *Fig.* 85. to a Braſs Arm, B, which is faſtened at the other End into a
Fig. 84, 85. Slider E E. *Fig.* 84. and 85. This Slider is of an equal Thickneſs with the Side of the Tube, and has a Groove, G G, *Fig.* 84. cut for it in that Side, parallel to the *Axis*, and long enough to give room for its Motion, to ſet the two *Specula* at the different Diſtances, which the ſeveral Eye-Glaſſes require. It reſts on the Inſide againſt two thin Ledges, faſtened within the Tube along the Sides of the Groove. On the Outſide it is kept in its Place by a ſliding Shutter, not expreſſed in the Figure. In the Middle it has a Cylin-
Fig. 85. drick Cavity, D, *Fig.* 85. whoſe *Axis* is exactly perpendicular to its inner and outer Surfaces. Each of the Boxes, in which the Eye-Glaſſes are contained, is fitted to this Cavity. The beforementioned Braſs Arm is fix'd into the Inſide of this Slider, towards the End fartheſt from the Object-Metal; it riſes perpendicular for about two Inches, and is made flat, ſo as to turn one Edge to the Rays, which come from the Object. About *b*, it is bent forwards and flatted the other Way, ſo that when the Back of the oval Plane is held flat to it, by the two Screws *c c*, the Axis of the Cylindrick Cavity may fall on the Centre of its foreſide, inclin'd to its Surface in an Angle of ſomething leſs than forty five Degrees. This Angle is brought to be exact by two very ſmall Screws, *i i*, whoſe Threads

take hold in the flatted End of the Braſs Arm, and their Points bearing againſt the Back of the Oval, raiſe one End of it a little from the flat of the Arm. The *Specula* are ſet at their due Diſtance, by turning of a long Screw, CC, for which there is a Nut lodged in the Slider at *g*; the Screw is kept from moving backward or forward, when 'tis turned, by a Braſs Plate, F, which is to be fix'd to the flat End of the Side of the Tube, and taken off at Pleaſure. Each of the Eye-Glaſs Boxes, H, has a Screw on the outward End, to faſten to it a Bowl, or Diſh, I, to receive the Ball of the Eye, and guard it from external Light.

On the Top of the Tube is fix'd, on two ſmall Pedeſtals, a common Dioptrick Teleſcope, H, *Fig.* 84. about eighteen Inches long, its *Axis* parallel to that of the Tube; and having two Hairs plac'd in the common *Focus* of its Object and Eye-Glaſſes, croſſing one another in its *Axis.*

There are three convex Eye-Glaſſes belonging to the Inſtrument. The firſt, or ſhalloweſt, has its focal Diſtance of about $\frac{1}{3}$ of an Inch; the ſecond, of $\frac{1}{10}$; and the deepeſt, of $\frac{11}{40}$, or ſomething leſs. When the firſt of theſe is uſed with the Inſtrument, it magnifies about 188, or 190 times, in Diameter; with the ſecond, about 208; and with the third, 228 or 230. Each of theſe Glaſſes has placed, in that *Focus* neareſt the Oval, a Circle to determine the Part of the Object ſeen at one View; and in the other *Focus* toward the Eye, a Braſs Plate with a little Hole in the middle, to let no Light paſs to the Eye from the Inſide of the Tube, but what comes from the Oval. Beſides theſe three convex, there are two concave Eye-Glaſſes, with which it magnifies about 200 and 220 times; and alſo a Set of three Convex, which turn it into a Day Teleſcope, magnifying about 125 times. The Aperture is limited by a Circle of Card, or Paſtboard, placed before the Object-Metal in the Tube. To vary the Aperture there are three of theſe Circles, and the Apertures allowed by them are five Inches and an half, five Inches, and four and an half, tho' for ſome Objects the whole Metal may be left open.

The Engine made uſe of to direct the Tube to any Object, conſiſts of a ſtrong Plank, F F, *Fig.* 85 and 86, about fourteen Inches *Fig.* 85, 86.
wide, and two Feet and an half, or three Feet long, which ſerves as a Foundation for the whole. Near one End of this Plank is placed an upright fourſided Box, III, *Fig.* 84 and 86, about two *Fig.* 84, 86.
Feet high, narrower at the Back next the End of the Plank than before: Its two Sides are mortiſed both into the Plank below, *a a*, *Fig.* 86, and into the Top of the Box above, *d d*; the back and fore *Fig.* 86.
Part are faſten'd to the Edges of the Sides with Wood-Screws. The Top has a circular Hole cut in it, ſomething above three Inches in Diameter, whoſe Centre is about three Inches diſtant from the outſide of the Back, and at an equal Diſtance from the two Sides.

Sides. This Hole gives Paſſage to a turning Pillar B, in the Bottom of which there is fix'd an Iron Pivot *c*, to turn in a thick Braſs Plate lodged in the Plank, *b*. The upper End of the Pillar riſes about an Inch and an half above the Top of the Box, and is mortiſed into a ſtrong Head, K, *Fig*. 84. and 86, about eight Inches in Length, and four or five in Breadth and Thickneſs. This Head carries two Cheeks, L L, about thirteen or fourteen Inches in Height, their hinder Edges, towards the lower End, extending five Inches beyond the *Axis* of the Pillar backward. Along the Back of theſe Cheeks, at equal Diſtances above one another, there are Notches, tending obliquely downwards, and anſwering one another in each Cheek, to receive the Pivots of a crooked Iron *Axis*, C, *Fig*. 86. on which the Tube is plac'd. The Notches are made at different Heights, to keep the Eye-Glaſs at a proper Height for the Eye, in different Elevations of the Object above the Horizon. The Figure of the *Axis* anſwers that of the three under Sides of the Tube. The *Axis* of the Tube lies about two Inches and an half higher than the *Axis* of the Motion upon theſe Pivots, and the Centre of Gravity, when the Object-Metal is in, is about three Inches backwarder. To keep the Tube from ſlipping back, when its fore End is raiſed, it has two Buttons fixed to it, which reſt againſt the fore Part of the *Axis*.

Fig. 84, 86. *Fig*. 86.

To keep the Pillar from touching any of the Sides of the round Hole, in which it turns, a Cylindrick Sector, containing about 65° or 70°, and about an Inch in Height, is cut on the back Part of the Pillar, near the upper End D. In the Angle of this Cavity is fix'd a thin Steel Plate *o o*, bent croſs the middle to the ſame Angle. The internal angular Edge, between the two Parts of this Plate, lies in the *Axis* of the Pillar, and turns upon the harden'd Edge of a Wedgelike Iron, *f*, whoſe Baſe, or Board Part, is faſten'd with two ſtrong Screws on the Top of the Box, directly behind the round Hole beforemention'd.

The upper Parts of the Cheeks are ſtrengthened by two Brackets, G G, leaving Room between them for the Bottom of the Tube to touch the upper Edge of the fore Part of the Head. The hinder Part of the Head is alſo hollow'd, in the Manner preſented in the third Figure.

Fig. 84. The Head on its fore Part carries a flat Arm, M, *Fig*. 84. about twenty ſeven Inches long, a little taper towards the farther End, where it is four Inches broad. This is ſtrengthened by a narrow Slip, glew'd edgewiſe along the middle underneath, O, and alſo by a Brace or Stay, N, reaching from the turning Pillar to within nine Inches of the End of the Arm. The Stay paſſes through a tranſverſe opening cut in the fore Part of the Box, P, which is long enough to allow room for a ſufficient Motion of the Pillar round its *Axis*.

On the other End of the Bottom Plank, tranſverſely to its Length, is erected a Board about twelve Inches wide, and twenty ſix or twenty ſeven high, Q, the Top of it reaching within an Inch and an half of the under Side of the Arm. This Board is held firm in its Poſition by a Spur, R, part of its upper End on the outſide is pared off toward the Edges, to form it into the Segment of a Cylinder, whoſe *Axis* coincides with that of the Pillar. Its Uſe is to ſupport a Reſt, S S, on which the End of the flat Arm moves backward and forward. This Reſt being apply'd tranſverſely to the outer Part of the upright Board, where it is made Cylindrick, is bent into the ſame Figure, by the means of four Screw-Pins, two of which paſſing through each End of this, and of another Piece of the ſame Length, T, (but ſomething narrower) placed over againſt it on the inſide of the Board, by their Nuts, draw them together, ſo as to graſp the End of the upright Board between them; the upper Edge of the Reſt being firſt ſhot with a Plane very ſtrait and ſmooth. To render the Motion of the Arm along the Reſt ſmooth and eaſy, it has two Rollers lodged in a Box fix'd near the End, on its underſide, V, to roll upon the Edge of the Reſt, when the End of the Arm is moved along it. One of the Rollers is placed near each Edge of the Arm, and their *Axes* lye in Lines paſſing through the *Axis* of the turning Pillar. The Reſt is kept up to them, with a proper Degree of Force, by two Screws, W W, which run into two Plugs, X X, faſtened on the Sides of the upright Board, and bear againſt the under Sides of two Pieces fix'd on the Inſide of the Reſt.

The Motion of the Tube is governed by two Braſs Pegs, Y and Z. The firſt of theſe, Y, is plac'd about 10 or 11 Inches from the End of the Arm, and has a Line wound round it, which paſſing under a ſmall Pulley, *f*, fix'd in a vertical Poſition near the End of the Arm, is faſtened to a Staple on the under ſide of the Tube *g*. This Line, by the turning of the Peg, brings the fore End of the Tube to its due Elevation, being acted againſt by the Exceſs of Weight in the hinder End of the Tube, when the Metal is in it, which is equivalent to about two Pound at *g*, where the Line is faſtened. In great Elevations of the Object above the Horizon, the Line is not carried ſo far as the Point *g*; but is faſten'd a little above the Pulley, to a light ſquare Stick, *h*, having at one End a Hook, by which it takes hold of the Staple *g*. This is done that the Springyneſs of the Line may not continue a vibrating Motion in the Tube, (when any thing happens to ſhake the Inſtrument) and make the Object appear to tremble. The lower Part of the Stick reſts againſt the End of the Arm, and by its ſlight Friction contributes to the ſame Effect.

The other Peg, *Z*, is ſo plac'd, that it may be conveniently reached by one Hand of the Obſerver, while the other is employed about the Peg Y: It regulates the Horizontal Motion of the Tube, by means of a Line, which being wound about the Peg at one End, paſſes by another ſmall Pulley placed cloſe by the Side of the aforementioned one in an Horizontal Poſture (not to be ſeen in the Figure) and is hung on a Pin driven into the little Head K. It is acted againſt by two Springs, *m* and *n* *Fig.* 86. placed in the Box, III, one on each Side of the turning Pillar; that on the right Hand, *m*, draws the right Side of the Pillar forward, by a very ſtrong Line, which being faſtened to the Head of the Spring, paſſes round the back Part of the Pillar to a Pin, at P, by which it is ſtrain'd to its due Strength. The Spring on the Left Hand *n*, draws the Left Side of the Pillar backwards in the ſame manner. Theſe Pins are plac'd on the Pillar a little higher than the Tops of the Springs, that being drawn a little downwards, as well as turn'd round its *Axis*, the Pivot in its Bottom may not be raiſed out of the Hole in the Braſs Plate, when the Reſt bears hard againſt the Rollers at the End of the Arm. Each of theſe Springs draws with a Force equal to about 18 or 20 Pounds Weight, when the End of the Arm is carried cloſe to the ſmall Head *k*, *Fig.* 84. and conſequently (the Semidiameter of the Pillar being an Inch and Half, and the Diſtance of that Head from the *Axis* about 28 or 29 Inches) the End of the Arm will be carried by the united Forces of both the Springs, towards the other End of the Reſt, with a Force equivalent to the Weight of about two Pounds. Each of the Pegs, Y and Z, turns in a Hole made in a Piece of Wood *l*, faſtened to the under Side of the Arm; and the Pieces being ſlit with a Saw from one End through the Hole, and about half an Inch beyond it, the ſeparated Parts are drawn together by a Skrew *m*, till the End of the Peg is griped between them, with a due Degree of Force. By theſe Pegs, with the help of the Teleſcope H, the Tube is eaſily directed to any Object, and made to accompany a Celeſtial one in its Diurnal Motion, while the End of the Arm moves the whole Length of the Reſt. *Fig.* 86. *Fig.* 84.

The concave Surface of the Object-Metal has many little Spots in it, which could not be brought to take a Poliſh. In one, or two Places, the Metal itſelf ſeems to have ſome ſmall Parts, ſomething harder or ſofter than the reſt, occaſioning an irregularity in the Figure of the Metal about them. But theſe Parts being ſmall, in Proportion to the whole, do not ſeem conſiderably to affect the Diſtinctneſs of the Appearance.

The open Air has commonly an undulating Motion in its Parts, eſpecially in the day time, which occaſions the Rays of Light to deflect a little from the ſtrait Lines, in which they ought to move, in order to render the *Species* perfectly diſtinct. The Effect of this, though inſenſible to the naked Eye, or even through a ſmall Teleſcope, becomes conſiderable, when the Object is very much magnified. This Inſtrument, when try'd at an Object encloſed, ſo as to ſecure it from this Inconvenience, ſeems to bear an Aperture of five Inches and an half, with the deepeſt of the forementioned Eye-Glaſſes, as well as the common Teleſcopes do the uſual Charge and Aperture given to them, except that in theſe the Objects appear a little brighter.

Fig. 84. Repreſents the Inſtrument placed on the Machine, in order to be apply'd to Uſe.

Fig. 85. Repreſents the Inſide of the Slider, with the reſt of the *Apparatus* belonging to the oval Plane and Eye-Glaſs.

Fig. 86. Repreſents the hinder Part of the Machine, the Back, and one Side of the Box, being taken away, to ſhew the turning Pillar and Springs on the Inſide.

The Deſcription of an Inſtrument for taking Angles, by Reflections, invented by Mr. J. Hadley. V. P. N°420. p. 147

VI. The Inſtrument is deſign'd to be of Uſe, where the Motion of the Objects, or any Circumſtance occaſioning an Unſteadineſs in the common Inſtruments, renders the Obſervations difficult or uncertain.

The Contrivance of it is founded on this obvious Principle in Catoptricks: That if the Rays of Light diverging from, or converging to any Point, be reflected by a plane poliſh'd Surface, they will, after the Reflection, diverge from, or converge to another Point on the oppoſite Side of that Surface, at the ſame Diſtance from it as the firſt; and that a Line perpendicular to the Surface paſſing through one of thoſe Points, will paſs through both. Hence it follows, that if the Rays of Light emitted from any Point of an Object be ſucceſſively reflected from two ſuch poliſh'd Surfaces; that then a third Plane, perpendicular to them both, paſſing through the emitting Point, will alſo paſs through each of its two ſucceſſive Images made by the Reflections: All three Points will be at equal Diſtances from the common Interſection of the three Planes; and if two Lines be drawn through that common Interſection, one from the original Point in the Object, the other from that Image of it which is made by the ſecond Reflection; they will comprehend an Angle double to that of the Inclination of the two poliſh'd Surfaces.

Fig. 87. Let R F H and R G I repreſent the Sections of the the Plane of the Figure by the poliſh'd Surfaces of the two Specula B C and D E, erected perpendicularly thereon, meeting in R, which will be the Point where their common Section, perpendicular likewiſe to the ſame Plane, paſſes it, and H R I is the Angle of their Inclination. Let A F be a Ray of Light from any Point of an Object A falling on the Point F of the firſt Speculum B C, and thence reflected into the Line F G, and at the Point G of the ſecond Speculum D E reflected again into the

Fig. 87.

Line G K, produce G F and K G backwards to M and N, the two ſucceſſive Repreſentations of the Point A ; and draw R A, RM, and R N.

Since the Point A is in the Plane of the Scheme, the Point M will be ſo alſo by the known Laws of Catoptricks. The Line F M is equal to F A, and the Angle M F A double the Angle H F A or M F H ; conſequently R M is equal to R A, and the Angle M R A double the Angle H R A or M R H. In the ſame manner the Point N, is alſo in the Plane of the Scheme, the Line R N equal to R M, and the Angle M R N double the Angle M R I or I R N : Subſtract the Angle M R A from the Angle M R N, and the Angle A R N remains equal to double the Difference of the Angles M R I and M R H, or double the Angle H R I, by which the Surface of the Speculum D E is reclin'd from that of B C ; and the Lines R A, R M and R N are equal.

Corol. 1. The Image N will continue in the ſame Point ; altho' the two Specula be turn'd together circularly on the Axis R, ſo long as the Point A remains elevated on the Surface of B C : provided they retain the ſame Inclination.

Corol. 2. If the Eye be plac'd at L, (the Point where the Line A F continued cuts the Line G K ;) the Points A and N will appear to it at the angular Diſtance A L N, which will be equal to A R N : For the Angle A L N is the Difference of the Angles F G N and G F L ; and F G N is double F G I ; and G F L double G F R, and conſequently their Difference double F R G or H R I: Therefore L is in the Circumference of a Circle paſſing through A, N, and R.

Corol. 3. If the Diſtance A R be infinite, thoſe Points A and N will appear at the ſame angular Diſtance, in whatever Points of the Scheme the Eye and Specula are placed : Provided the Inclination of their Surfaces remain unaltered, and their common Section parallel to itſelf.

Corol. 4. All the Parts of any Objects will appear to an Eye viewing them by the two ſucceſſive Reflections, as before deſcribed, in the ſame Situation as if they had been turn'd together circularly round the Axis R, keeping their reſpective Diſtances from one another, and the Axis, with the Direction H I, *i. e.* the ſame Way the ſecond Speculum D E reclines from the firſt B C.

Corol. 5. If the Specula be ſuppos'd to be at the Center of an infinite Sphere ; Objects in the Circumference of a great Circle, to which their common Section is perpendicular, will appear remov'd by the two Reflections, through an Arch of that Circle, equal to twice the Inclination of the Specula, as is before ſaid. But Objects at a Diſtance from that Circle will appear removed thro' the ſimilar Arch of a Parallel : Therefore the Change of their apparent Place will be meaſured by an Arch of a great Circle whoſe

Plate VIII. Vol. VI. Part I. page 140.

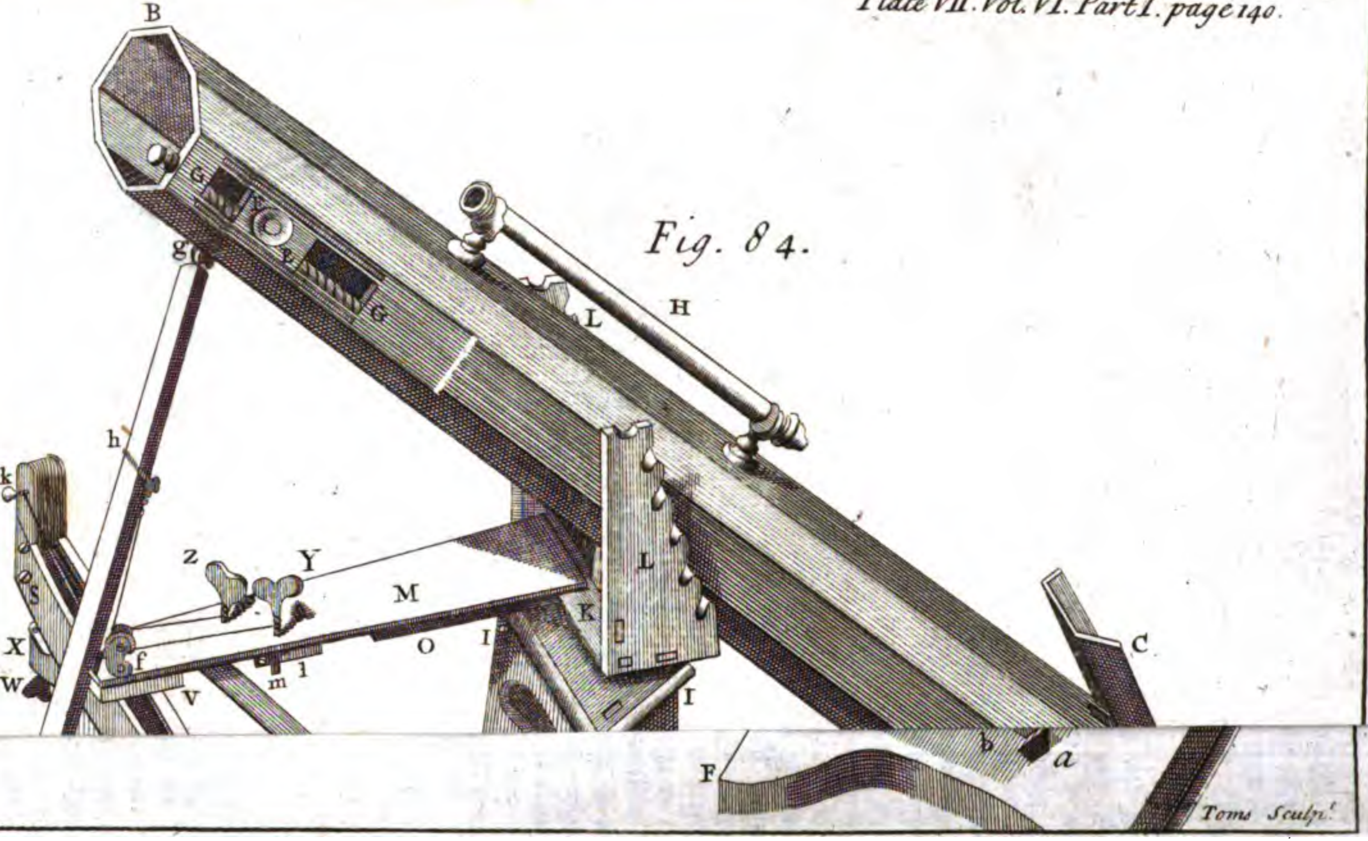


Fig. 84.

Toms Sculp.t

Chord is to the Chord of the Arch equal to double the Inclination of the Specula, as the Sines Complements of their reſpective Diſtances from that Circle are to the Radius: And if thoſe Diſtances are very ſmall, the Difference between the apparent Tranſlation of any one of theſe Objects, and the Tranſlation of thoſe which are in the Circumference of the great Circle aforeſaid, will be to an Arch equal to the verſed Sine of the Diſtance of this Object from that Circle, nearly as double the Sine of the Angle of Inclination of the Specula, is to the Sine Complement of the ſame.

Fig. 88. The Inſtrument conſiſts of an Octant A B C, having on its Limb B C an Arch of 45 Degrees, divided into 90 Parts or half Degrees; each of which anſwers to a whole Degree in the Obſervation. It has an Index M L moveable round the Center, to mark the Diviſions: And upon this, near the Center, is fix'd a plane Speculum E F perpendicular to the Plane of the Inſtrument, *Fig.* 88. and making ſuch an Angle with a Line drawn along the middle of the Index, as will be moſt convenient for the particular Uſes the Inſtrument is deſigned for; (for an Inſtrument made according to *Fig.* 88. the Angle L M F may be of about 65 Degrees.) IKGH is another ſmaller plane Speculum, fix'd on ſuch Part of the Octant as will likewiſe be determin'd by its particular Uſe, and having its Surface in ſuch Direction, that when the Index is brought to mark the beginning of the Diviſions (*i. e.* 0°) it may be exactly parallel to that of the other; this Speculum being turned towards the Obſerver, and the other from him. P R is a Teleſcope fix'd on one Side of the Octant, having its Axis parallel to that Side, and paſſing near the middle of one of the Edges I K or I H of the Speculum I K G H; ſo that half its Object-Glaſs may receive the Rays reflected from that Speculum, and the other half remain clear to receive them from a diſtant Object. The two Specula muſt alſo be diſpos'd in ſuch manner, that a Ray of Light coming from a Point near the middle of the firſt Speculum, may fall on the middle of the ſecond in an Angle of 70 Degrees or thereabouts, and be thence reflected into a Line parallel to the Axis of the Teleſcope, and that a clear Paſſage be left for the Rays coming from the Object to the Speculum E F by the Side H G. S T is a dark Glaſs fix'd in a Frame, which turns on the Pin V; by which Means it may be plac'd before the Speculum E F, when the Light of one of the Objects is too ſtrong: Of theſe there may be ſeveral.

Fig. 89. In the diſtinct Baſe of the Teleſcope, repreſented by the Circle *a b c d e f*, are placed three Hairs, two of which, *a c* and *b d*, are at equal Diſtances from, and parallel to the Line *g h*, which paſſes through the Axis, and is parallel to the Plane of the Octant: *Fig.* 89. The third *f e* is perpendicular to *g h* through the Axis.

The Inſtrument, as thus deſcribed, will ſerve to take any Angle not greater than 90 Degrees; but if it be deſign'd for Angles from 90 to 180 Degrees, the poliſh'd Surface of the Speculum E F *Fig.* 88. muſt be turn'd towards the Obſerver; the ſecond I K G H muſt be brought forward to the Poſition N O, ſo as to receive on its Middle the Rays of Light from the middle of the firſt in an Angle of about 25 Degrees, their Surfaces being perpendicular to one another when the Index is brought to the End of the divided Arch next C; and this ſecond muſt ſtand five or ſix Inches wide of the firſt, that the Head of the Obſerver may not intercept the Rays in their Paſſage towards it, when the Angle to be obſerv'd is near 180°. The ſmaller Speculum is fix'd perpendicularly on a round braſs Plate, tooth'd on the Edge; and may be adjuſted by an endleſs Screw.

In order to make an Obſervation, the Axis of the Teleſcope is to be directed towards one of the Objects, the Plane of the Inſtrument paſſing as near as may be through the other, which muſt lie to that Hand of the Obſerver, as the particular Form of the Inſtrument may require; *viz.* the ſame Way that the Speculum E F does from I K G H, if it be compoſed according to this Figure and Deſcription. The Obſerver's Eye being applied to the Teleſcope, ſo as to keep ſight of the firſt Object; the Index muſt be moved backward and forward till the ſecond Object is likewiſe brought to appear through the Teleſcope, about the ſame Diſtance from the Hair *c f* *Fig.* 90. as the firſt: If then the Objects appear wide of one another, as at *i* and *k*, the Inſtrument muſt be turn'd a little on the Axis of the Teleſcope, till they come even, or very nearly ſo, and the Index muſt be remov'd till they unite in one, or appear cloſe to one another in a Line parallel to *c f*, both of them being kept as near the Line *g b* as they can. If the Inſtrument be then turn'd a little on any Axis perpendicular to its Plane, the two Images will move along a Line parallel to *g b*, but keep the ſame Poſition in reſpect of one another; ſo that in whatever Part of that Line they be obſerved, the Accuracy of the Obſervation will be no otherwiſe affected than by the Indiſtinctneſs of the Objects. If the two Objects be not in the Plane of the Inſtrument, but equally elevated on, or depreſs'd below it, they will appear together at a Diſtance from the Line *g b*, when the Index marks an Angle ſomething greater than their neareſt Diſtance in a great Circle: And the Error of the Obſervation will increaſe nearly in Proportion to the Square of their Diſtance from that Line; but may be corrected by help of the fifth Corollary. Suppoſe the Hairs *a e* and *b d*, each at a Diſtance from the Line *g b*, equal to $\frac{100}{4146}$ of the focal Length of the Object-Glaſs, ſo as to comprehend between them the Image of an Object, whoſe Breadth to the naked Eye is a little more than 2° $\frac{3}{4}$; and let the Images of the Objects appear united at either of thoſe Hairs: Then as the Sine Complement of half the Degrees and Minutes

Fig. 90.

Plate VIII. Vol: VI. Part I. page 142.

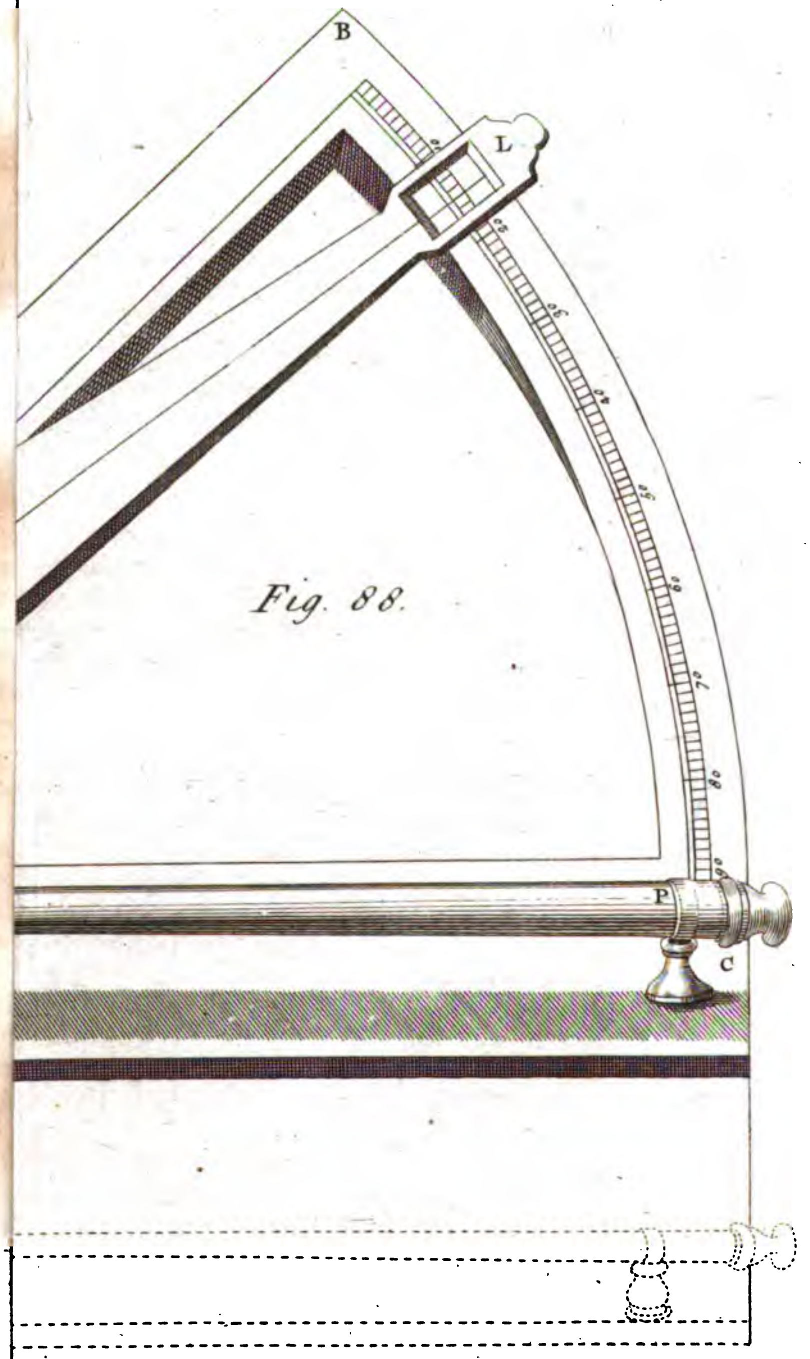
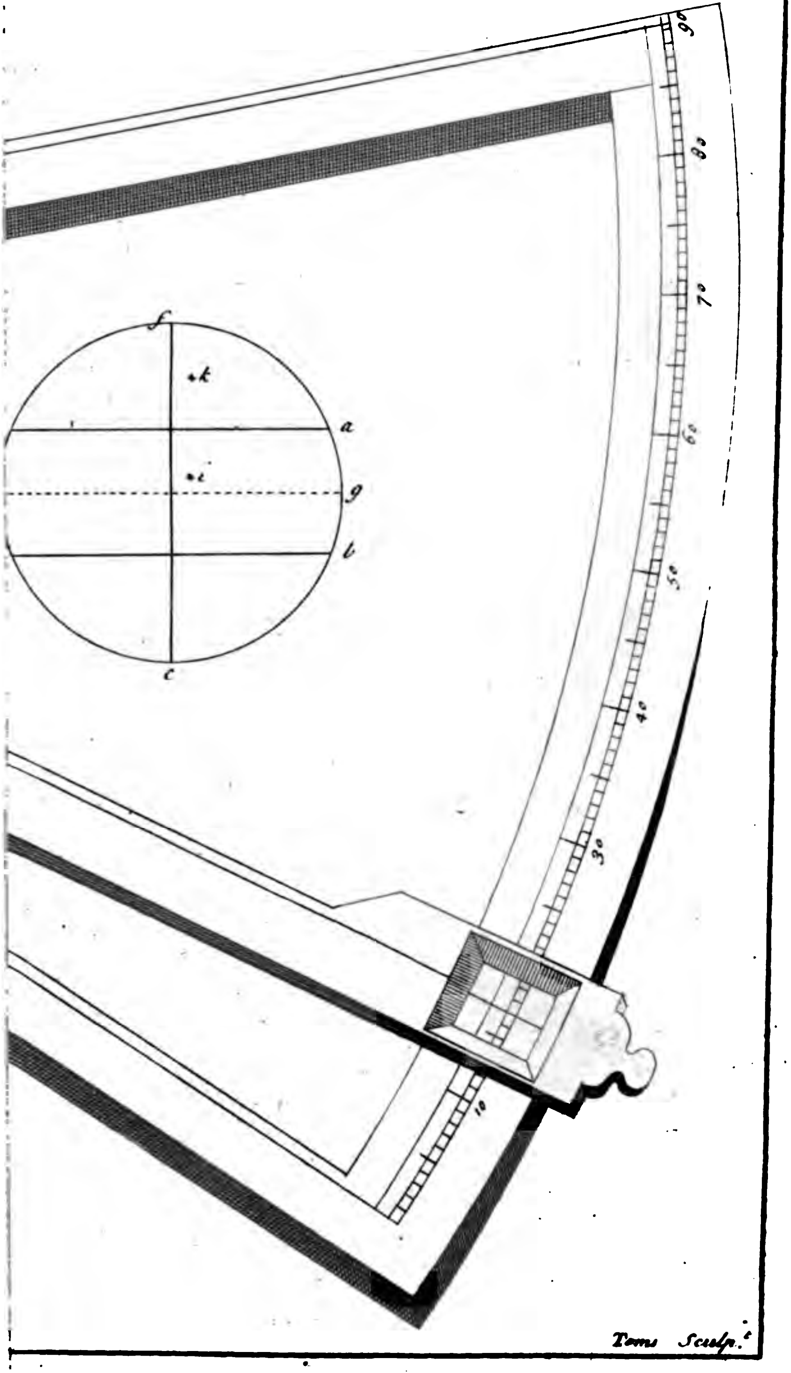


Fig. 88.

Plate IX. Vol. VI. Part I. page 142.



Toms Sculp.

Minutes mark'd by the Index, is to the doubled Sine of the ſame; ſo is one Minute to the Error which is always to be ſubſtracted from the Obſervation. Other Hairs may alſo be plac'd in the Area *a b c d e f*, parallel to *g h*, and at Diſtances from it proportional to the ſquare Roots of the Numbers 1, 2, 3, 4, *&c.* and then the Errors to be ſubſtracted from the ſame Obſervation made at each of thoſe Hairs reſpectively, will be in Proportion to the Numbers 1, 2, 3, 4, *&c.* This Correction will always be exact enough if the Obſerver take care (eſpecially when the Angle comes near 180°) to keep the Plane of the Inſtrument from varying too much from the great Circle paſſing thro' the Objects.

In regard to the Workmanſhip, if an Exactneſs be required in the Obſervations, the Arch ought to be divided with the greateſt Care; becauſe all Errors committed in the Diviſion are doubled by the Reflections. The Index muſt have a ſteady Motion on the Center, ſo that the Axis of it remain always perpendicular to the Plane of the Octant; for if that alter, it will be liable to vary the Inclination of the Speculum it carries to the other: The Motion muſt likewiſe be eaſy, leſt the Index be ſubject to bend edgeways: For the ſame reaſon it ſhould be as broad at that End next the Center as conveniently can be. The Specula ſhould have their Surfaces of a true Flat; becauſe a Curvature in either of them, beſide rendering the Object indiſtinct, will vary its Poſition, when ſeen by Reflection from different Parts of them: They muſt alſo be of a ſufficient Length and Breadth for the Teleſcope to take in a convenient Angle without loſing the Uſe of any Part of the Aperture of its Object-Glaſs, and that in all the different Poſitions of the Index. They may be either of Metal or Glaſs Plates foil'd, having their two Surfaces as nearly parallel as they can; yet a ſmall Deviation may be allowed; provided either their thickeſt or thinneſt Edges (and conſequently the common Section of their Surfaces) be parallel to the Plane of the Octant: For in that Caſe, though there are ſeveral Repreſentations of the Object, they will be always very near one another in a Line parallel to *c f*; and any of them may be uſed, except when the Angle to be obſerved is very ſmall. The chief Inconvenience will be, that a ſmall Star will be more difficultly diſcerned, the Light being divided among the ſeveral Images. The Teleſcope may be contrived to alter its Situation, ſo as to receive the reflected Rays on a greater or leſs Part of its Object-Glaſs, if the Objects differ in Brightneſs. The ſecond Speculum may have a Part unfoil'd, that if either of them be ſufficiently luminous, the leſs bright may be ſeen through it by the whole Aperture. If the Sun be one of the Objects, or the Moon be compared with a ſmaller fix'd Star; their reflected Images muſt be ſtill farther weakened by the Interpoſition of one or more of the dark Glaſſes S T. An exact Poſition of the Teleſcope is not neceſſary;

ſary; and the Inſtrument may be uſed without one, the Diſpoſition of the Specula, with regard to the Sector and Index, being ſuch as may allow the Eye to be brought as near the ſecond Speculum as may be, and make the Inſtrumeut the moſt commodious for the Obſerver.

No greater Degree of Steadineſs is requiſite in the Pedeſtal, or Machine which carries this Inſtrument, than what is ſufficient for the Teleſcope us'd with it: For although the vibrating Motion of the Inſtrument may occaſion the Images of the Objects alſo to vibrate croſs one another; their apparent relative Motion will be very nearly in Lines parallel to *c f*: and it will not be difficult to diſtinguiſh whether they coincide in croſſing one another, or paſs at a Diſtance: And if the Objects are near one another, and the Teleſcope magnify but about four or five Times, it may be held in the Hand without any ſtanding Support. In this Manner the Altitude of the Sun, Moon, or ſome of the brighter Stars from the viſible Horizon may be taken at Sea, when it is not too rough.

Fig. 90. ſhews an Inſtrument deſigned for this Purpoſe; differing from the foregoing Deſcription chiefly in the placing the Specula and Teleſcope, with regard to the Sector and Index; it has alſo a third Speculum N O diſpos'd according to the Directions when the Angle is greater than 90 Deg. whoſe Uſe is to obſerve the Sun's Altitude by Means of the oppoſite Part of the Horizon. In placing theſe two ſmaller Specula, it will be farther neceſſary to take care that the Speculum I K G H do not ſtand ſo as to intercept any of the Rays coming from the greater one fix'd on the Index to the third N O, nor either of them hinder the Index from coming Home to the End of the divided Arch. W Q is a Director for the Sight; which is neceſſary when the Teleſcope is not made uſe of. This conſiſts of a long narrow Piece, which ſlides on another fix'd on the back of the Octant, and carries at each End a Sight erected perpendicularly on it: It may be removed at Pleaſure, and exchanged for the Teleſcope, which ſlides on in the ſame manner, both ſerving indifferently with either of the two ſmaller Specula. The Eye is to be plac'd cloſe behind the Sight at W; and the Thread ſtretch'd acroſs the opening of the other Sight at Q perpendicular to the Inſtrument is to aſſiſt the Obſerver in holding it in a vertical Poſture, who is to keep this Thread as near as he can parallel to the Horizon, and the Object near the upright one.

How far an Inſtrument of this Kind may be of Uſe at Sea to take the Diſtance of the Moon's Limb from the Sun or a Star, in order to find the Ship's Longitude, when the Theory of that Planet is perfect, I leave to trials to determine.

The Society has the Satisfaction of knowing that Theory to be already brought to a good Degree of certainty and exactneſs, through the conſummate Skill of a very learned Member; and have great reaſon to hope, it will in a little time appear to be compleated by the continu'd Application of ſome of their own Body.

The different refrangibility of colour'd Light confirmed by Dr. Deſaguliers. N°374. p. 206.

VII. *Signior Gizlanzoni*, an *Italian* Gentleman, ſhew'd me a Paper of *Signior Rizzetti**, wherein he denied the different Refrangibility of the Rays of Light; becauſe an Experiment mention'd in Sir *Iſaac Newton*'s *Optics* (*B.* 1. *Prop.* 1. *Exp.* 1.) had not ſucceeded with him tho' after many Trials. At the Deſire of Sir *Iſaac Newton*, I repeated the Experiment before him and *Signior Gizlanzoni*, and ſome other Perſons, who were fully ſatisfied with the Succeſs of it. The Experiment and the Particulars which if duly put in Practice, will make it always ſucceed, I beg leave to add.

Fig. 91.

I painted one half of the Card RB, *Fig.* 91. as B, with *Ultramarine*, made deeper with a ſmall Mixture of *Indigo*, and the other half R, with *Cinnabar* heighten'd with a little *Carmine*, ſo that the Line, that ſeparated the red from the blue, was perpendicular to the long ſides of the Card.

Fig. 92.

Then I wrapp'd a black Silk four times together, over the middle of each painted part of the Card, as in *Fig.* 92.

Fig. 93. *Fig.* 94.

Upon a ſquare Trencher, *Fig.* 93. painted black, and ſuſpended vertically againſt a Wall, I fix'd my colour'd Card with a Pin, and the Room being made very dark, I enlighten'd the Card with a ſtrong Light thrown upon it from a dark Lanthorn, that had two Convex Glaſſes in it; then ſetting up the *Lens* LL, (repreſented by *Fig.* 94.) in ſuch manner, that its Axis paſs'd perpendicularly thro' the Image of the Card, at the diſtance of nine Feet from the Card, the Image of the Card being receiv'd upon a white Paper, at the diſtance of nine Feet on the other ſide of the *Lens*, at B, the blue half appear'd diſtinct, with the Image of the black Silk going vertically along its Plain, whilſt no Appearance of the black Silk was perceivable on the red half. Then removing the Paper about two Inches, to R, the red half of the Image had a black Line very plain upon it, whilſt it was inviſible on the blue half. This was more evident, when a ſtrong Image of the Candle was ſucceſſively thrown on that half of the Card, whoſe Image was under Examination. When the Paper was held in the middle between R, and B, the black Line upon each Colour was viſible, but indiſtinct.

N. B. Care muſt be taken that the Colours be deep.

VIII. *An Account of a Book*, intitl'd,

N°416. p. 598.

De Luminis Affectionibus Specimen Phyſico-Mathematicum, dedicated to Cardinal *Polignac*, and printed at *Treviſe* and *Venice*, 1727, by Signior *Rizzetti*.

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* See alſo Act. Erudit. Lipſ. Supplem. tom. 8. §. 3. p. 130. 131.

Dr. *Deſaguliers* takes notice, in his report, of the Author's unhandſome treatment of Sir *I. Newton*; the many groſs miſtakes he has committed; that there is no Experiment of Sir *I. Newton* call'd in queſtion, but what is true, and no Conſequence different from Sir *Iſaac*'s drawn from the Experiments he allows to be true, but what is falſe; and concludes that *ten Months* well employed in reading Sir *Iſaac*'s Book, will make him amends for his *ten Years* prejudiced Examination.

CHAP. III.

ASTRONOMY.

Of the Infinity of the Sphere of Fix'd Stars, by Dr. Halley. N°364. *p.* 22.

I. THE Syſtem of the World, as it is now underſtood, is taken to occupy the whole *Abyſs of Space*, and to be as ſuch actually infinite; and the Appearance of the Sphere of Fix'd Stars ſtill diſcovering ſmaller and ſmaller ones, as you apply better Teleſcopes, ſeems to confirm this Doctrine. And indeed, were the whole Syſtem finite, it, though never ſo extended, would ſtill occupy no part of the *infinitum* of Space, which neceſſarily and evidently exiſts; whence the whole would be ſurrounded on all ſides with an infinite *inane*, and the ſuperficial Stars would gravitate towards thoſe near the Center, and with an accelerated Motion run into them, and in proceſs of Time coaleſce and unite with them into one. And, ſuppoſing Time enough, this would be a neceſſary Conſequence. But if the whole be Infinite, all the Parts of it would be nearly *in æquilibrio*, and conſequently each Fix'd Star, being drawn by contrary Powers, would keep its Place; or move, till ſuch time, as, from ſuch an *æquilibrium*, it found its reſting Place; on which account, ſome, perhaps, may think the Infinity of the Sphere of Fix'd Stars no very precarious Poſtulate.

But to this I find two Objections, which are rather of a Metaphyſical than Phyſical Nature; and firſt, this ſuppoſes, as its conſequent, that the Number of Fix'd Stars is not only indefinite, but actually more than any finite Number; which ſeems abſurd *in terminis*, all Number being compoſed of Units, and no two Points or Centers being at a Diſtance more than finite. But to this it may be anſwer'd, that by the ſame Argument we may conclude againſt the poſſibility of eternal Duration, becauſe no number of Days, or Years, or Ages, can compleat it. Another Argument I have heard urged, that if the Number of Fix'd Stars were more than finite, the whole Superficies of their apparent Sphere would be luminous, for that thoſe ſhining Bodies would be more in Number than there are Seconds of a Degree in the *Area* of the whole ſpherical Surface, which I think cannot be denied. But if we ſuppoſe all the Fix'd Stars to be as far from one another, as the neareſt of them is from the Sun; that is, if we may ſuppoſe the Sun to be one of them, at a greater Diſtance their Diſks and Light will be diminiſh'd in the Proportion of Squares, and the Space to contain them will be increaſed in the ſame Proportion; ſo that in each ſpherical Surface the Number of Stars it might contain, will be as the Square of their Diſtances. Put then the

Diſtances immenſely great, as we are well aſſured they cannot but be, and from thence by an obvious *Calculus*, it will be found, that as the Light of the Fix'd Stars diminiſhes, the Intervals between them decreaſe in a leſs Proportion, the one being as the Diſtances, and the others as the Squares thereof, reciprocally. Add to this, that the more remote Stars, and thoſe far ſhort of the remoteſt, vaniſh even in the niceſt Teleſcopes, by reaſon of their extream minuteneſs; ſo that, though it were true, that ſome ſuch Stars are in ſuch a Place, yet their Beams, aided by any help yet known, are not ſufficient to move our Senſe; after the ſame manner as a ſmall Teleſcopical Fix'd Star is by no means perceivable to the naked Eye.

Of the Number, Order, and Light of the Fix'd Stars, by the ſame. N° 364. p. 24.

II. At the laſt meeting of the Society, I adventured to propoſe ſome Arguments, that ſeemed to me to evince the Infinity of the Sphere of Fix'd Stars, as occupying the whole Abyſs of Space, or the τὸ πᾶν, which at preſent is generally underſtood to be neceſſarily Infinite; and thence I laid before you what may ſeem a very *Metaphyſical Paradox*, *viz.* That the Number of Fix'd Stars muſt then be more than any finite Number, and ſome of them more than at a finite Diſtance from others. This ſeems to involve a Contradiction, but it is not the only one that occurs to thoſe who have undertaken freely to conſider the Nature of Infinite, which perhaps the very narrow Limits of humane Capacity cannot attain to.

Since then, I have attentively examined what might be the Conſequence of an Hypotheſis, that the Sun being one of the Fix'd Stars, all the reſt were as far diſtant from one another, as they are from us; and by a due Calculation I find, that there cannot, upon that Suppoſition, be more than thirteen Points in the Surface of a Sphere, as far diſtant from the Center of it, as they are from one another: and I believe it would be hard to find how to place thirteen Globes of equal Magnitude, ſo as to touch one in the Center: for the twelve Angles of the *Icoſaedron* are from one another very little more diſtant than from its Center; that is, the Side of the Triangular Baſe of that Solid, is very little more than the Semidiameter of the circumſcribed Sphere, it being to it nearly as 21 to 20; ſo that it is plain that ſomewhat more than twelve equal Spheres may be poſited about a middle one; but the ſpherical Angles or Inclinations of the Planes of theſe Figures being incommenſurable with the 360 Degrees of the Circle, there will be ſeveral Interſtices left, between ſome of the twelve, but not ſuch as to receive in any Part the thirteenth Sphere.

Hence it is no very improbable Conjecture, that the Number of the Fix'd Stars of the firſt Magnitude is ſo ſmall, becauſe this ſuperior Appearance of Light ariſes from their nearneſs; thoſe that are leſs ſhewing themſelves ſo ſmall by reaſon of their greater diſtance.

Now there are in all but ſixteen fix'd Stars in the whole Number of them, that can indiſputably be accounted of the firſt Magnitude; whereof four are *extra Zodiacum*; viz. *Capella*, *Arcturus*, *Lucida Lyræ*, and *Lucida Aquilæ*, to the *North*; four in the way of the *Moon* and *Planets*, to wit, *Palilicium*, *Cor Leonis*, *Spica*, and *Cor Scorpii*; and five to the *Southward*, that are ſeen in *England*, viz. The *Foot* and Right *Shoulder* of *Orion*, *Sirius*, *Procyon*, and *Fomalhaut*; and there are three more that never riſe in our Horizon, *viz. Canopus*, *Acharnâr*, and the *Foot* of the *Centaur*.

But that they exceed the Number thirteen, may be eaſily accounted for from the different Magnitudes that may be ſeen in the Stars themſelves; and perhaps ſome of them may be much nearer to one another, than they are to us; this exceſs of Number being found ſingly in the Signs of *Gemini* and *Cancer*. And indeed within 45 Degrees of Longitude, or one 8th of the whole, there are no leſs than *five* of theſe *ſixteen* to be ſeen. If therefore the Number of them be ſuppoſed *thirteen*, omitting Niceties in a Matter of ſuch Irregularity, at twice the Diſtance from the Sun there may be placed four times as many, or 52; which, with the ſame Allowance, would nearly repreſent the Number of the Stars we find to be of the 2d Magnitude: ſo 9 x 13, or 117, for thoſe at three times the Diſtance: and at ten times the Diſtance 100 x 13 or 1300 Stars; which Diſtance may perhaps diminiſh the Light of any of the Stars of the firſt Magnitude to that of the ſixth, it being but the hundredth Part of what, at their preſent Diſtance, they appear with. But if, ſince we have room enough for it, we ſhould ſuppoſe the Sphere continued to 10 times the laſt, or 100 times the firſt Diſtance, the Number of Stars would be 130,000, and they would appear but with the 10,000th Part of the Light of a firſt Magnitude Star, as we now ſee it. This is ſo ſmall a Pulſe of Light, that it may well be queſtioned, whether the Eye, aſſiſted with any artificial help, can be made ſenſible thereof. But 100 times the Diſtance of a Star we ſee, is ſtill Finite: from whence I leave thoſe that pleaſe to conſider it attentively, to draw the Concluſion.

A new apparent Motion in the Fix'd Stars diſcover'd, its Cauſe aſſign'd, the Velocity and æquable Motion of Light deduced; by Mr. J. Bradley. N° 406. *p.* 364.

III. The following Obſervations were begun by the Honourable *Samuel Molyneux*, Eſq; at *Kew*, continued and repeated by myſelf at *Kew* and *Wanſtead*, in hopes of verifying thoſe, that * Dr. *Hooke* formerly communicated to the Public, concerning the *Parallax* of *the Fix'd Stars.* Therefore the ſame Star was made choice of by Mr. *Molyneux*, almoſt the ſame Method follow'd, and his Inſtrument conſtructed upon Principles nearly the ſame; but greatly exceeding the Doctor's in exactneſs, which was chiefly owing to our curious Member Mr. *George Graham*, to whom the Lovers of Aſtronomy are alſo indebted for ſeveral other exact and well contrived Inſtruments.

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* An Attempt to prove the *Motion* of the *Earth*, from Obſervations made by *Robert Hooke*, Fellow of the *Royal Society*, Lond. 1674.

Mr. *Molyneux*'s *Apparatus* was compleated and fitted for obſerving about the End of *November* 1725, and on the third Day of *December* following, the bright Star in the Head of *Draco* (marked γ by *Bayer*) was for the firſt Time obſerved, as it paſſed near the Zenith, and its Situation carefully taken with the Inſtrument. The like Obſervations were made on the 5th, 11th, and 12th Days of the ſame Month, and there appearing no material Difference in the Place of the Star, a farther Repetition of them at this Seaſon ſeemed needleſs, it being a Part of the Year, wherein no ſenſible Alteration of Parallax in this Star could ſoon be expected. It was chiefly therefore Curioſity that tempted me (being then at *Kew*, where the Inſtrument was fixed) to prepare for obſerving the Star on *December* 17th; when having adjuſted the Inſtrument as uſual, I perceived that it paſſed a little more Southerly this Day than when it was obſerved before. Not ſuſpecting any other Cauſe of this Appearance, we firſt concluded, that it was owing to the Uncertainty of the Obſervations, and that either this or the foregoing were not ſo exact as we had before ſuppoſed; for which Reaſon we purpoſed to repeat the Obſervation again, in order to determine from whence this Difference proceeded; and upon doing it on *December* 20th, I found that the Star paſſed ſtill more Southerly than in the former Obſervations. This ſenſible Alteration the more ſurprized us, in that it was the contrary way from what it would have been, had it proceeded from an annual *Parallax* of the *Star*: But being now pretty well ſatisfied, that it could not be entirely owing to the want of Exactneſs in the Obſervations; and having no Notion of any thing elſe, that could cauſe ſuch an apparent Motion as this in the Star; we began to think that ſome Change in the Materials, &c. of the Inſtrument itſelf, might have occaſioned it. Under theſe Apprehenſions we remained ſome time, but being at length fully convinced, by ſeveral Trials, of the great Exactneſs of the Inſtrument, and finding by the gradual Increaſe of the Star's Diſtance from the Pole, that there muſt be ſome regular Cauſe that produced it; we took care to examine nicely, at the Time of each Obſervation, how much it was: and about the Beginning of *March* 1726, the Star was found to be 20″ more Southerly than at the Time of the firſt Obſervation. It now indeed ſeemed to have arrived at its utmoſt Limit Southward, becauſe in ſeveral Trials made about this Time, no ſenſible Difference was obſerved in its Situation. By the Middle of *April* it appeared to be returning back again towards the North; and about the Beginning of *June*, it paſſed at the ſame Diſtance from the Zenith as it had done in *December*, when it was firſt obſerved.

From the quick Alteration of this Star's Declination about this Time (it increaſing a Second in three Days) it was concluded, that it would now proceed Northward, as it before had gone Southward of its preſent Situation; and it happened as was conjectured: for the Star continued to move Northward till *September* following, when it again became ſtationary, being then near 20″ more Northerly than in *June*, and no leſs than 39″ more Northerly than it was in *March*. From *September* the Star returned towards the South, till it arrived in *December* to the ſame Situation it was in at that time twelve Months, allowing for the Difference of Declination on account of the Preceſſion of the Equinox.

This was a ſufficient Proof, that the Inſtrument had not been the Cauſe of this apparent Motion of the Star, and to find one adequate to ſuch an Effect ſeemed a Difficulty. *A Nutation of the Earth*'s *Axis* was one of the firſt things that offered itſelf upon this occaſion; but it was ſoon found inſufficient; for though it might have accounted for the change of Declination in γ *Draconis*, yet it would not at the ſame time agree with the Phænomena in other Stars; particularly in a ſmall one almoſt oppoſite in right Aſcenſion to γ *Draconis*, at about the ſame Diſtance from the North Pole of the Equator: For, though this Star ſeemed to move the ſame way, as a Nutation of the Earth's Axis would have made it, yet it changing its Declination but about half as much as γ *Draconis* in the ſame time (as appeared upon comparing the Obſervations of both made upon the ſame Days, at different Seaſons of the Year) this plainly proved, that the apparent Motion of the Stars was not occaſioned by a real Nutation, ſince if that had been the Cauſe, the Alteration in both Stars would have been near equal.

The great Regularity of the Obſervations left no room to doubt, but that there was ſome regular Cauſe that produced this unexpected Motion, which did not depend on the Uncertainty or Variety of the Seaſons of the Year. Upon comparing the Obſervations with each other, it was diſcovered, that in both the forementioned Stars, the apparent Difference of Declination from the *Maxima*, was always nearly proportional to the verſed Sine of the Sun's Diſtance from the Equinoctial Points. This was an Inducement to think, that the Cauſe, whatever it was, had ſome Relation to the Sun's Situation with reſpect to thoſe Points. But not being able to frame any Hypotheſis at that Time, ſufficient to ſolve all the Phænomena, and being very deſirous to ſearch a little farther into this Matter; I began to think of erecting an Inſtrument for myſelf at *Wanſtead*, that having it always at Hand, I might with the more Eaſe and Certainty, enquire into the Laws of this new Motion. The Conſideration likewiſe of being able by another Inſtrument, to confirm the Truth of the Obſervations hitherto made with Mr. *Molyneux*'s, was no ſmall Inducement to me; but the Chief of all was, the Opportunity I ſhould thereby have of trying, in what Manner other Stars were affected by the ſame Cauſe, whatever it was. For Mr. *Molyneux*'s Inſtrument being originally deſigned for obſerving *γ Draconis* (in order, as I ſaid before, to try whether it had any ſenſible Parallax) was ſo contrived, as to be capable of but little Alteration in its Direction, not above ſeven or eight Minutes of a Degree: and there being few Stars within half that Diſtance from the Zenith of *Kew*, bright enough to be well obſerved, he could not, with his Inſtrument, throughly examine how this Cauſe affected Stars differently ſituated with reſpect to the equinoctial and ſolſtitial Points of the Ecliptick.

Theſe Conſiderations determined me; and by the Contrivance and Direction of the ſame ingenious Perſon, Mr. *Graham*, my Inſtrument was fixed up *Auguſt* 19, 1727. As I had no convenient Place where I could make uſe of ſo long a Teleſcope as Mr. *Molyneux*'s, I contented my ſelf with one of but little more than half the Length of his (*viz.* of about 12 ½ Feet, his being 24¼) judging from the Experience which I had already had, that this Radius would be long enough to adjuſt the Inſtrument to a ſufficient Degree of Exactneſs; and I have had no Reaſon ſince to change my Opinion: for from all the Trials I have yet made, I am very well ſatisfied, that when it is carefully rectified, its Situation may be ſecurely depended upon to half a Second. As the Place where my Inſtrument was to be hung, in ſome Meaſure determined its Radius, ſo did it alſo the Length of the Arch, or Limb, on which the Diviſions were made to adjuſt it: For the Arch could not conveniently be extended farther, than to reach to about 6°¼ on each Side my Zenith. This indeed was ſufficient, ſince it gave me an Opportunity of making Choice of ſeveral Stars, very different both in Magnitude and Situation; there being more than two hundred inſerted in the *Britiſh* Catalogue, that may be obſerved with it. I needed not to have extended the Limb ſo far, but that I was willing to take in *Capella*, the only Star of the firſt Magnitude that comes ſo near my Zenith.

My Inſtrument being fixed, I immediately began to obſerve ſuch Stars as I judged moſt proper to give me light into the Cauſe of the Motion already mentioned. There was Variety enough of ſmall ones; and not leſs than twelve, that I could obſerve through all the Seaſons of the Year; they being bright enough to be ſeen in the Day-time, when neareſt the Sun. I had not been long obſerving, before I perceived, that the Notion we had before entertained of the Stars being fartheſt North and South, when the Sun was about the Equinoxes, was only true of thoſe that were near the ſolſtitial Colure: And after I had continued my Obſervations a few Months, I diſcovered, what I then apprehended to be a general Law, obſerved by all the Stars, *viz.* That each of them became ſtationary, or was fartheſt North or South, when they paſſed over my Zenith at ſix of the Clock, either in the Morning or Evening. I perceived likewiſe, that whatever Situation the Stars were in with reſpect to the cardinal Points of the Ecliptick, the apparent Motion of every one tended the ſame Way, when they paſſed my Inſtrument about the ſame Hour of the Day or Night; for they all moved Southward, while they paſſed in the Day, and Northward in the Night; ſo that each was fartheſt North, when it came about ſix of the Clock in the Evening, and fartheſt South, when it came about ſix in the Morning.

Though I have ſince diſcovered, that the *Maxima* in moſt of theſe Stars do not happen exactly when they come to my Inſtrument at thoſe Hours, yet not being able at that time to prove the contrary, and ſuppoſing that they did, I endeavoured to find out what Proportion the greateſt Alterations of Declination in different Stars bore to each other; it being very evident, that they did not all change their Declination equally. I have before taken notice, that it appeared from Mr. *Molyneux*'s Obſervations, that γ *Draconis* altered its Declination about twice as much as the fore-mentioned ſmall Star almoſt oppoſite to it; but examining the Matter more particularly, I found that the greateſt Alteration of Declination in theſe Stars, was as the Sine of the Latitude of each reſpectively. This made me ſuſpect that there might be the like Proportion between the *Maxima* of other Stars; but finding, that the Obſervations of ſome of them would not perfectly correſpond with ſuch an Hypotheſis, and not knowing, whether the ſmall Difference I met with, might not be owing to the Uncertainty and Error of the Obſervations, I deferred the farther Examination into the Truth of this Hypotheſis, till I ſhould be furniſhed with a Series of Obſervations made in all Parts of the Year; which might enable me, not only to determine what Errors the Obſervations are liable to, or how far they may ſafely be depended upon; but alſo to judge, whether there had been any ſenſible Change in the Parts of the Inſtrument itſelf.

Upon theſe Conſiderations, I laid aſide all Thoughts at that Time about the Cauſe of the fore-mentioned Phænomena, hoping that I ſhould the eaſier diſcover it, when I was better provided with proper Means to determine more preciſely what they were.

When the Year was compleated, I began to examine and compare my Obſervations, and having pretty well ſatisfied myſelf as to the general Laws of the *Phænomena*, I then endeavoured to find out the Cauſe of them. I was already convinced, that the apparent Motion of the Stars was not owing to a *Nutation* of the Earth's Axis. The next Thing that offered itſelf, was an *Alteration* in the *Direction* of the *Plumb-line*, with which the Inſtrument was conſtantly rectified; but this upon Trial proved inſufficient. Then I conſidered what *Refraction* might do, but here alſo nothing ſatisfactory occurred. At laſt I conjectured, that all the *Phænomena* hitherto mentioned, proceeded from the *progreſſive Motion of Light and the Earth's annual Motion in its Orbit.* For I perceived, that, if Light was propagated in Time, the apparent Place of a fix'd Object would not be the ſame when the Eye is at Reſt, as when it is moving in any other Direction, than that of the Line paſſing through the Eye and Object; and that, when the Eye is moving in different Directions, the apparent Place of the Object would be different.

Fig. 95. I conſidered this Matter in the following Manner. I imagined C A to be a Ray of Light, falling perpendicularly upon the Line B D; then if the Eye is at reſt at A, the Object muſt appear in the Direction A C, whether Light be propagated in Time or in an Inſtant. But if the Eye is moving from B towards A, and Light is propagated in Time, with a Velocity that is to the Velocity of the Eye, as C A to B A; then Light moving from C to A, whilſt the Eye moves from B to A, that Particle of it, by which the Object will be diſcerned, when the Eye in its Motion comes to A, is at C when the Eye is at B. Joining the Points B, C, I ſuppoſed the Line C B, to be a Tube (inclined to the Line B D in the Angle D B C) of ſuch a Diameter, as to admit of but one Particle of Light; then it was eaſy to conceive, that the Particle of Light at C (by which the Object muſt be ſeen when the Eye, as it moves along, arrives at A) would paſs through the Tube B C, if it is inclined to B D in the Angle D B C, and accompanies the Eye in its Motion from B to A; and that it could not come to the Eye, placed behind ſuch a Tube, if it had any other Inclination to the Line B D. If inſtead of ſuppoſing C B ſo ſmall a Tube, we imagine it to be the Axis of a larger; then for the ſame Reaſon, the Particle of Light at C, could not paſs through that Axis, unleſs it is inclined to B D, in the Angle C B D. In like manner, if the Eye moved the contrary way, from D towards A, with the ſame Velocity; then the Tube muſt be inclined in the Angle BDC. Although therefore the true or real Place of an Object is perpendicular to the Line in which the Eye is moving, yet the viſible Place will not be ſo, ſince that, no doubt, muſt be in the Direction of the Tube; but the Difference between the true and apparent Place will be (*cæteris paribus*) greater or leſs, according to the different Proportion between the Velocity of Light and that of the Eye. So that if we could ſuppoſe that Light was propagated in an Inſtant, then there would be no Difference between the real and viſible Place of an Object, altho' the Eye were in Motion, for in that caſe, A C being infinite with Reſpect to A B, the Angle A C B (the Difference between the true and viſible Place) vaniſhes.

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Fig. 91.

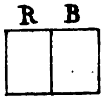


Fig. 92.

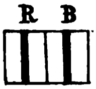
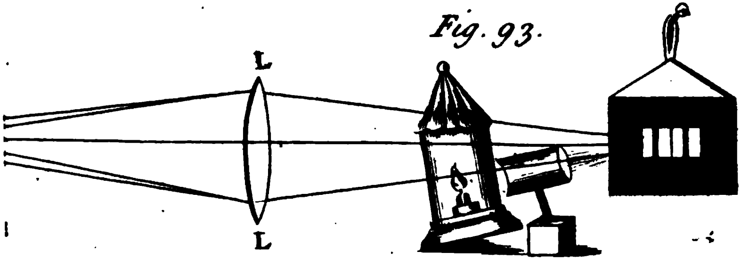


Fig. 93.



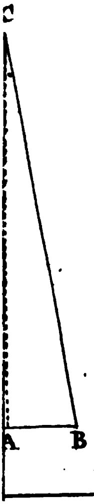
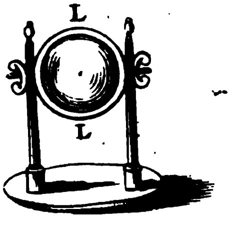


Fig. 94.



But if Light be propagated in Time (which I preſume will readily be allowed by moſt of the Philoſophers of this Age) then it is evident from the foregoing Conſiderations, that there will be always a Difference between the real and viſible Place of an Object, unleſs the Eye is moving either directly towards or from the Object. And in all Caſes, the Sine of the Difference between the real and viſible Place of the Object, will be to the Sine of the viſible Inclination of the Object to the Line in which the Eye is moving, as the Velocity of the Eye to the Velocity of Light.

If Light moved but 1000 times faſter than the Eye, and an Object (ſuppoſed to be at an infinite Diſtance) was really placed perpendicularly over the Plain in which the Eye is moving, it follows from what hath been already ſaid, that the apparent Place of ſuch an Object will be always inclined to that Plain, in an Angle of 89° 56′½; ſo that it will conſtantly appear 3′½ from its true Place, and ſeem ſo much leſs inclined to the Plain, that way towards which the Eye tends. That is, if A C is to A B (or A D) as 1000 to one, the Angle A B C will be 89° 56′½, and A C B = 3′½, and B C D = 2 A C B = 7′. So that according to this Suppoſition, the viſible or apparent Place of the Object will be altered 7′, if the Direction of the Eye's Motion is at one time contrary to what it is at another.

If the Earth revolve round the Sun annually, and the Velocity of Light were to the Velocity of the Earth's Motion in its Orbit (which I will at preſent ſuppoſe to be a Circle) as 1000 to one; then 'tis eaſy to conceive, that a Star really placed in the very Pole of the Ecliptick, would, to an Eye carried along with the Earth, ſeem to change its Place continually, and (neglecting the ſmall Difference on the Account of the Earth's diurnal Revolution on its Axis) would ſeem to deſcribe a Circle round that Pole, every Way diſtant therefrom 3′½. So that its Longitude would be varied through all the Points of the Ecliptick every Year; but its Latitude would always remain the ſame. Its Right Aſcenſion would alſo change, and its Declination, according to the different Situation of the Sun in reſpect to the equinoctial Points; and its apparent Diſtance from the North Pole of the Equator would be 7′ leſs at the Autumnal, than at the Vernal Equinox.

The greateſt Alteration of the Place of a Star in the Pole of the Ecliptick (or which in Effect amounts to the ſame, the Proportion between the Velocity of Light and the Earth's Motion in its Orbit) being known; it will not be difficult to find what would be the Difference upon this Account, between the true and apparent Place of any other Star at any time; and on the contrary, the Difference between the true and apparent Place being given, the Proportion between the Velocity of Light and the Earth's Motion in its Orbit may be found.

As I only obſerved the apparent Difference of Declination of the Stars, I ſhall not now take any farther Notice in what manner ſuch a Cauſe as I have here ſuppoſed would occaſion an Alteration in their apparent Places in other Reſpects; but, ſuppoſing the Earth to move equally in a Circle, it may be gathered from what hath been already ſaid, that a Star which is neither in the Pole nor Plain of the Ecliptick, will ſeem to deſcribe about its true Place a Figure, inſenſibly different from an Ellipſe, whoſe Tranſverſe Axis is at a Right-angle to the Circle of Longitude paſſing through the Star's true Place, and equal to the Diameter of the little Circle deſcribed by a Star (as was before ſuppoſed) in the Pole of the Ecliptick; and whoſe Conjugate Axis is to its Tranſverſe Axis, as the *Sine* of the Star's Latitude to the *Radius*. And allowing that a Star by its apparent Motion does exactly deſcribe ſuch an Ellipſe, it will be found, that if A be the Angle of Poſition (or the Angle at the Star made by two great Circles drawn from it, thro' the Poles of the Ecliptick and Equator) and B be another Angle, whoſe Tangent is to the Tangent of A as Radius to the Sine of the Latitude of the Star; then B will be equal to the Difference of Longitude between the Sun and the Star, when the true and apparent Declination of the Star are the ſame. And if the Sun's Longitude in the Ecliptick be reckoned from that Point, wherein it is when this happens; then the Difference between the true and apparent Declination of the Star (on Account of the Cauſe I am now conſidering) will be always as the Sine of the Sun's Longitude from thence. It will likewiſe be found that the greateſt Difference of Declination that can be between the true and apparent Place of the Star, will be to the Semi-Tranſverſe Axis of the Ellipſe (or to the Semidiameter of the little Circle deſcribed by a Star in the Pole of the Ecliptick) as the Sine of A to the Sine of B.

If the Star hath North Latitude, the Time, when its true and apparent Declination are the ſame, is before the Sun comes in Conjunction with or Oppoſition to it, if its Longitude be in the firſt or laſt Quadrant (*viz.* in the aſcending Semi-circle) of the Ecliptick; and after them, if in the deſcending Semi-circle; and it will appear neareſt to the North Pole of the Equator, at the Time of that *Maximum* (or when the greateſt Difference between the true and apparent Declination happens) which precedes the Sun's Conjunction with the Star.

Theſe Particulars being ſufficient for my preſent Purpoſe, I ſhall not detain you with the Recital of any more, or with any farther Explication of theſe. It may be time enough to enlarge more upon this Head, when I give a Deſcription of the Inſtruments, *&c.* if that be judged neceſſary to be done; and when I ſhall find, what I now advance, to be allowed of (as I flatter myſelf it will) as ſomething more than a bare Hypotheſis. I have purpoſely omitted ſome matters of no great Moment, and conſidered the Earth as moving in a Circle, and not an Ellipſe, to avoid too perplexed a *Calculus*, which after all the Trouble of it would not ſenſibly differ from that which I make uſe of, eſpecially in thoſe Conſequences which I ſhall at preſent draw from the foregoing Hypotheſis.

This being premiſed, I ſhall now proceed to determine from the Obſervations, what the real Proportion is between the Velocity of Light and the Velocity of the Earth's annual Motion in its Orbit; upon Suppoſition that the *Phænomena* before mentioned do depend upon the Cauſes I have here aſſigned. But I muſt firſt let you know, that in all the Obſervations hereafter mentioned, I have made an Allowance for the Change of the Star's Declination on Account of the Preceſſion of the Equinox, upon Suppoſition that the Alteration from this Cauſe is proportional to the Time, and regular through all the Parts of the Year. I have deduced the real annual Alteration of Declination of each Star from the Obſervations themſelves; and I the rather chooſe to depend upon them in this Article, becauſe all which I have yet made, concur to prove, that the Stars near the Equinoctial Colure, change their Declination at this time 1″½ or 2″ in a Year more than they would do if the Preceſſion was only 50″, as is now generally ſuppoſed. I have likewiſe met with ſome ſmall Varieties in the Declination of other Stars in different Years, which do not ſeem to proceed from the ſame Cauſe, particularly in thoſe that are near the ſolſtitial Colure, which on the contrary have altered their Declination leſs than they ought, if the Preceſſion was 50″. But whether theſe ſmall Alterations proceed from a regular Cauſe, or are occaſioned by any Change in the Materials, &c. of my Inſtrument, I am not yet able fully to determine. However, I thought it might not be amiſs juſt to mention to you how I have endeavoured to allow for them, though the Reſult would have been nearly the ſame, if I had not conſidered them at all. What that is, I will ſhew, firſt from the Obſervations of γ *Draconis*, which was found to be 39″ more Southerly in the Beginning of *March*, than in *September*.

From what hath been premiſed, it will appear that the greateſt Alteration of the apparent Declination of γ *Draconis*, on Account of the ſucceſſive Propagation of Light, would be to the Diameter of the little Circle which a Star (as was before remarked) would ſeem to deſcribe about the Pole of the Ecliptick, as 39″ to 40″,4. The half of this is the Angle A C B (as repreſented in the *Fig.*) This therefore being 20″,2, A C will be to A B, that is, the Velocity of Light to the Velocity of the Eye (which in this Caſe may be ſuppoſed the ſame as the Velocity of the Earth's annual Motion in its Orbit) as 10210 to One, from whence it would follow, that Light moves, or is propagated as far as from the Sun to the Earth in 8′ 12″.

It is well known, that Mr. *Romer*, who firſt attempted to account for an apparent Inequality in the Times of the Eclipſes of *Jupiter*'s Satellites, by the Hypotheſis of the progreſſive Motion of Light, ſuppoſed that it ſpent about 11 Minutes of Time in its Paſſage from the Sun to us: but it hath ſince been concluded by others from the like Eclipſes, that it is propagated as far in about 7 Minutes. The Velocity of Light therefore deduced from the foregoing Hypotheſis, is as it were a *Mean* betwixt what had at different times been determined from the Eclipſes of *Jupiter*'s Satellites.

Theſe different Methods of finding the Velocity of Light thus agreeing in the Reſult, we may reaſonably conclude, not only that theſe *Phænomena* are owing to the Cauſes to which they have been aſcribed; but alſo that Light is propagated (in the ſame *Medium*) with the ſame Velocity after it hath been reflected as before: for this will be the Conſequence, if we allow that the Light of the Sun is propagated with the ſame Velocity, before it is reflected, as the Light of the *Fix'd Stars*. And I imagine this will ſcarce be queſtioned, if it can be made appear that the Velocity of the Light of all the *Fix'd Stars* is equal, and that their Light moves or is propagated through equal Spaces in equal Times, at all Diſtances from them: both which Points (as I apprehend) are ſufficiently proved from the apparent Alteration of the Declination of Stars of different Luſtre; for that is not ſenſibly different in ſuch Stars as ſeem near together, though they appear of very different Magnitudes. And whatever their Situations are (if I proceed according to the foregoing Hypotheſis) I find the ſame Velocity of Light from my Obſervations of ſmall Stars of the fifth or ſixth, as from thoſe of the ſecond and third Magnitude, which in all probability are placed at very different Diſtances from us. The ſmall Star, for Example, before ſpoken of, that is almoſt oppoſite to γ *Draconis* (being the 35th *Camelopard. Hevelii* in Mr. *Flamſteed*'s Catalogue) was 19″ more Northerly about the Beginning of *March* than in *September*. Whence I conclude, according to my Hypotheſis, that the Diameter of the little Circle deſcribed by a Star in the Pole of the Ecliptick would be 40″, 2.

The laſt Star of the great Bear's-tail of the 2d Magnitude (marked η by *Bayer*) was 36″ more Southerly about the Middle of *January* than in *July*. Hence the *Maximum*, or greateſt Alteration of Declination of a Star in the Pole of the Ecliptick would be 40″,4, exactly the ſame as was before found from the Obſervations of γ *Draconis*.

The Star of the 5th Magnitude in the Head of *Perſeus* marked τ by *Bayer*, was 25″ more Northerly about the End of *December* than on the 29th of *July* following. Hence the *Maximum* would be 41″. This Star is not bright enough to be ſeen as it paſſes over my Zenith about the End of *June*, when it ſhould be according to the Hypotheſis fartheſt South. But becauſe I can more certainly depend upon the greateſt Alteration of Declination of thoſe Stars, which I have frequently obſerved about the Times, when they become ſtationary, with reſpect to the Motion I am now conſidering; I will ſet down a few more Inſtances of ſuch, from which you may be able to judge how near it may be poſſible from theſe Obſervations, to determine with what Velocity Light is propagated.

α *Perſei Bayero* was 23″ more Northerly at the beginning of *January* than in *July*. Hence the *Maximum* would be 40″, 2.

α *Caſſiopeæ* was 34″ more Northerly about the End of *December* than in *June*. Hence the *Maximum* would be 40″, 8.

β *Draconis* was 39″ more Northerly in the beginning of *September* than in *March*; hence the *Maximum* would be 40″, 2.

Capella was about 16″ more Southerly in *Auguſt* than in *February*; hence the *Maximum* would be about 40″. But this Star being farther from my Zenith than thoſe I have before made uſe of, I cannot ſo well depend upon my Obſervations of it, as of the others; becauſe I meet with ſome ſmall Alterations of its Declination that do not ſeem to proceed from the Cauſe I am now conſidering.

I have compared the Obſervations of ſeveral other Stars, and they all conſpire to prove that the *Maximum* is about 40″ or 41″. I will therefore ſuppoſe that it is 40″½ or (which amounts to the ſame) that Light moves, or is propagated as far as from the Sun to us in 8′ 13″. The near Agreement which I met with among my Obſervations induces me to think, that the *Maximum* (as I have here fixed it) cannot differ ſo much as a Second from the Truth, and therefore it is probable that the Time which Light ſpends in paſſing from the Sun to us, may be determined by theſe Obſervations within 5″ or 10″; which is ſuch a Degree of exactneſs as we can never hope to attain from the Eclipſes of *Jupiter*'s Satellites.

Having thus found the *Maximum*, or what the greateſt Alteration of Declination would be in a Star placed in the Pole of the Ecliptick, I will now deduce from it (according to the foregoing Hypotheſis) the Alteration of Declination in one or two Stars, at ſuch times as they were actually obſerved, in order to ſee how the Hypotheſis will correſpond with the *Phænomena* through all the Parts of the Year.

It would be too tedious to ſet down the whole Series of my Obſervations; I will therefore make Choice only of ſuch as are moſt proper for my preſent Purpoſe, and will begin with thoſe of γ *Draconis*.

This Star appeared fartheſt North about *September* 7th, 1727, as it ought to have done according to my Hypotheſis. The following Table ſhews how much more Southerly the Star was found to be by Obſervation in ſeveral Parts of the Year, and likewiſe how much more Southerly it ought to be according to the Hypotheſis.

1727.	D.	The Difference of Declination by Obſervation. "	The Difference of Declination by the Hypotheſis. "	1728.	D.	The Difference of Declination by Obſervation. "	The Difference of Declination by the Hypotheſis. "
October	20	4½	4½	*March*	24	37	38
November	17	11½	12	*April*	6	36	36½
December	6	17½	18½	*May*	6	28½	29½
— —	28	25	26	*June*	5	18½	20
1728				— —	15	17½	17
January	24	34	34	*July*	3	11½	11½
February	10	38	37	*Auguſt*	2	4	4
March	7	39	39	*September*	6	0	0

Hence it appears, that the Hypotheſis correſponds with the Obſervations of this Star through all Parts of the Year; for the ſmall Differences between them ſeem to ariſe from the Uncertainty of the Obſervations, which is occaſioned (as I imagine) chiefly by the tremulous or undulating Motion of the Air, and of the Vapours in it; which cauſes the Stars ſometimes to dance to and fro, ſo much that it is difficult to judge when they are exactly on the Middle of the Wire that is fixed in the common Focus of the Glaſſes of the Teleſcope.

I muſt confeſs to you, that the Agreement of the Obſervations with each other, as well as with the Hypotheſis, is much greater than I expected to find, before I had compared them; and it may poſſibly be thought to be too great, by thoſe who have been uſed to Aſtronomical Obſervations, and know how difficult it is to make ſuch as are in all reſpects exact. But if it would be any Satisfaction to ſuch Perſons (till I have an Opportunity of deſcribing my Inſtrument and the manner of uſing it) I could aſſure them, that in above 70 Obſervations which I made of this Star in a Year, there is but one (and that is noted as very dubious on account of Clouds) which differs from the foregoing Hypotheſis more than 2", and this does not differ 3".

This therefore being the Fact, I cannot but think it very probable, that the *Phænomena* proceed from the Cauſe I have aſſigned, ſince the foregoing Obſervations make it ſufficiently evident, that the Effect of the real Cauſe, whatever it is, varies in this Star, in the ſame Proportion that it ought according to the Hypotheſis.

But leſt γ *Draconis* may be thought not ſo proper to ſhew the Proportion, in which the apparent Alteration of Declination is increaſed or diminiſhed, as thoſe Stars which lie near the Equinoctial Colure: I will give you alſo the Compariſon between the Hypotheſis and the Obſervations of η *Urſæ Majoris*, which was fartheſt South about the 17th Day of *January* 1728, agreeable to the Hypotheſis. The following Table ſhews how much more Northerly it was found by Obſervation in ſeveral Parts of the Year, and alſo what the Difference ſhould have been according to the Hypotheſis.

1727.	D.	The Difference of Declination by Obſervation. ″	The Difference of Declination by the Hypotheſis. ″	1728.	D.	The Difference of Declination by Obſervation. ″	The Difference of Declination by the Hypotheſis. ″
September -	14	29 ½	28 ½	*April* - -	16	18 ½	18
— —	24	24 ½	25 ½	*May* - -	5	24 ½	23 ½
October —	16	19 ½	19 ½	*June* - -	5	32	31 ½
November -	11	11 ½	10 ½	— —	25	35	34 ½
December -	14	4	3	*July* - -	17	36	36
1728							
February -	17	2	3	*Auguſt* - -	2	35	35 ½
March - -	21	11 ½	10 ½	*September* -	20	26 ½	26 ½

I find upon Examination, that the Hypotheſis agrees altogether as exactly with the Obſervations of this Star, as the former; for in about 50 that were made of it in a Year, I do not meet with a Difference of ſo much as 2″, except in one, which is mark'd as doubtful on Account of the Undulation of the Air, *&c.* And this does not differ 3″ from the Hypotheſis.

The Agreement between the Hypotheſis and the Obſervations of this Star is the more to be regarded, ſince it proves that the Alteration of Declination, on account of the Preceſſion of the Equinox, is (as I before ſuppoſed) regular thro' all Parts of the Year; ſo far at leaſt, as not to occaſion a Difference great enough to be diſcovered with this Inſtrument. It likewiſe proves the other part of my former Suppoſition, *viz.* that the annual Alteration of Declination in Stars near the Equinoctial Colure, is at this Time greater than a Preceſſion of 50″ would occaſion: for this Star was 20″ more Southerly in *September* 1728, than in *September* 1727, that is, about 2″ more than it would have been, if the Preceſſion was but 50″. But I may hereafter, perhaps, be better able to determine this Point, from my Obſervations of thoſe Stars that lie near the Equinoctial Colure, at about the ſame Diſtance from the North Pole of the Equator, and nearly oppoſite in right Aſcenſion.

I think it needleſs to give you the Compariſon between the Hypotheſis and the Obſervations of any more Stars; ſince the Agreement in the foregoing is a kind of Demonſtration (whether it be allowed that I have diſcovered the real Cauſe of the *Phænomena* or not;) that the Hypotheſis gives at leaſt the true Law of the Variation of Declination in different Stars, with Reſpect to their different Situations and Aſpects with the Sun. And if this is the Caſe, it muſt be granted, that the Parallax of the Fix'd Stars is much ſmaller, than hath been hitherto ſuppoſed by thoſe, who have pretended to deduce it from their Obſervations. I believe, that I may venture to ſay, that in either of the two Stars laſt mentioned, it does not amount to 2″. I am of Opinion, that if it were 1″, I ſhould have perceived it, in the great number of Obſervations that I made, eſpecially of γ *Draconis*; which agreeing with the Hypotheſis (without allowing any thing for Parallax) nearly as well when the Sun was in Conjunction with, as in Oppoſition to, this Star, it ſeems very probable that the Parallax of it is not ſo great as one ſingle Second; and conſequently that it is above 400000 times farther from us than the Sun.

P. S. As to the Obſervations of Dr. *Hook*, I muſt own that before Mr. *Molyneux*'s Inſtrument was erected, I had no ſmall Opinion of their Correctneſs; the Length of his Teleſcope and the Care he pretends to have taken in making them exact, having been ſtrong Inducements with me to think them ſo. And ſince I have been convinced both from Mr. *Molyneux*'s Obſervations and my own, that the Doctor's are really very far from being either exact or agreeable to the *Phænomena*; I am greatly at a Loſs how to account for it. I cannot well conceive that an Inſtrument of the Length of 36 Feet, conſtructed in the Manner he deſcribes his, could have been liable to an Error of near 30″ (which was doubtleſs the Caſe) if rectified with ſo much Care as he repreſents.

The Obſervations of Mr. *Flamſteed* of the different Diſtances of the Pole Star from the Pole at different Times of the Year, which were through Miſtake looked upon by ſome as a Proof of the annual *Parallax* of it, ſeem to have been made with much greater Care than thoſe of Dr. *Hook*. For though they do not all exactly correſpond with each other, yet from the whole, Mr. *Flamſteed* concluded that the Star was 35″, 40″, or 45″ nearer the Pole in *December* than in *May* or *July*: and according to my Hypotheſis it ought to appear 40″ nearer in *December* than in *June*. The Agreement therefore of the Obſervations with the Hypotheſis is greater than could reaſonably be expected, conſidering the *Radius* of the Inſtrument, and the Manner in which it was conſtructed.

Mr. Caſſini'*s Eſſay on the Parallax and Magnitude of* Sirius *conſidered by Dr.* Halley. N°364. *p.* 1.

IV. In the *Memoires* of the *Royal Academy* of *Paris*, for the Year 1717. there is one very remarkable Eſſay, by Mr. *Caſſini*, concerning the *Annual Parallax* of the *Fix'd Stars*, and particularly of *Sirius*; and in Concluſion, he determines the Diameter of *Sirius* to be as much bigger than that of the *Sun*, as the *Sun*'s is greater than that of the *Earth*, which he ſuppoſes to be 100 times: And the Diſtance from the *Sun* to the *Earth* being certainly about 100 Diameters of the *Sun*, it will follow, that the Globe of *Sirius* muſt be a Sphere, whoſe Diameter muſt equal the Diſtance between the *Earth* and *Sun*.

To prove this, he tells us that he made uſe of an excellent Teleſcope of 34 *French* Feet, or 36 *Engliſh*, leaving an Aperture of but an Inch and half, to take off the ſpurious Rays of the *Star*, which then appeared round, and ſufficiently well defined; and comparing his body to that of *Jupiter*, which he ſays, was then 50 Seconds Diameter, he found that the Diameter of *Jupiter* was ten times greater than that of the Star, which by conſequence was ſeen under an Angle of about 5 Seconds; which is his firſt Poſition.

Then he tells us, that to make the Obſervations of the Parallax of this Star with all the exactneſs poſſible, he employed a Teleſcope of *three Foot*, in a Copper Tube, having fixed in the common *Focus* of the two Glaſſes, four Threads croſſing one another in the Center, under Angles of 45 Degrees. This Tube he firmly fix'd to the Plain of a *Mural Arch*, which had been for above 30 Years immoveably cemented to the Wall of the *Royal Obſervatory*, ſo that there was no fear of its ſettling any further in the Space of one Year; beſides, that it was eaſy to perceive if any ſuch Alteration ſhould happen to it.

Having therefore fix'd his three Foot Tube, as above, ſo that, about the Beginning of *April*, 1714. *New Stile*, the Star being exactly in the *Meridian*, paſt over the Center of the Tube, he obſerved that on the 20th of *April* the Star touched the Horizontal Thread with its under-Edge, being apparently all above it, in the inverting Tube, but really below. On the 15th of *May*, and 6th of *June*, it paſt again by the Center. On *June* the 27th it appeared a little under, and on *July* the 9th it was found to touch the under Part of the Thread. On *October* the 5th it again paſt by the Center; but on *December* the 29th, it touched the upper Part of the Thread. *January* the 18th, 1715. being the coldeſt Day of that Winter, it paſt exactly by the Center; and on the 27th of *March*, and the 1ſt of *April*, it almoſt touched the upper Side of the Horizontal Thread, from which it ſeem'd a little ſeparated. But on *June* the 7th, it paſt a little under the Center; and on *June* the 29th, the *Sun* being then in conjunction with *Sirius*, it paſt under the Thread, ſo as to touch it with its upper Edge. Whence it appears, that in the Space of the whole Year, there had been no other variation of the *Meridian* Altitude of *Sirius*, than the Breadth of the Thread, which appear'd equal to the Diameter of the Star, which he takes to be five, or at moſt ſix Seconds.

Suppoſing this to be ſo, he then ſhews that the whole Diameter of the annual Orb is to the Diſtance of *Sirius*, as the Sine of 6″ to the Sine of 39° 33′ the Latitude of the Star, whence the aforeſaid immenſe Magnitude of the Body thereof, is a neceſſary Conſequence.

But before this obtain a full Aſſent, it may not perhaps be amiſs to enquire whether the ſuppos'd viſible Diameter of *Sirius*, were not an Optick Fallacy, occaſioned by the great Contraction of the *Aperture* of the *Object Glaſs*: For we all know that the Diameters of *Aldebaran* and *Spica Virginis*, are ſo ſmall, that when they happen to immerge on the dark Limb of the *Moon*, they are ſo far from loſing their Light gradually, as they muſt do were they of any ſenſible Magnitude, that they vaniſh at once with their utmoſt Luſtre; and emerge likewiſe in a Moment, not ſmall at firſt, but at once appear with their full Light, even though the Emerſion happen very near the *Cuſp*; where, if they were four Seconds in Diameter, they would be many Seconds of Time in getting entirely ſeparated from the Limb. But the contrary appears to all thoſe, that have obſerved the Occultations of theſe bright Stars. And though *Sirius* be bigger than either of them, yet he is by far leſs than two of them; and conſequently his Diameter to theirs is leſs than the Square Root of 2 to 1, or than 14 to 10; whence, in Mr. *Caſſini*'s excellent 36 Foot Glaſs, thoſe Stars ought to be about four Seconds in Diameter; and they would undoubtedly appear ſo, if view'd after the ſame manner; whereas we are *aliundè* certain, that they are leſs than one ſingle Second in Diameter. The great ſtrength of their native Light, forming the reſemblance of a Body, when it is nothing elſe but the ſpiſſitude of their Rays.

As to the other Part of the Argument, that the Alteration of the Declination of *Sirius*, on the Score of the Acceſs of the Earth in *December*, and its Receſs in *June*, amounts to 6 Seconds; I can only remark, that, beſides that a *Radius* of 3 Feet, as it ſeems that made uſe of was no more, is ſomewhat too ſmall for ſo extremely nice an Obſervation, 6″ being ſubtended by the $\frac{1}{1000}$ Part of an Inch, ſome of the Obſervations before recited do plainly ſhew, that the *Refraction* of the *Medium* did intermix with thoſe Differences that might be occaſioned by the *Parallax*.

But the principal Objection againſt the Concluſion of this Argument, ſeems to be, that the Meridian altitude of *Sirius* at *Paris* being under 25 Degrees, the ordinary Refraction of the Star is 1′ 55″ or 115 Seconds; and the Barometer riſing and falling above two Inches in Thirty, ſhews that the *Denſity* of the *Air*, on that ſcore, may be a 15th Part more at one time than another. Whence the Refractions being always proportional to the *Denſity* of the *Medium*, as we have all ſeen it often demonſtrated by Mr. *Hawkſbee*, both in *Vacuo*, and in a *doubly* and *trebly condenſed Air*, it is plain that in that Altitude the Refraction of a Star may differ about 7 or 8 Seconds, or the 15th Part of 115″, which is more than the whole *Parallax* ſuppoſed to have been obſerved.

It were to be wiſh'd that Mr. *Caſſini* would pleaſe to try this Matter by the *Lucida Lyræ*, inſtead of *Sirius*, which, though ſomewhat leſs than him, is as near to the *Solſtitial Colure*, and has much greater Latitude, being but 28 *grad.* from the *Pole* of the *Ecliptick*, whence its Parallax would be ſo much greater, and being at *Paris* within 10 *grad.* of the *Zenith*, the grand Objection of the difference of Refraction, would be almoſt wholly removed.

Remarks upon Obſervations, by Croſs-Hairs in a Teleſcope, by Dr. Halley. N° 366. *p.* 113.

V. We are greatly obliged to the late Signior *Caſſini* for his Thought of applying Threads at half Right Angles in the common *Focus* of a Teleſcope, to determine thereby the Differences of Right Aſcenſion and Declination of any two Stars, whoſe ſituation is ſuch, that by their diurnal Motion they follow each other thro' the Aperture of the Teleſcope, ſo fixt as that the firſt of them may paſs over the Centre of the Glaſs, and move exactly along one of the Threads, whilſt the interval of Time between the Tranſit thereof, and that of the following Star, is exactly meaſured by a *Pendulum* Clock well adjuſted to the mean Motion of the Sun, or elſe to the Revolution of the fix'd Stars, whereby the Difference of Right Aſcenſion is given; as is the Difference of Declination, by the time the following Star takes to paſs from one diagonal Thread to the other.

This manner of obſerving being long ſince publiſhed, will not need any further Explication; but it may not be amiſs to ſay ſomething of the *Sufficiency* thereof, and of the *Exactneſs* of which an Inſtrument of ſo little Charge and *Apparatus* is capable; eſpecially being at this time obliged to make uſe of it and the Micrometer only, for my Obſervations.

I need not mention with what exactneſs Dr. *Pound*, and his Nephew Mr. *Bradley* did, myſelf being preſent, in the laſt Oppoſition of the Sun and *Mars*, this way demonſtrate the extream Minuteneſs of the Sun's Parallax, and that it was not more than 12″, nor leſs than 9″, upon many repeated Trials, it having been ſoon after the time laid before the Society. But being mindful that in *October* next, *Mars* would be again in Oppoſition to the Sun, about the tenth Degree of *Taurus*, but would not come very near any fixt Star that has a Place in Mr. *Flamſteed*'s Catalogue; I was ſolicitous to ſee if there were any Teleſcopick Stars to which he would very nearly approach; and on the 28th of *February* laſt, the Heavens being very ſerene and clear in the Evening, and *Venus* having nearly the Declination in which *Mars* will move in *October* next, I fixt my Teleſcope on her, at 7^{h}. 28′ equal time, and noted the Moment ſhe paſs'd over the Center of my Glaſs, or rather the common interſection of the four croſs Hairs; and in half an Hour's time I noted eight very conſpicuous Stars, four of which being within the compaſs of one Degree, fell very nearly in the ſaid way of *Mars*, and from the Intervals of Time, I then obſerved, with their difference of Declination from *Venus*, I determined their Right Aſcenſions and Declinations, as well as her Place from my Tables, (which by Obſervation I found at this time needed no correction) would allow me; they all falling between the ninth and tenth Degree of *Taurus*, with very little Latitude. But what confirm'd me that all was right, was, that on *Tueſday* laſt, *March* 21, *Mercury* appearing very fair, and newly paſt his greateſt Elongation, I found by *Senex*'s Zodiack that he was nearly in the ſame Parallel that *Venus* had before deſcribed; and though the Brightneſs of the *Crepuſculum* effaced the ſmaller Stars, yet in a quarter of an Hour I had one paſt 10′ $\frac{1}{4}$ more Southerly than the Planet, which in leſs than 3′ of Time was ſucceeded by another, which was but one Minute more Northerly than the former; when after an interval of about 14 Minutes of time, in which I was ſurpriſed to find the Sky ſo void of Stars, the four before mentioned Stars paſt ſucceſſively over my Glaſs, with the ſame interval of Time in which I had ſeen them follow one another, on the 28th of *February*; whereupon I was deſirous to try, whether, if the Place of *Mercury* in my Tables were aſſumed, the ſame Right Aſcenſions and Declinations of thoſe Stars would be deduced from him, as from *Venus*; and to my great Satisfaction, I found on trial by an exact *Calculus*, that I had the ſame Right Aſcenſions now as before, in none of the four differing fully half a Minute, ſo that theſe Stars may ſecurely be added to the Catalogue, and the Appulſe of *Mars* to them be obſerved in very long Teleſcopes, in *October* next, to a further aſcertaining the immenſe Diſtance between the Sun and Earth.

Hence it will alſo appear that our *Mercurial* Numbers are, at leaſt at this time, and in this Part of his Orb, not leſs exact than thoſe of *Venus*. And whereas this Planet ſcarce ever appears with us out of the Sun's Beams, and always low, and therefore under great Refraction; this way of obſerving takes off all the uncertainty that accrues therefrom; and when once the *Zodiack* ſhall be compleated with the Stars that are wanting to fill up the vacant Places, it will be eaſy at any time, by this method, to obſerve *Mercury* or a *Comet* within the Sun's Beams, with the ſame Certainty, as if it were remote, and out of the Neighbourhood of the Horizon, where the different Vapours that lie near the Earth, render the Appearances of the Stars ſomewhat dubious upon the Account of the irregular Refractions.

March 23. 1721.

Remarks on the Aſtronomical Allowances to be made for the Refraction of the Air, with an accurate Table of Refractions. N° 368. *p.* 169.

VI. Were the *Medium* of our *Air* much more in Quantity, or the Force of *Gravity* much greater than it is, or in a Word, were the Refractive Power of the Air much more ſenſible than we find it, nothing could have been a greater Impediment to Diſcoveries in Aſtronomy: For all Objects appearing by Refraction higher than really they are, till ſuch times as the Laws and Quantity of that Refraction had been aſcertained, it would have been impoſſible to have been ſecure of the true obſerved Place of any Cœleſtial Object. But as it falls out to be ſo little, that none but nice Inſtruments can perceive its Effects, it was not diſcovered to be at all, till *Bernard Walther*'s time, about the Year 1500; nor brought to any ſort of Rule till *Tycho Brahe*; nor aſcertained, till our worthy *Preſident* made the firſt accurate Table thereof: The *Curve* which a Beam of *Light* deſcribes, as it approaches the *Earth*, being one of the moſt perplex'd and intricate that can well be propoſed, as Dr. *Brook Taylor* in the laſt Propoſition of his *Methodus Incrementorum* has made it evident.

By this Table it follows that the *ratio* of the Sine of the Angle of Incidence to that of the Refracted Angle, encreaſing as the Beam approaches, makes a very notable difference in the Place of an Object near the Horizon: but in Objects that are much elevated, the Refractions become ſmall, and their Differences ſcarce exceed a Second *per* Degree; ſo that they are ſufficiently the ſame, as if the Incident and Refracted Angles were on the Surface of a Sphere of Air of the ſame uniform Denſity cloſe adjoining to the Eye.

When therefore the Stars are twenty degrees or upwards elevated above the Horizon, we may take it for granted, without ſenſible Error, that the Sines of the true and apparent Diſtances from the *Vertex*, are in the ſame conſtant *ratio*. Hence it will appear that the Diſtances of all the Stars are ſeen leſs than they really are, in whatever poſition they are taken, and that not leſs than a Second *per* Degree of the diſtance; that is, a Diſtance of 30 Degrees, for Example, is contracted at leaſt ſo many Seconds, and one of 60 gr. no leſs than a Minute, if the Diſtances be taken by an Inſtrument that is truly divided. So that when Mr. *Hevelius*, to ſhew the exactneſs of his Obſervations, brings eight Diſtances, as taken by his *Sextant*, which exactly compleat the

Circle, both in Longitude and Right Aſcenſion; the Conſequence is really quite oppoſite to his Deſign: for if thoſe diſtances were the true ones, they being all contracted by appearing through a refracting *Medium*, the Sum of the eight Differences of both Longitude and Right Aſcenſion, ought to fall ſhort of a whole Circle or 360 Degrees by at leaſt ſix Minutes; ſo that I am inclined to believe that the ſixty Degrees of Mr. *Hevelius*'s Sextant wanted about a Minute of its true quantity.

Such an allowance as this may perhaps be a proper Expedient to avoid accounting for Refraction in cœleſtial Obſervations, provided the Objects be nearly parallel to the Horizon, or at a good height above it. For all diſtances of Stars are contracted by Refraction, when they are parallel to the Horizon, by the ſame conſtant quantity, be they high or low, that is, by about one Second *per* Degree; the Chords of the Arches of the real and viſible diſtances being always in the ſame *ratio* as is the Sine of the Angle of Incidence to that of the refracted Angle.

And this is the caſe wherein the *Refraction* of the *Air* does leaſt affect the Diſtances of the *Stars*, which Diſtances are ſtill more and more contracted, as they are nearer to a perpendicular Situation: So that a Diſtance, for Example, of thirty Degrees loſes but half a Minute in a horizontal Site; but if the one Star be 20 Degrees high, and the other fifty, it will be leſſened by above three times as much, or by 1 Minute 41 Seconds. If the one be 30 and the other 60 Degrees high, the ſame Diſtance will appear leſs than 30 Degrees by about one Minute; the Difference ſtill decreaſing as the Objects are more elevated above the Horizon. But in all caſes to account for the effect of the *Refraction* upon the Diſtances of the Stars, requires, beſides ſome Trigonometrical Work, the help of the aforementioned Table, which I here ſubjoin for the uſe of the Curious, ſuch as I long ſince received it from its Great Author; it having never yet, that I know of, been made publick.

Tabula Refractionum Siderum ad Altitudines apparentes.

Alt. Appar. deg. m.	Refractio. m. ſec.	Alt. Appar. deg.	Refrac. tio. m. ſec.	Alt. Appar. deg.	Refractio. m. ſec.
0 0	33 45	16	3 4	46	0 52
0 15	30 24	17	2 53	47	0 50
0 30	27 35	18	2 43	48	0 48
0 45	25 11	19	2 34	49	0 47
1 0	23 7	20	2 26	50	0 45
1 15	21 20	21	2 18	51	0 44
1 30	19 46	22	2 11	52	0 42
1 45	18 22	23	2 5	53	0 40
2 0	17 8	24	1 59	54	0 39
2 30	15 2	25	1 54	55	0 38
3 0	13 20	26	1 49	56	0 36
3 30	11 57	27	1 44	57	0 35
4 0	10 48	28	1 40	58	0 34
4 30	9 50	29	1 36	59	0 32
5 0	9 2	30	1 32	60	0 31
5 30	8 21	31	1 28	61	0 30
6 0	7 45	32	1 25	62	0 28
6 30	7 14	33	1 22	63	0 27
7 0	6 47	34	1 19	64	0 26
7 30	6 22	35	1 16	65	0 25
8 0	6 0	36	1 13	66	0 24
8 30	5 40	37	1 11	67	0 23
9 0	5 22	38	1 8	68	0 22
9 30	5 6	39	1 6	69	0 21
10 0	4 52	40	1 4	70	0 20
11 0	4 27	41	1 2	71	0 19
12 0	4 5	42	1 0	72	0 18
13 0	3 47	43	0 58	73	0 17
14 0	3 31	44	0 56	74	0 16
15 0	3 17	45	0 54	75	0 15

On the Places of the Planets determin'd by their Appulſes to the Fix'd Stars, by Dr. Halley. N° 369. *p.* 209.

VII. No Cœleſtial Obſervations are ſo capable of perfect Exactneſs, as the near Appulſes of the Moon and Planets to the Fixed Stars; for though the Places of the Stars have not as yet attained an ultimate Preciſion, yet theſe Sorts of Obſervations are ever good, and the Places of the Planets are hereby aſcertain'd, in Proportion to the Correctneſs of the Catalogues that may hereafter be made: But the ordinary Number of the Stars, with which the Planets may be thus compared, being ſmall, the Opportunities of obſerving are conſequently rare: Whence appears the great Uſe of a full Catalogue of Teleſcopical Stars, within the Limits of the Zodiack; *viz.* that thereby theſe Opportunities may be more frequent: And wherever ſuch Obſervations have formerly been made to theſe ſmall Stars, we may be enabled to find them out, and by determining their Places, to be certain of the Places of the Planets alſo: Of which I have given a notable Inſtance * in finding the Place of the great Comet of 1680, in its firſt Appearance, even before it had a Tail viſible to the naked Eye.

Since the *Royal Obſervatory* at *Greenwich* has been put under my Care, I have endeavoured to put myſelf into a Condition to ſupply the many and great Vacancies to be met with in the preſent Zodiack; and particularly I have ſought out and ſettled the Places of two Teleſcopick Stars, to one of which, *Jupiter* was obſerved to apply by *Galileo* at the Beginning of *March* 1610, *New Stile*, and which is the very firſt Obſervation of that kind that was made with the Teleſcope †.

On the 28th of *February*, one Hour after Sun-ſet, a ſmall Fix'd Star was in Conjunction with the fourth Satellite, being then Eaſtwards of the Planet. The next Day, *Mart.* 1°. at the ſame Hour, the Center of ♃ was in the Angle of an equilateral Triangle with the fourth Satellite and the Star: And again, *March* 2°. *Jupiter* being retrograde, had paſt the Conjunction of the Star, and a Line from the Star, perpendicular to that of the Satellites, fell on the firſt Satellite then two Minutes to the Weſt of the Planet, and in Latitude the Star was more Southerly than the Satellite eight Minutes. This Star, by the Direction of the Place of *Jupiter* at that Time, I found out, and, by comparing it with others in the Catalogue, having nearly the ſame Declination, I ſettled its Place in ♊ 13° 24'½ to the Time of the *Britiſh* Catalogue, 1690, with 0° 25' South Latitude.

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* Philoſ. Tranſact. N° 342.

† Nuncius Syder, *pag.* 27. *Edit. prin.* 1610.

Another remarkable Obſervation of *Saturn* is recorded in *Riccioli*[*], ſaid to have been made at *Modena* by the Marquis *Malvazzo*, on *July* 3° *N. S.* 1662, when the Eaſtern *Anſa* of *Saturn* touched a Fix'd Star. By the then Place of *Saturn*, I look'd out for this Star, to which *Saturn* is at this time very near, and after the ſame Manner, I ſettled its Place *ineunte Anno* 1690, in 29°, 34′ of *Scorpio*, with 2° 0′ ½ North Latitude. By this it will appear, how defective the obſerved Place of *Saturn* is ſtated in *Riccioli*, there being above 7 Minutes erred in the Latitude thereof.

Eclipſes of the SUN.

VIII. 1.]

An Eclipſe of the Sun Nov. 27 1722. P. M. *at* Greenwich, *by Dr.* Halley. N° 374. *p.* 197.

Phaſes.	*Tempora.*		
	h	′	″
Eclipſin jam inceptam vidi.	1	29	16
Diſtantia Cuſpidum 7′ 4″, unde pars deficiens 0′ 47″ ac initium verum 1 *h* 28′ 58″	1	31	6
Diſtantia Cuſpidum 10′ 50″	1	34	18
Repet. 16′ 20″	1	42	28
Inclinatio Cuſpidum ad dextras 44° 30′	1	43	26
Partes lucidæ reſiduæ 17′ 20″	2	32	37
Repet. in med. Eclipſis proxime 17′ 9″	2	40	16
Inclinatio ad ſiniſtras 19° 0′	3	28	45
Diſtantia Cuſpidum 15′ 21″	3	31	45
Repet. 10′ 50″	3	37	35
Finis Eclipſeos dubius, ob limbum Solis aſperum & undulantem nec ſat bene definitum.	3	43	25
Certe deſierat Eclipſis.	3	43	45

2.]

The ſame in Fleet-ſtreet, London, *by Mr.* G. Graham. N° 374. *p.* 198.

Phaſes.	*Apparent Times.* P. M.		
	h	′	″
The Beginning of the Eclipſe. —— ——	1	28	38
The Cuſps parallel to the Horizon, by Eſtimation.	2	29	34
The End of the Eclipſe. ————	3	43	22
The Duration. ————	2	14	44
The Quantity eclipſed. ————	5	Dig.	716
			1000

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[*] Aſtron. Reform. *pag.* 286.

I had very correct Obſervations both of the Sun and Stars, the 26th, 27th, and 28th, for determining the exact Time by my Clock.

For ſome Minutes before the Eclipſe began, I obſerv'd the Sun with a Teleſcope of 12 Foot, furniſhed with a Micrometer; keeping that Part of the Limb in the middle of the Glaſs, where I expected the Moon firſt to touch, and in leſs than four Seconds of Time, from the Moment I judged the Eclipſe begun, it was ſo conſiderably advanc'd, that I cannot doubt of having the Beginning to leſs than three Seconds. I believe the exact Time of ending was within the ſame Limit, notwithſtanding that the Undulation of the Limb was then much greater than at the Beginning. The Parts eclipſed, meaſured with the Micrometer, at the Time of the greateſt Obſcuration, were 927 ſuch Parts as the Sun's Vertical Diameter contained 1946; which was taken a little before the Beginning of the Eclipſe.

By this Obſervation, the Beginning differ'd not 2′ ½, and the End not ½ a Minute from Dr. *Halley*'s Calculus.

N. B. *The ſame Eclipſe was obſerv'd by Mr.* Hawkins *at* Wakefield *in* Yorkſhire, *to begin at* 1 *h.* 21 *m. P. M. and to end* 3 *h.* 30′ 3″. *The Sun's Diameter was obſcured ſomewhat more than* 5 *Digits.*

3.]

At Cambridge *in* N. Engl. *Mr.* T. Robie. N° 382. *p.* 68.

h.	′	″	
7	27		Morn. I ſaw the Sun riſe Eclipſed, on its ſupreme Vertex to the South, about 4 *Dig.* tho' ſome on the Top of the new College ſaw it 2′ or 3′ before. The Sun's true riſing this Morn. was 7 *h.* 30′, hence the Refraction is about 6′ and ſo I have often obſerved it. From this time, till about 8 *h.* 30′ or 40′ I ſaw no more of the Sun, but then I judge it was eclipſed 6 *Dig.* or more.
h.	′	″	
8	55	15	The Sun was eclipſed 4 ¼ *Dig.* neareſt.
9	00	16	4. *Dig.* ½.
9	19	45	A little Spot in the Sun emerged.
9	25	45	I ſaw the Moon go off the Sun.
9	25	45	Mr. *Danforth* in a Room juſt by me ſaw the Shadow go off the Paper about 30° from its lower Vertex to the Eaſt.
9	25	20	Mr. *Appleton* ſaw the Shadow go off the Paper fix'd to the College Braſs Quadrant at his Houſe.
9	26		Mr. *Owen Harris*, an ingenious Schoolmaſter in *Boſton*, ſays he obſerved the End at about 26′ *p.* 9.

By the ſecond Obſervation the Sun's Diameter was to the Moon's as 1000 to 972 ; by the third, as 1000 to 975. At *Boſton* the Eclipſe was obſerved, allowing for its Diſtance, as I obſerved it at the College. And at *Barnſtable*, on Cape *Cod*, there was but a little left of the Sun, and nearer the Head of the Cape there was a Ring of Light quite round the Moon.

The Teleſcope I made my Obſervations by is 24 Feet long. The Teleſcope that Mr. *Danforth* uſed, thro' which the Rays were tranſmitted, was 8 Feet, and the Braſs Quadrant the very ſame Dr. *Halley* uſed at St. *Helena*. If I have been guilty of any Miſtake pardon me, and if, with eaſe, you could tell me where the Shadow would paſs off *America*, I ſhould be glad, for I made it to be about Cape *Cod*. Taking its Latitude to be 40° North, or 40° 10', and Eaſt from the College 10' or 15' I forget which.

[IX. 1.]

An Eclipſe of the Sun Sept. 25. *N.S.* 1726, *at* Padua *by Mr* J. Polenus. N°395. *p.* 157.

Temp. Appar.			*Phaſes.*	*Obſervata.*
H	'	"	*Dig. Obſcur.*	
5	25	25	—	Initium defectus hoc tempore nullum apparebat: tum poſt tempus hoc, ingruerunt denſæ nubes.
	29	5	1	
	31	48		Tegitur maxima ſolaris Macula inter duas minores ſita.
	34	27	2	
	40	43	3	
	44	20	3 ½	
	47	15	4	
	48	12	4 ¼	
	49	00	....	Creſcebat certe adhuc Obumbratio in Solari diſco, ut trans nubes apparebat: mox vero cum denſiores fierent nubes, atque Sol ad Finitorem properabat, nihil quicquam amplius licuit obſervare.

IX. 2.]

At Lisbon *by* F. J. Bapt. Carboni. N°400.*p.*335.

2.] Cœlo, præter ſpem, clariſſimo Deliquium hoc Ulyſſipone conſpeximus, ejuſque finem tantum non vidimus; monte ſiquidem interjecto, delituit Sol uno circiter minuto temporis antequam plenè reſtitueretur. Teleſcopio uſus ſum pedum 8. Pariſinorum, quod micrometro inſtruxeram accuratiſſimè elaborato. Nonnulla tamen obſervanti acceſſerunt incommoda, quibus præpedientibus, phaſes aliquot digitorum inveſtigare, aut opportunè adnotare non licuit. Quas tamen obſervare datum eſt, eas ſanè ſenſibili errore carere autumo; tum quòd in digitis Solaribus rectè dimetiendis nullam prætermiſſerim diligentiam; tum quod correctionem temporis repetitis eo die Obſervationibus accuratiſſimè inveſtigaverim.

Ne verò ſuo careat teſtimonio prædicta correctio, duas hîc apponam altitudines Solis, quarum alteram ante Eclipſeos initium, alteram ſub ejus finem, Quadrante Aſtronomico trium pedum Pariſinorum, deprehendi; & utraque ſanè Obſervationi Meridianæ conſentit, ejuſdemque defectûs horologium arguit.

	H.	'	''
In Meridie vero, ſeu Apparenti indicabat horologium. — — — —	11	58	34
Nempe deficiebat à Tempore vero — —		1	26

	°	'	''
A Meridie; Altitudo vera centri Solis — —	28	53	45
Ejuſdem Declinatio Auſtralis — — —		53	18
Altitudo Poli, ex pluribus Obſervationibus, certa, ſaltem quoad minuta prima — —	38	42	0
Quibus datis invenitur per Trigonometriam arcus diſtantiæ Solis à Meridiano — —	59	49	36
	H.	'	''
Qui, ſi in tempus convertatur, dat — —	3	23	18
Indicabat verò horologium — — —	3	21	48
Ergo à tempore vero deficiebat — —		1	30

	°	'	''
Iterum, Altitudo vera centri Solis — —	4	52	54
Ejuſdem Declinatio Auſtralis — —		55	24
Altitudo Poli, ut ſupra — — —	38	42	0
Ex quibus denuò per Trigonometriam infertur arcus diſtantiæ Solis à Meridiano — —	32	59	40

	H.	'	"
Qui in tempus converſus dat — —	5	31	59
Indicabat verò horologium — —	5	30	26
Ac proinde à tempore vero deficiebat — — —		1	33

Hanc igitur æquationem tempori horologii addidi, quò fieret tempus verum, ſeu apparens correctum, eoque uſus ſum in ſequentibus Obſervationibus.

Phaſes Immerſionum.	*Temp. Ver.* H.	'	"
Incipit limbus Lunæ limbum Solarem perſtringere. dub. — — — — —	5	59	50
I. Digitus latet — — —	4	5	10
II. Digitus — — — —	4	10	45
III. Digitus — — — —	4	16	55
Dimidium quarti — — —	4	20	14
IV. Digitus — — — —	4	23	48
V. Digitus — — — —	4	31	5
Dimidium ſexti — — — —	4	34	51
VI. Digitus — — — —	4	38	57
VII. Digitus — — — — —	4	47	57
Dimidium octavi — — —	4	54	59
Maximæ obſcurationis tempus, quantum ex reliquis phaſibus colligi poteſt —	4	58	30
lig. VII. 45. Maximæ Obſcurationis *Quantitas.*			

Phaſes Emerſionum.	*Temp. Ver.* H.	'	"
Dimidium ſeptimi digiti latet — —	5	13	40
VI. Digitus — — — —	5	17	58
V. Digitus — — — — —	5	25	58
Dimidium quinti — — —	5	29	43
Dimidium quarti — — —	5	36	49
III. Digitus — — —	5	40	8
Dimidium tertii — — —	5	43	14
II. Digitus — — — — —	5	46	10
Dimidium ſecundi — — — —	5	48	58
I. Digitus — — — — —	5	51	40
Limbus Solis inferior montem ſtringit. —	5	52	42
Sol totus deliteſcit, adhuc deficiens min. circiter 12. nempè $\frac{1}{3}$ unius digiti. — — —	5	55	27

H.	′	″	
5	55	27	Altitudo apparens montis, ſeu limbi ſuperioris Solis, 57′ 30″, quæ correcta, erit 34′ 29″; ex his rurſus ſubtractâ quantitate ſemidiametri Solaris 16′ 4″ remanebit, altitudo vera centri Solis, quando totus delituit 18′ 25″; ex qua; infertur hora prædictæ altitudini reſpondens, nimirùm — —
5	57	1	— Occaſus Solis verus — —
6	0	0	— Sed apparens, propter refractionem, circ. —
5	56	50	Unde patet, finem Eclipſeos in noſtro Hemiſpheric viſibilem fuiſſe; contigit enim, quantùm ex prædictis colligi poteſt, circiter 5ʰ 56′ 50″ — —

3.]

At Ingolſtat, *by the Fathers of the Societ. of* Jeſus. N°405.*p*.558.

Temp. H	′	″	*Phaſes.*
5	17	52	Imago Solis per Helioſcopium excepta in obſcuro loco, cœptæ Eclipſeos initium præbet circa 46° ½ a Nadir ad Boream.
5	19	24	Sol obſcuratus $\frac{1}{10}$ unius digiti Teleſcop. 12 & 16 pedibus videtur.
5	23	30	Centrum maculæ Solis limbo propioris immergitur.
5	24	40	Centrum maculæ inſignis.
5	26	36	Centrum maculæ tertiæ.
5	30	46	2 Digiti obſcurati a Nadir in Bor. 39°
5	37	12	3 Dig. — — — 35 ½
5	43	10	4 Dig. — — — 27
5	49	0	4 ½ Dig. circiter deficientem ſolem nubes ſurripuere.

Phaſes Micrometro dimenſæ.

H.	′	″		*Digit.*	′
5	22	30	— — — —	1	
	30	50	— — — —	2	
	37	54	— — — —	3	
	44	30	— — — —	4	
	47	30	— — — — —	4	33

Solis ſemidiameter ſæpius micrometro dimenſa exacte implebat 1′6 0′0.

X. 1.]

X.] The Latitude of this Place, I found by ſeveral Obſervations with a Quadrant of four feet Radius to be 19° 12′*N.* On *March* 11, 1727. there happened an Eclipſe of the Sun, whoſe greateſt Obſcuration here, was about 10 ½ Digits. Having carefully adjuſted the Pendulum Clock, and fix'd a Teleſcope to the Index of the foreſaid Quadrant, I obſerv'd it to begin in about the S. E. by S. Part of the Sun's Diſk.

An Eclipſe of the Sun at Vera Cruz, *March* 11, 1727. *by Mr.* J. Harris. N°401. *p.* 388.

	Appar. Time P. M.		
	H.	′	
The Beginning — — — —	0	49	½
Middle as near as we could judge. — — —	2	30	
End about the N. N. E. part of his disk — —	3	59	½
	°	′	
The Sun's Altitude at Beginning	67	53	
The Sun's Altitude at End	28	34	

Comparing theſe Obſervations with my Calculation from Mr. *Flamſtead*'s Tables, I judge *Vera Cruz* to lie 97° 30′ to the W. of the Meridian of thoſe Tables.

XI. 1.] In *Prædio*, quod eſt occidentalius noſtro Collegio D. Antonii M. 4″ hor. circiter, & cujus Latitudo Quadrante aſtronomico trium pedum explorata, eſt 38° 42′ 58″, obſervavi hanc Eclipſim Teleſcopio pedum circiter 8, quod micrometro inſtruxeram ritè comparato. Initium infra horizontem celebratum eſt; jamque digitos circiter 4 deficiebat Sol, quando ex oppoſito monte primò emerſit. Sequentes tamen phaſes obſervari tantùm potuere, reliquis fortuito eventu impeditis.

An Eclipſe of the Sun on 15. Sept. 1727. *NS. Near* Lisbon, *by* F. J. Carbone. N°403. *p.* 471.

Digiti	Phaſes Immerſ.	Temp. A. M.			
		H.	′	″	
		Temp. Ver. corr.			
VI ½	— — — —	5	55	8.	dub.
VIII	— — — —	6	10	54.	dub.
VIII	m. 1′, ſec. 48″ max. obſc. —	6	13	29.	circ.
	Emerſ.				
VI ½	— — — —	6	31	49	
VI	— — — —	6	35	23	
V ½	— — — —	6	38	45	
V	— — — —	6	41	57	
IV ½	— — — — —	6	45	2	
IV	— — — —	6	47	59	
III ½	— — — —	6	50	49	
III	— — — —	6	53	34	
II ½	— — — —	6	56	16	
II	— — — —	6	58	54	
I ½	— — — — —	7	1	28	
I	— — — — — —	7	3	59	
½	— — — — —	7	6	28	
Finis	— — — —	7	9	2	certiſſ.

Poſt finem Eclipſeos, ſtatim horologium pendulo inſtructum, duplici Solis altitudine eodem quadrante aſtronomico obſervatâ, ad trutinam revocavi; inventamque correctionem, in phaſibus ſuperiùs adnotatis adhibui.

The ſame at Padua, by Sig. J. Polenus. Nº 403. p. 479.

2.] Denſioribus nubibus ſæpius intercurrentibus, non plures obſervari potuerunt Phaſes, quam eæ, quas ſubjeci.

Temp. Ver.			Phaſes	Digit.	Min.
H.	′	″			
19.	3.	45.	— — —	0	10
	24.	12.	— — — —	3	0
	41.	27.	— — —	4	30
20.	30.	45.	— — — —	1	30
	38.	42.	— — — —	Finis.	

3.] Horæ Minuta prima & ſecunda temporis veri poſt mediam noctem, ex tranſitu ſolis rectificantur. Videram die præcedenti maculas plures in diſco ſolis, apparere. Completa celeriter harum delineatione, ſequentes phaſes accurate adnotavi cum V. C. *Ja. Dominico Maraldi*, directo in ſolem tubo optico, exceptâque in charta Solis imagine. *At* Rome N°. 403. *p*. 473. Fig. 97.

Temp. Ver.			*Phaſes.*
H.	′	″	
7	0	0	Directo in ſolem Tubo optico, Eclipſim ex aliquot minutis horariis cepiſſe video.
7	2	17	Latent ſolaris diſci tres digiti cum quadrante à Luna contecti. Interſectio diſcorum ſolis, & Lunæ incidit in gr. 5 & 95. numeratis à puncto, *A*. quod eſt in Figurâ inverſâ 96. verticale. Macula *a* eſt in plano Azimuthi per centrum Solis à Zenith ducti, & diſtat in ſemidiametro diſci Solaris à centro verſus peripheriam digitis 4 $\frac{1}{4}$, ut in figura 97. Etiam macula *c* in eodem ferè Azimutho verſatur. *Fig*. 96. *Fig*. 97.
7	21	47	Nunc ſpectantur à Luna contecti digiti Solaris diſci 4 $\frac{1}{2}$. Interſectio diſcorum Solis, & Lunæ fit in gr. diſci Solaris 10 & 111. ut antea à puncto *A* imaginis per Lævam reſpicientis numeratis.
7	24	0	Veſtigium dilutioris maculæ *m* prope *b* ſitæ Lunæ diſcum ſubit.
7	24	40	Macula *b* incipit perſtringi à diſco Lunæ.
7	25	11	Eadem macula *b* tota immergitur.
7	27	41	Initium maculæ *c* incipit ſubire diſcum Lunæ.
7	28	31	Eadem macula *c* tota jam occultatur.
7	29	10	Digiti Solaris diametri 5 $\frac{2}{3}$ latent.
7	31	9	Latent digiti 5 $\frac{3}{4}$. Interſectio diſcorum utriuſque Luminaris fit in gr. 20, & 136.
7	38	45	Latent digiti 6. & gr. 31. atque 150 Solaris diſci ſunt puncta interſectionum cum Lunari.
7	40	58	Maculæ *d* Limbus prior à Lunæ circulo perſtringitur.
7	41	45	Tota macula *d* jam latet.
7	43	15	Conteguntur Solis digiti 6 $\frac{1}{4}$. & interſectio diſcorum incidit in gr. 39 & 162.
7	45	26	Maculæ ferme Evanidæ *f* Limbus perſtringitur ab incurſu Lunæ.

Fig. 96.

H.	′	″	
7	46	20	Tota macula *f* à Luna contegitur.
7	50	0	Latent digiti 6 ¼, & luminarium peripheriæ ſe interſecant in gr. 61 & 185.
8	0	12	Latent paulo minus quàm digiti ſex, ſeu 5 ⅜. interſecant ſeſe diſci in gr. 62. & 182.
8	2	25	Latent digiti 5 ¼, ex gr. 63 ad 183.
8	5	24	Latent digiti 5 ¼. Interſectio Luminarium diſci in gr. Solaris 80 & 192.
8	8	32	Latent digiti 5. Interſectio in gr. 82 ½, & 192 ½.
8	11	50	Incipit emergere Limbus prior maculæ *b*.
8	12	38	Tota macula *b* extra Limbum Lunæ; & macula *n* eidem proxima ſimul exit.
8	14	46	Emergit etiam macula *m* vicina maculæ *b*.
8	16	34	Emergit quoque macula *l* eidem *b* proxima.
8	18	29	Macula *c* emergit.
8	22	38	Latent digiti 3 ½. Interſectio diſcorum in gr. 105 & 195.
8	23	40	Incipit emergere macula *d*.
8	24	10	Tota macula *d* extra diſcum Lunæ.
8	27	23	Latent digiti 2 ½. Interſectio diſcorum fit in gr. Solaris 115 & 182.
8	34	5	Incipit emergere è diſco Lunæ prior limbus maculæ *e*.
8	34	55	Tota macula *e* extra limbum Lunæ.
8	35	46	Latent digiti 1 ½.
8	37	9	Latent digiti 1 ¼.
8	37	27	Exeunt extra Limbum Lunæ maculæ *g*, & *h*, ſitæ prope maculam *e*.
8	39	46	Latent digiti 0 ¼. Interſectio Limborum utriuſque diſci Luminarium in Solari incidit in gr. 140 & 180.
8	42	8	Limbus ſuperior Solis diſtat à Vertice gr. 58. 1′. inſpectus per quadrantem aurichalchicum tubo inſtructum, cujus radius eſt palm. Rom. 3.
8	44	10	Finis Eclipſeos diſci.
8	46	53	Limbus ſuperior Solis per quadrantem aurichalchicum inſpectus diſtat à vertice gr. 57 30′. adeoque centrum Solis diſtat à vertice gr. 57. 46′.
8	48	1	Limbus ſuperior Solis iterum inſpectus diſtat à vertice gr. 57. 20′ adeoque centrum Solis diſtat à vertice gr. 57. 36′.
			Limbus inferior Solis diſtat à vertice per ſupradictum quadrantem gr. 57. 20′. adeoque centrum Solis diſtat à vertice gr. 57. 4′

Eadem die in meridie cœlo clariſſimo.

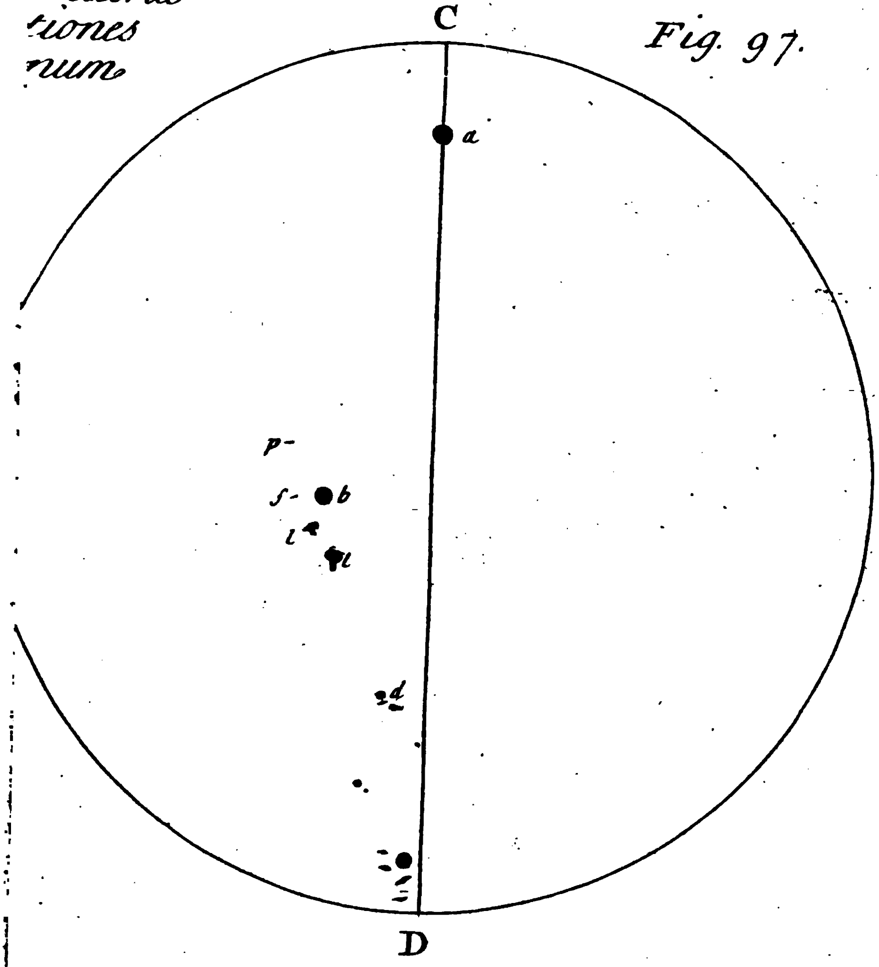
Primus

næ die 15 Septembr. 1727. N. S.

laris Disci CD
atæ
r
r literas
tiones
num

CD planum circuli Verticalis
per centrum Solis ducti

Fig. 97.



Maculæ in disco Solis
per Telescopium in chartam excepto
ita erant conſtitutæ

H.	′	″	
11	58	25	Primus Solis limbus cum penumbra attingit lineam meridianam.
0	1	35	Secundus Solis limbus cum penumbra attingit lineam meridianam. *H.* ′ ″ Ergo Meridies ———— 0 0 0

In meridie diſtantia à vertice per quadrantem explorata fuit

Gr. 38. 27′. Limbi ſuperioris. } Centri Solis gr.
Gr. 38. 59. Limbi inferioris. } 38. 43.

4.] Plurimæ in Sole maculæ hujuſce Eclipſis tempore videbantur: Cùm tamen præ nubibus neceſſariæ circa illas inſtitui obſervationes non potuerint, ut earum ſitus in Solari diſco deprehenderetur præcipuarum tantùm aſpectus in appoſita figura 98 exhibetur, qualis inferri potuit ex obſervationibus veſpere diei 14. circa hor. 5. 26′. habitis; idque ſatis eſſe arbitor ad eas indicandas quæ ſunt à Luna occultatæ Eclipſis tempore.

At the Obſervatory of Bologna *by Sign.* Euſtachio Manfredi. N°. 403. *p.* 477. *Fig.* 98.

Initium Eclipſis obſervari non potuit; ſed hor. 18. 55′. 48″. Aſtronomic. ſenſibilis apparebat defectus, ut inter nubes.

Tempora.			*Phaſes.*
H.	′	″	
18	59	37	Unus digitus, & amplius fortaſſe latebat.
19	3	12	Digitus cum dimidio.
19	6	50	Duo digiti.
19	10	11	Duo cum dimidio. *dub.* Paulò poſt nubes Solem obtegunt.
19	30	35	Sol inter nubes videtur deficere plus digitis 4 cum dimidio.
19	35	46	Eclipſis nondum pertingere videtur ad 5 digit.
19	40	47	Quinque digiti circiter.
19	44	31	Maculæ, *b*, *c*, *d*, *e*, jam latebant. Nunc occultatur centrum, *i*.
19	47	27	Limbus ☽ inter maculas, *s*.
19	47	46	Secunda ex duabus maculis, *s*, omnino tegitur.
19	51	37	Eclipſis eſt paulò minor 5 digit. inter nubes.
19	54	12	Centrum maculæ, *p*, occultatur.
19	55	37	Centrum, *q*, item occultatur.
19	59	2	Quatuor dig. cum dimidio eclipſantur. *dub.*
20	1	22	Una ex maculis prope limbum (fortaſſe *n*) occultatur.
20	10	10	Eclipſis paulò min. dig. 3 $\frac{1}{2}$.
20	11	47	Tota macula, *i*, emerſit.

Fig. 98.

Eclipſis

H.	'	"	
20	15	0	Eclipſis 3 digitorum.
20	18	48	Duorum cum dimidio.
20	18	49	Incipit emergere macula, *s.*
20	22	26	Duo digiti circiter eclipſantur.
20	26	14	Digitus 1 ½.
20	36	6	Finis Eclipſeos à tribus obſervatoribus notatus, in eodem ſecundo temporis concordibus.

XIX.] *A Scheme of an Eclipſe of the Sun* July *4th. O. S.* 1730. *at* Wirtemburg *by Mr.* J. F. Weidler. N°. 415. p. 394.

The Type Fig. 99.

Temp. Ver. Ante Merid.			*Phaſes.*		*Annotata.*
H.	'	"	*Dig.*	*Min.*	
3	56	0			Sol oritur poſt nubes.
	59	0	5 +		Sol latet poſt nubes.
4	10	30	6	55	
	26	0	6	30	I. Sol oriens figuram monſtrat ellipticam: diameter verticalis duobus digitis ſive ſexta ſui parte contractior apparet horizontali.
	33	0	6	0	
	38	0	5	30	
	43	30	5	0	
	47	0	4	30	II. Memorabilis erat conſpectus orbis Lunæ aſpero margine præditi, quoad partem quæ in occaſum ſpectabat. Diſtincte enim cernebatur *4h. 3m.* Vallis $\frac{1}{100}$ diam. Lunæ profunda & $\frac{1}{21}$ ejuſdem diam. circiter longa. In progreſſu eclipſeos aſperitas limbi Lunæ minuebatur, & adhærente eidem faſciâ cœruleâ abſcondebatur. Hæc enim faſcia ſenſim ſole altius ſurgente dilatabatur. Tum præter colorem cœruleum etiam puniceus Lunæ proprius incumbens in oculos incurrebat, & circa finem deliquii faſciæ coloratæ ſpiſſitudo trigeſimæ ſextæ parti diametri Lunaris prope par videbatur. III. Præterea juxta Lunaris diſci marginem coloratum perpetua Luminis ſolaris commotio notata eſt.
	50	15	4	0	
	53	30	3	30	
	57	0	3	0	
5	3	30	2	0	
	7	0	1	30	
	10	30	1	0	
	13	0	0	30	
	15	30	0	0	Finis Eclipſeos.

2.] Oriente Sole, Nubes tenues Finitorem quaſi cingebant: quibus deinde evaneſcentibus, aer tantillum nebuloſus fuit, ut maculæ ſolares haud ſatis diſtincte apparerent.

The ſame at Padua *by Mr.* J. Polenus. N°. 415. *p.* 396.

Digiti Obſcur.	*Tempora Vera.*		
	H.	′	″
4	16	46	12
3 ½	16	48	7
3	16	50	36
2	16	57	24
1	17	1	20
½	17	3	29
Finis	17	6	8

July 15*th N. S.*

3.] Ex inſperato ſub ipſum Eclipſeos initium ſiſtere pluviæ ſimulque nubes rareſcere cœperunt, ac poſt horæ quadrantem Sol per nubila rariora nudis oculis Eclipſi infectus circiter ſeſquidigitum apparuit.

An Eclipſe of the Sun July 15*th.* 1730. N. S. *at* Peking *by F F.* Ignat. Kegler *&* Andrew Pereyra. N° 420. *p.* 179.

Præparaveramus organon, ad ſpeciem *Solis* ſcilicet per teleſcopium 6 pedum Sinicorum excipiendam in orthogonaliter ſubjectâ menſulâ, è cujus centro ad amplitudinem apparentis ſpeciei accuratè deſcriptus erat circulus per 10 digitos more Sinico diviſus. Parati quoque habebantur in chartâ mundâ plures circuli ſimiliter diviſi, & ſuper illum ſucceſſive applicandi, in quibus præſignatæ erant phaſes eclipticæ per ſingulos digitos apparituræ, ſecundum inclinationes *Lunæ* ad lineam verticalem *Solis*.

Fig. 100.

Interim verò aliud ad *Solem* dirigebatur teleſcopium 2 lentibus objectivis inſtructum, in eâ inter ſe diſtantiâ, ut filare reticulum in foco teleſcopij diſpoſitum, pariterque per 10 digitos diviſum exactè quadraret apparenti magnitudini *Solis*, atque per iſtud primo obſervatus fuit appulſus *Lunæ*.

H.	′			′
11	40	a. m. ad dig. III. id eſt Europ. dig.	3	36
11	51	ad dig. IV.	4	48

Poſtea clariſſime allucente *Sole* per hujus ſpeciem in diſco notati fuerunt ut ſequitur.

H.	′				
0	2		p. m. ad centrum ſeu d. V. Europ. d.	6	0
0	14		ad dig. VI.	7	12
0	26 ½		ad dig. VII.	8	24
0	40		ad dig. VIII.	9	36
0	51		max. Eclipſ. dig. VIII. ¼	9	54
1	2		regreſſ. ad dig. VIII.	9	36
1	16	20″	ad dig. VII.	8	24
1	27	50	ad dig. VI.	7	12

Dein rurſus tenui nebula involutus *Sol* ſuam ſpeciem infuſcavit, teleſcopio tamen præfato clare viſibilis ad cujus reticulum obſervatus eſt.

H.	′			
1	39	p. m. receſſ. ad dig. V. ſeu	6	0
1	50	ad dig. IV.	4	48
2	0	ad dig. III.	3	36

Iterum emergens è nebula *Sol* clariſſimam exhibuit ſpeciem ad quam porro notati ſunt.

H.	′	″			
2	9	20	receſſ. ad dig. II.	2	24
2	18	20	ad dig. I.	1	12

2 27 10 Finis Eclipſis: qui itidem per aliud teleſcopium excellens 14 pedum Sinicorum eodem momento eſt annotatus. Horologium denique correxit *Sol* ipſe, tum in magno ſciatherico, armillaque æquatoria Obſervatorii ſingula minuta horaria commonſtrans, tum per captas aliquot altitudines eadem momenta temporis comprobans.

Temp.

Temp. Ver. P. M. H.	'	"	*Occultationes & retectiones nonnullarum* Solis *macularum.*
0	22	0	Macula major, quæ erat in ipſa peripheria dig. 11. ad *Nord-Oſt* immerſa.
0	27	50	1ª Minores inter dig. 11 & 1 immerſæ.
0	31	40	2ª
0	37	10	3ª
0	38	35	4ª
1	18	45	Maculæ duæ inter dig. III & IV. verſus *Sud. Weſt.* retectæ.
1	23	50	
2	5	20	Maculæ quatuor ad *Nord-Oſt* retectæ fuerunt.
2	7	30	
2	11	25	
2	12	25	

XIII.
1.] *An Eclipſe of the Moon* Sep. 8*th*. 1718. *at* Padua, *by* Signori J. Poleni *and* J. B. Morgagni. N° 382. *p.* 71.

Temp. Appar. P. M. H.	'	"	*Phaſes.*
			Sub initium Eclipſis nubes lunam obtexere.
6	54	25	Umbra appellit ad partem ortivam maris humorum; diſtat ab Ariſtarcho diametro ejuſdem maculæ, parique intervallo a Keplero.
7	5	5	Appellit umbra ad Copernicum.
	12	56	Umbra appellit ad Tychonem.
	18	10	Appellit ad Platonem.
	22	31	Tegit Manilium totum.
	30	55	Tegere incipit partem ortivam maris Nectaris.
	41	53	Appellit ſecundam partem ortivam ſuperiorem ad mare Criſium.
	46	58	Penumbra extremum diſci attingit.
	49	4	Vix quidquam immerſionis ſupereſt.
			Toto integræ immerſionis tempore luna videri potuit commixta colore quodam obſcuro & ſubrubro. Principio poſt immerſionem lunæ pars orientem verſus erat obſcurior.
8	33	3	Obſcurior erat lunæ pars prope diſci medium: minus obſcuræ erant circumquaque partes diſci extremæ.
9	30	49	Stellula quædam, nudis oculis inconſpicua, vix decem ſecundis diſtare videbatur a lunæ diſco e regione Lanſbergii.

H.	′	″	
	32	9	Penumbra ſit clara in diſci extremâ parte ortivâ.
	36	4	Initium emerſionis ex ortivâ plagâ.
	40	39	Grimaldus jam emerſit ab umbrâ a qua diſtat ſui ipſius minori ſpatio.
	44	38	Stellulæ ante viſæ a lunâ occultatio : tamen incerta.
	49	34	Gaſſendus emergit.
	50	49	Mare humorum extra umbram totum.
10	00	3	Copernici emerſio.
	5	55	Plato emergere incipit.
	14	41	Eudoxus exit.
	19	12	Menelai emerſio.
	27	7	Mare nectaris totum emerſit.
	36	28	Umbra dividit Mare criſium biſariam ſecundum ipſius majorem diametrum.
	39	12	Incipit umbra fieri tantillum rarior.
	41	2	Viſus eſt eſſe umbræ finis.
	42	57	Et finis penumbræ.

2.] *The ſame at* Bologna *in the* Palace *by Signori* Geminiano Rondelli, Giuſeppo Ant. Nadio & Giul. Cæſare Pariſi. Nº 382. *p.* 72.

Temp. Ver. P. M.			*Phaſes.*
H.	′	″	
			Initium eclipſis non eſt obſervatum.
6	51	36	Mare humorum ad umbram.
	56	22	Capuanus ad umbram.
6	56	37	Mare humorum totum in umbrâ.
7	1	7	Bullialdus ad umbram.
	2	52	Bullialdus totus ſub umbra.
	3	37	Copernicus totus ſub umbra.
	11	22	Tycho ad umbram.
	12	52	Totus Tycho ſub umbra.
	15	37	Plato ad umbram.
	16	27	Totus Plato ſub umbra.
	19	22	Manilium ad umbram.
	19	52	Mare ſerenitatis.
	23	57	Mare tranquillitatis.
	35	8	Meſſalla ad umbram.
	36	8	Totus Meſſalla ſub umbra.
	36	38	Mare fœcunditatis ad umbram.
	37	23	Promontorium ſomni.
	39	23	Cleomedes ad umbram.
	39	53	Mare criſium.
	44	8	Mare fœcunditatis totum.
	44	43	Mare criſium totum.
	47	18	Totalis obſcuratio lunæ, juxta D. *Nadii* æſtimationem.

Totalis

H.	'	"	
		53	Totalis obſcuratio, juxta D. *Pariſium*.
9	47	40	Initium emerſionis lunæ.
	33	35	Grimaldus totus extra umbram.
	36	54	Galilæus extra umbram.
	40 42	34	Sidus quoddam a luna tegitur in eodem proxime verticali cum centro lunæ.
	47	50	Mare humorum extra umbram.
	52	10	Bullialdus extra umbram.
	54	25	Centrum Tychonis extra umbram.
	55	12	Tycho totus extra umbram.
	58	46	Mare nubium extra umbram.
10	4	2	Plato ad umbræ terminum.
	5	33	Totus Plato extra umbram.
10	17	12	Inſula ſinus medii extra umbram.
	23	47	Meſſalla ad terminum umbræ; ſimul totum mare ſerenitatis extra umbram.
	27	58	Mare tranquillitatis extra umbram.
	30	12	Cleomedes extra umbram.
	32	8	Mare criſium umbræ terminum attingit.
	34	7	Mare fœcunditatis totum extra umbram.
	36	19	Mare criſium totum extra umbram.
	37	36	Finis eclipſis.

3.] *The ſame in the Suburbs of* Bologna *by Signori* Euſtachio *&* Gabrielo Manfredi. N° 382. *p.* 74.

Temp. Ver. P. M.			*Phaſes.*
H.	'	"	
6	31	48	Nunc primum luna e collibus aſſurgere incipit, penumbrâ atmoſpheræ jam infectâ.
	42	13	Initium veræ eclipſis, quantum judicare patiebatur ſubdubius umbræ terminus. Paulo poſt nubecularum atque arborum objectu luna tegebatur.
	52	48	Umbra per Ariſtarchum & Keplerum protenditur, atque unâ mare humorum tangere videtur.
7	2	23	Umbra per medium Bullialdi, ſimul tangens Copernicum.
	4	2	Umbra per medium Copernici.
	5	4	Totus Copernicus latet.
	7	58	Umbra Pitatum attingit.
	10	54	Attingit Tychonem.
	12	19	Medium Tychonis latet.
	13	9	Totus Tycho latet.
	15	34	Umbra ad Platonem.
	16	7	Ad medium Platonis.
7	16	54	Totus Plato latet.
	20	9	Manilius tegitur.

H.	′	″	
	20	33	Umbra tangit mare ſerenitatis.
	23	44	Menelaus tegitur.
	24	36	Dionyſius tegitur.
	27	34	Plinius tegitur.
	29	49	Umbra ad Catharinam, Theophilum, Cyrillum.
	30	36	Umbra tangit Fracaſtorium.
	31	44	Medium Fracaſtorii tegitur.
	32	34	Promontorium acutum umbram ſubit.
	35	15	Promontorium ſomni latet.
	37	57	Taruntius latet.
	39	39	Umbra tangit mare criſium.
	42	16	Umbra per medium mare criſium.
	44	5	Totum mare criſium in umbra conditur.
	47	50	Totalis immerſio lunæ in umbram. Toto tempore eclipſis luna clariſſimè in ſudo ſpectabatur, colore rubeſcenti, ea parte denſiori, qua altius in umbram immergebatur.
8	38	50	Hoc tempore, & deinceps aliquot minutis, omni ex parte æque obſcura apparebat lunæ facies, ut facile conſtaret eam prope umbræ centrum verſari.
9	27	50	E regione Grimaldi, qua parte emerſio imminebat, inſignis fulgor ſpectari cœperat.
	29	20	Dubitari cœptum de emerſionis initio.
	33	20	Proculdubio emerſio jam inceperat.
	35	11	Grimaldus ab umbra ſe ſubducere incipit.
	35	55	Centrum Grimaldi emergit, totus Ricciolus jam detectus erat.
	36	26	Totus Grimaldus exit ab umbra.
	39	28	Galilæus exit.
	41	22	Umbra tangit mare humoſum.
	42	31	Stellula quæ diu prope limbum lunæ inferiorem (qui teleſcopio ſuperior apparebat) morata fuerat, nunc demum ſub lunam conditur, circa plagam Tychonis, adhuc eclipſi laborantem. Aliæ Stellulæ lunam ſubituræ videbantur, ſed poſtquam unus vel alter digitus lunaris faciei illuſtrari cœpit, præ fulgore omnes evaneſcebant.
	43	53	Umbra per medium mare humorum.
	45	41	Ariſtarchus emergit.
	47	23	Keplerus emergit.
	52	6	Emergits Bulliladus.
	53	31	Tycho detegi incipit.
	54	9	Medium Tychonis detegitur.
	55	1	Totus Tycho detectus: quu tempore nondum ſtellula e lunâ ſe ſubduxerat.
	55	21	Copernicus emergere incipit.
	56	6	Medium Copernici emergit.

H.	'	"	
9	57	15	Totus Copernicus.
10	0	1	Stellula quæ paulo antea ſub luna delituerat, jam ſpectabatur a lunæ limbo nonnihil diſtans, ut appareret eam ante 4 vel 5 minuta temporis emerſiſſe. Verſabatur autem e regione partis obſcuræ inferioris limbi lunæ, neque longe ab umbræ termino.
	4	51	Umbra per medium Platonis.
	5	36	Totus Plato detegitur.
	13	6	Manilius emergit.
	16	31	Dionyſius emergit.
	17	41	Menelaus emergit.
	20	31	Fracaſtorius totus jam emerſerat.
	23	51	Snellius & Furnerius totaliter emergunt.
	24	5	Promontorium acutum detegitur.
	25	11	Meſſalla totus apparet.
	31	11	Proclus emergit.
	31	51	Mare criſium emergere incipit.
	34	3	Medium mare criſium emergit.
	36	7	Totum mare criſium extra umbram.
	38	51	Circa hoc tempus umbra vera lunam deſerere videtur, penumbra adhuc ad multum temporis perdurante.

Obſervations made by the Marquis Antonio Ghiſilieri, *at* Bologna, on the Obſervatory in his own Houſe.

4.] *The ſame at* Bologna *by the Marquis* Antonio Ghiſilieri. N° 312. *p.* 77.

Tempus.			*Phaſes.*
H.	'	"	
6	40	23	Initium eclipſis dubium.
	51	23	Mare humorum ad umbram.
	55	46	Capuanus ad umbram.
7	1	13	Bullialdus ad umbram.
	28	14	Mare nectaris totum ſub umbra.
	32	30	Promontorium acutum ad umbram.
	36	45	Promontorium ſomni ad umbram.
	38	45	Mare criſium ad umbram.
	46	37	Totalis immerſio lunæ.
9	33	50	Initium emerſionis.
	35	39	Grimaldus totus extra umbram.
	54	17	Tycho totus extra umbram.
10	15	6	Plinius totus extra umbram.
	32	39	Mare criſium emergere incipit.
	37	42	Finis eclipſis.

An Eclipſe of the Moon June 18*th*. 1722. *and the Longitude of* Port Royal *in* Jamaica, *by Dr.* Halley. Nº 375. *p.* 235.

XIV. The Eclipſe of the Moon which happened in *June* laſt, 1722, was ſo far hid by the cloudy Sky, that neither myſelf, nor any of our Aſtronomical Friends, in or about *London*, could furniſh an Obſervation thereof worthy to be laid before the Society. But the ſame having been well obſerved at *Jamaica*, by the late curious Capt. *Candler*, and at *Berlin*, by Mr. *Chriſtfried Kirck*, Aſtronomer of the Royal Academy of Sciences there, I thought it not amiſs to prefix to their Accounts that little I was able to note concerning it.

June 18, *mane*, Having perfectly rectified my Clock ſo as to ſhew the Apparent Time, neither the tranſit of the Moon over the Meridian, nor the beginning of the Eclipſe which ſoon followed, could be ſeen through the very thick Cloud.

At 13 *h*. 12′ *T. app.* a ſmall Particle of the Moons Body was ſeen through a very little *hiatus* in the Cloud, by which glimpſe I could only be aſſured that the Eclipſe was not yet Total.

At 13 *h*. 29′ by ſuch another view, I was ſatisfied that it was now become Total; but in a Moment, it again diſappeared, till 14 *h*. 49′ 10″; when the Clouds beginning to break, I got time to meaſure with the Micrometer, the *Partes Lucidæ* now recovered in the Moon's Diameter, which I found 14′ 00″, though this not ſo well as I could wiſh, by reaſon of a thinner Sort of Cloud which perpetually intercurr'd, and render'd the Edge of the Shadow ſomewhat dubious.

At 15 *h*. 15′ the Moon was pretty well got out of the thick ~~Cloud, but being very low~~, and ~~the Daylight become ſtrong~~; ſhe ſhone very faintly, and the Shadow became worſe and worſe defined.

From 15 *h*. 26′ to 15 *h*. 27′ *T. app.* I doubted of the End, and am confident it did not exceed the 27th Minute. It ended overagainſt the North Part of the *Palus Mæotis* of Mr. *Hevelius*, much about the middle of the Weſtern or Right-hand Limb of the Moon, ſhe being then very near ſetting.

Capt. *Candler*, being then at *Port Royal*, in *Jamaica*, had much better Fortune, and a ſerene Sky from the beginning to the End; who having uſed due care to be aſſured of his Times, by Altitudes taken with an Inſtrument of three Foot *Radius*, was pleaſed to ſend us the Reſult of his Obſervation as follows.

	H.	′	″
The Eclipſe began	6	59	10
Immerſion	8	7	50
Emerſion	9	11	0
The End	10	19	40
Whence the Middle	8	39	25

And ſuppoſing the Eclipſe to have ended at *Greenwich*, at 15 *h.* 26' ½ the Difference of Longitude between *Port Royal* and *Greenwich*, will be 5 *h.* 6' 50'', or 5 *h.* 6' ½ from *London*, that is, 76. *gr.* 37' ½.

Mr. *Kirck* being in a more Eaſterly Meridian, could ſee nothing of the Emerſion, but has carefully noted the Time of the Beginning and Immerſion, as he obſerved them at *Berlin*, *viz.* the Beginning of the Eclipſe at 12 *h.* 59' 55'' and the Immerſion at 14 *h.* 8' 8'': Now by comparing ſeveral Obſervations made at both Places, we have formerly concluded *Berlin* to be 54 Min. of Time, or 13 ½ *grad.* of Longitude more Eaſterly than *London*; wherefore at *London* it began at 12 *h.* 5' 55'' and immerged at 13 *h.* 14' 8'' that is, the beginning was later here than at *Jamaica* 5 *h.* 6' 45'', and the Immerſion later 5 *h.* 6' 18'', punctually agreeing with what reſulted from my own Obſervation of the End as aboveſaid; and ſufficiently with what I had long ſince determined from Obſervations ſent me from *Jamaica* by my old Aſtronomical Friend Mr. *Charles Boucher*.

XV. 1.] *October* the 21th. 1724. being at *Gomroon* in *Perſia*, the Moon enter'd into the dark Shadow or *Umbra* of the Earth at 11 Minutes 33 Seconds paſt five, Ante Meridiem.

1.] *An Eclipſe by Mr.* W. Saunderſon. N°. 397. *p.* 213.

2.] Obſervavimus hanc Eclipſim Teleſcopiis, altero quidem Pedum Pariſinorum 8. ſed clariſſimo, altero 10. ſed minus claro: utroque tamen lunares maculæ perfectiſſimè diſcernebantur. Ad temporis dimenſionem uſi ſumus Horologio oſcillatorio, ſatis exacto, pluribus ante diebus in ipſo obſervationis loco firmato, & quotidiano examine per meridianam lineam, ibidem a nobis jamdiu inventam, & pluries examinatam, ad medium Solis motum quam proxime reducto. Nocte vero ipſius Eclipſis ter illud ad trutinam revocavimus, ut ejus a vero tempore diſcordiam deprehenderemus. Primo in tranſitu *Fomabantis* per Meridianum, Hor. 8. M. 17. Sec. 18. Secundo in tranſitu *Rigel* ſeu *Pedis Lucidi Orionis*, Hor. 2. M. 35. Sec. 21. Tertio in tranſitu *Sirii*, Hor. 4. M. 7. Sec. 40. (Aſcenſiones rectæ deductæ ſunt ex Tabulis Hirianis.) Invenimus autem Horologium tardius incedere ſecundis tantum 7. quæ jam addita ſunt momento Obſervationis mox apponendæ.

2.] *An Eclipſe of the Moon* Nov. 1. *N. S.* 1724. *by* F. F. N. B. Carbone & Dominico Capaſſo. N° 385. *p.* 180.

A Solis Occaſu uſque ad Mediam Noctem Nubes ac Pluviæ Cœlum nobis identidem adimebant. Ventus tamen ſub horam 1. illud nobis ſatis clarum reſtituit, eoque uſi ſumus ad horam prope tertiam.

Temp.

Temp. Ver.			*Phaſes.*
H.	′	″	
1	38	0	Penumbra incipit eſſe ſenſibilis
1	41	0	fit ſpiſſior
1	43	29	fit ſpiſſiſſima
1	47	45	Umbra incipit
1	49	25	Diſcus Lunæ apparet deficiens
2	0	16	Umbra pertingit ad Ariſtarchum
2	0	39	Pertingit ad Platonem
2	1	10	Ariſtarchus totus in Umbra
2	6	22	Archytas
2	8	7	Ariſtoteles
2	10	29	Pitheas
2	11	28	Galilæus
2	13	22	Umbra ad littus Orientale Maris ſerenitatis
2	15	34	Endymion immergitur totus
2	18	2	Copernicus incipit immergi
2	20	7	totus in Umbra
2	21	5	Poſſidonius incipit
2	22	8	totus latet
2	27	49	Ricciolus incipit
2	31	56	Umbra pervenit ad Grimaldum
2	34	37	Ad Litus Boreale Maris criſium
2	37	17	Proclus immergitur
2	40	0	Nubes ſupervenit, quæ Lunam omnino tegit, diuque videtur duratura
3	25	0	Nubes diſcedit. Jam autem ex Umbrâ emerſerunt
			Grimaldus
			Ricciolus
			Keplerus
			Galilæus
3	29	2	Ariſtarchus emergit
3	30	30	Copernicus incipit emergere
3	31	34	totus extra Umbram
3	39	18	Pitheas emergit
3	47	46	Timocharis
3	54	57	Archimedes
3	57	18	Plato incipit emergere
3	58	59	emergit totaliter
4	2	0	Nubecula iterum Lunæ aſpectum nobis adimit
4	6	0	Jam Luna reſtituitur
4	8	15	Ariſtotelis totalis emerſio
			Nubeculis identidem Lunam occupantibus, reliquarum macularum emerſiones exacte obſervari nequeunt

Finis

H.	'	"	
4	20	36	Finis Eclipſis, tardius fortaſſe viſus ob tenuem vaporem interpoſitum.
4	26	0	Deſinit Penumbra ſpiſſior.
4	28	0	Deſinit Penumbra ſenſibilis.

Peculiari profecto cura, ac ea qua fieri potuit diligentia, Eclipſim hanc obſervare conati ſumus; non modo ut noſtri muneris partes pro modulo noſtro impleremus, verum etiam, ut Sereniſſimi Regis ingenio plenius, ut par erat, indulgeremus. Valde enim Ipſe in hujuſmodi obſervationibus delectatur, ad eaſque perfecte inſtituendas, copioſam nobis Inſtrumentorum ſupellectilem, munificentia vere Regia, ſuppeditavit.

T. V. poſt Med. Noct.			*Phaſes Eclipſis.*
H.	*M.*	*S.*	
3	15	40	Initium umbræ veræ (quantum ſecerni poteſt à penumbra) apparere nunc incipit in limbo Lunæ, in ea parte diſci, quam ſecat radius à centro Lunæ ductus per maculam Ariſtarchi.
3	32	40	Cum nubes redderent difficiliorem conſpectum macularum, curavi definire quantitatem diametri obſcuratæ ope Micrometri. Nunc igitur latent Digiti 3. circiter. Nam portio diametri A B, *Fig.* 101. ab umbra libera, eſt partium 14, qualium tota Lunæ diameter eſt 18 ½.
3	39	40	Serenato tantiſper cœlo, umbræ limes tranſire videtur per Rheinholdum. Portio autem Diametri A B ab umbra non infecta eſt partium Micrometri 13.
3	50	40	Umbræ limes tanto ſpatio eſt infra Grimaldum (in tubo invertente objecta) quanta eſt diameter ejuſdem maculæ Grimaldi. Tranſit quoque limes umbræ per Eudoxum & Ariſtotelem.
3	54	40	A B partes Micrometri 9.
4	6	10	Umbræ limes pervadit centrum Maris Criſium, & ſtringit limbum maculæ Grimaldi. Portio vero A B immunis ab umbra æquat partes Micrometri 8 ½.

3] *At* Romæ 31 Oct. 1724. *N. S. by* S. Fr. Blanchini. N° 396. *p.* 174.

Fig. 101.

H.	M.	S.	
4	7	53	Sirius ad Meridianum appellit clariſſimè.
4	12	40	Totum Mare Criſium jam latere incipit, & in umbræ limite verſatur etiam centrum maculæ Grimaldi. A B eſt partium Micrometri 8.
4	20	0	A B partium Micrometri $7\frac{2}{3}$.
4	25	0	A B partium $7\frac{1}{4}$ circiter. Totus Grimaldus extra umbram, & Ricciolus quoque viſitur. Limes umbræ tranſit per marginem Maris Nectaris.
4	41	0	Detegitur Galilæus.
4	52	0	A B partes Micrometri 8.
4	55	30	Ariſtarchus incipit ex limite umbræ prodire.
4	56	30	Centrum Ariſtarchi exit.
4	57	30	Totus Ariſtarchus extra umbram.
4	59	20	Centrum Copernici exit.
5	0	10	Totus Copernicus extra umbram.
5	8	30	Eratoſthenes exit.
5	18	50	Helicon incipit emergere.
5	24	50	A B partes Micrometri $12\frac{2}{3}$.
5	25	50	Plato incipit emergere.
5	26	55	Totus Plato extra umbram.
5	28	20	Plinius extra umbram.
5	33	50	A B partes Micrometri 14, vel $14\frac{1}{2}$. Quare digiti tres adhuc latent.
5	38	50	Prior limbus Maris Criſium incipit prodire.
5	40	50	Centrum Maris Criſium extra umbram.
5	44	50	Totum mare Criſium extra umbram.
5	48	50	Finis umbræ veræ, quæ exit è limbo Lunæ circa punctum definitum per Diametrum à centro ductam per Cleomedem.

XVI.

1] *An Eclipſe of the Moon at* Albano, 21 Oct. *N. S.* 1725. *by* S. Fr. Blanchini. N° 396. *p.* 179.

H.	M.	S.	*Phaſes.*
			Horologiis pendulo inſtructis, & per dies plures ad meridiem verum exactis utebamur.
			Nubibus pariter orientalem tractum obſcurantibus in Lunæ ingreſſu in umbram, tam pertinaciter auſtralis ventus novas cogebat, ut per totum tempus immerſionis integri globi Lunaris in umbram, vix ter, aut quater, idque raptim, tubum opticum in eum dirigere datum ſit.

Circa

H.	M.	S.	
6	15	0	Circa hoc tempus umbra videbatur ad centrum Lunæ pertingere. Sed cum maculæ diſtinctè definiri non potuerint breviſſimo illo ſpatio temporis, quo per nubium intervalla diſcus Lunaris detegebatur, præciſè noſcere non valemus hanc ipſam phaſin digitorum ſex Lunaris diametri obtectorum, licet paulò abludat à minuto 15, poſt horam ſextam à Meridie.
6	45	0	Totalis Immerſio ad hoc circiter minutum temporis referenda eſt, quantum ſpectare licuit ex duobus, aut tribus minutis horariis, quibus Lunam vidimus ſatis diſtinctè.
			Poſt immerſionem totalem diſcus Lunæ apparebat ab Atmoſphæræ terreſtris radiis refractis rubeſcens, dilutiori tamen colore in ea parte limbi, quam poſtremam Sol deſeruerat.
			Inducta poſtmodum ſerenitate, licuit obſervationes emerſionis perficere.
8	20	0	Subalbicat diſcus Lunæ in limbo proximè illuminando. Nondum tamen lux directa Solis diſcum attingit.
8	25	0	Clarior adhuc fit limbus Lunæ; ſed nondum excedit ab umbra vera.
8	27	0	Nunc primum limbus Lunæ incipit lumen recuperare in parte circumferentiæ ſita inter maculas Grimaldi & Galilæi: quæ maculæ adhuc latent.
8	29	40	Limes illuminationis attingit primum limbum Grimaldi.
8	30	40	Totus Grimaldus extra umbram.
8	31	30	Galilæus emergit ex umbra.
8	35	40	Ariſtarchus incipit emergere.
8	36	6	Totus Ariſtarchus emerſit.
8	48	20	Prior Copernici limbus incipit illuminari.
8	49	50	Totus Copernicus extra umbram.
8	51	20	Totus Plato emerſit.
8	54	0	Prior limbus Tychonis incipit emergere.
8	56	0	Totus Tycho extra umbram.
8	59	0	Subtenſa arcus C A D, & C F D per Micrometrum explorata eſt partium Micrometri 22, qualium Lunæ diameter eſt 24 in tubo palmorum undecim Rom. A B verò eſt partium 12. (*Vide Fig.* 102.)

H.	M.	S.	
9	2	0	Menelaus 25 exit. (25 eſt numerus maculæ aſſignatus in Lunæ imagine, à Pariſienſi Academia edita.)
9	5	0	Macula clarior ſita ante Plinium exit.
9	6	0	Hermes ab umbra prodit.
9	50	0	Plinius emergit.
9	16	0	Incipit emergere Poſſidonius 27.
9	18	0	Maris Criſium limbus prior emergit.
9	25	0	Totum Mare Criſium extra umbram.
6	24	0	Langrenus 39 exit.
9	24	30	Umbræ extremum in limbo Lunæ adhuc videtur.
9	25	0	Finis umbræ veræ.

2] *An Eclipſe of the Moon* Oct. 10*th* 1725. *at* Briſtol *by* Jer. Burroughs Eſq; N° 392. *p.* 37.

The cloudy Weather here prevented us from ſeeing the Beginning of the Eclipſe, and of total Darkneſs; but I obſerved, pretty exactly, the firſt Appearance of Light, after the total Darkneſs, and the End of the Eclipſe; and their reſpective Times are as follows, *viz.*

	H.	′	″	
Beginning of Light	7	31	20	apparent Time.
End of the Eclipſe	8	29	30	

Some ſmall Time before the renewal of true Light, there appeared a remarkable Brightneſs upon the Eaſtern Limb of the Moon, which was alſo diffuſed about the Edge of the Moon, to a ſenſible diſtance from her, I would have meaſur'd it had I proper inſtruments. If others, who are more ſkill'd in theſe Affairs, have made the like Obſervation, I ſhall no longer doubt of the Moon's having an Atmoſphere.

XVII.

1] *An Eclipſe of the Moon* Oct. 10*th. N.S.* 1726. *at* Liſbon *by* F. J. Bap. Carbone. N° 400. *p.* 238.

Sub mediam noctem nubes diſpergi viſæ ſunt atque horâ ſecundâ occidentalis plaga ad quam Luna vergebat, nitidiſſima apparuit, eademque ad finem Eclipſeos permanſit, nullò igitur aëris incommodo Obſervationem hanc habui; nec quidquam prætermiſi, quod ejus rectitudini favere quoque modo intelligerem. Teleſcopio uſus ſum pedum 8 Pariſinorum, quod micrometro inſtruxeram accuratiſſime elaborato.

Ad Temporis verò dimenſionem horologio uſus ſum, pendulo inſtructo; cujus tanta æqualitas motus, ut dierum intervallo decem, & octo, vix uno, aut altero ſecundo à Medio Solis motu diſcrepâſſe deprehenderim.

Macularum nomina ex Selenographia *P. Grimaldi* excepta ſunt, quam *P. Ricciolus* ſuo Almageſto novo inſeruit, ac nominibus locupletavit. In eorum verò commodum, qui *Hevelianam* ſequuntur Nomenclaturam, ſynonymas quoque voces, ex hoc Auctore deſumptas, apponam, præpoſita litera H, quæ *Hevelium* ſignificat.

Temp. Ver. Correctum.			*Phaſes.*
H.	′	″	
14	37	0	Incipit penumbra ſenſibilis limbum Lunæ ad Euro-Auſtrum inficere.
14	46	0	Spiſſior apparet.
14	56	0	Spiſſiſſima.
14	57	20	Umbra Terræ, quantum diſcerni poteſt, ad eundem limbum pertingit.
15	3	50	Schickardus in Umbra.
15	4	53	I. Digitus obumbratur.
15	8	4	Umbra ad Kriſtmannum.
15	9	15	Immergitur totus.
15	9	50	Merſennus latet : Littus Orientale Maris Humorum incipit obumbrari : H. Sinus Sirbonis.
15	13	0	II. Digiti latent.
15	13	36	Capuanus immergitur.
15	14	54	Ad Grimaldum pervenit Umbra : H. Pal. Mareotis.
15	17	34	Gaſſendus incipit obumbrari.
15	19	46	Incipit Tycho : H. Mons Sinai. Grimaldus latet.
15	21	0	Tychonis medium tenet Umbra.
15	21	53	III. Digiti : Tycho totus immergitur.
15	23	58	Morinus : H. Caſſius.
15	25	20	Bullialdus : H. Inſula Creta.
15	30	58	Prophatius incipit obumbrari.
15	31	48	IV. Digiti.
15	32	40	Prophatius latet.
15	43	40	V. Digiti.
15	48	0	Umbra tantum non attingit Galilæum, quem propter diutiſſimè verſatur.
15	53	20	Ad Snellium, & Furnerium pervenit Umbra.
15	55	38	Ad Fracaſtorium. Snellius & Furnerius latent.
15	58	30	Fracaſtorius totus in Umbra.
16	3	4	VI. Digiti.
16	6	13	Sinum æſtuum tangit Umbra : H. Mare Adriaticum.
16	8	22	Medium Vendelini.
16	14	40	Totum Mare Nectaris in Umbra : H. Sinus extremus.
16	15	51	Grimaldus incipit emergere.

H.	′	″	
16	16	18.	Ricciolus extra Umbram.
16	23	30	Grimaldus totus.
16	24	27	Umbra ad Langrenum.
16	43	34	Gaſſendus reſtituitur.
16	47	22	V. Digiti deficiunt.
16	51	57	Bullialdus incipit emergere.
16	53	47	Totus extra Umbram.
16	54	17	Kriſtmannus incipit.
16	58	0	Item Schickardus.
16	59	27	IV. Digiti. Totus Schickardus.
17	3	30	Pitatus totus.
17	9	20	III. Digiti.
17	10	54	Tycho incipit emergere.
17	11	49	Tychonis medium extra Umbram.
17	12	40	Tycho totus emergit.
17	14	20	Fracaſtorius totus.
17	18	4	II. Digiti.
17	25	43	Snellius emergit.
17	26	10	I. Digitus tantum latet.
17	27	55	Furnerius extra Umbram.
17	32	50	Extremus limbus Lunæ, qui ad Africum ſpectat videtur proximè emerſurus.
17	33	30	Jam Lunæ diſcus integer apparet, ac proinde Finis Eclipſeos.
17	38	0	Adhuc denſiori penumbra prædictus limbus inficitur.
17	54	0	Deſinit penumbra ſenſibilis, ſuæque Luna claritati omninò reſtituta apparet.

H.	′	″	
2	36	10	Ex obſervatis Initio, ac Fine Eclipſeos colligitur ejus Duratio.
16	15	25	Medium, ſeu Maxima obſcuratio.
			Quantitas Micrometro diligenter inveſtigata, Dig. 6. min. 10′.

Temp. a Med. Noƈte.				*Phaſes.*
H.	'	"		
	49	0		Initium veræ Umbræ prox. Nod.
	51	0	Margo Umbræ attingit	Grimaldum.
	55	30		Ariſtarchum.
	59	0		Keplerum.
1	2	0		Mare Humorum.
	3	0		Gaſſendum.
	5	0		Sinum Irid. & Morinum.
	6	30		Copernicum.
	9	30		Bullialdum.
	11	0		Eratoſthenem.
	54	0		Platonem.
	59	0		Tychonem
	59	30		Aratum, toto Tych. obtecto.
2	22	0		Manilium.
	24	30		Menelaum.
	27	0		S. Dionyſium.
	29	0		Plinium.
	31	0		Poſſidonium.
	32	0		S. Catharinam.
	35	30		S. Theophil & Cenſorinum.
	37	30		Paludem Somni.
	39	30		Proclum.
	40	0		Goclenium, & littus orient. Maris Criſ.
	43	0		Lit. Occid. extremum Mar. Criſ.
	44	0		Langrenum.
	46	30		Immerſio totalis propè Nod. Occid.
3	27	30		Receptio 1ª. Lucis ad Nod. Orient.
	30	30		Emergit Grimaldi margo Orient.
	31	30	Emergunt ex Umbra	Ejuſdem margo Occident.
	32	30		Galilæus.
	36	0		Ariſtarchus.
	39	0		Keplerus.
	39	30		Littus Orient. Maris Humorum.
	43	0		Gaſſendus.
	49	30		Plato.
	51	0		Timochares.
	54	30		Tycho totus.
	59	0		Sinus æſtuum totus.
4	3	0		Manilius.
	6	0		Menelaus.
	9	0		Poſſidonius, & Endymion.

2] *The ſame at* Pekin, *by* F. Ignat. Kegler. N°. 405. *p.* 554.

Plinius.

H.	′	″		
	10	30	Emergunt ex Umbra.	Plinius.
	15	30		Cenſorinus.
	16	30		Palus Somni.
	19	0		Littus Or. Maris Criſ.
	22	0		Littus Occid. extremum.
	24	30		Langrenus.
4	26	0		Finis Eclipſis.

Horologium erat correctum per Culminationes Palilicii & aliquot Stellarum Orionis. Diameter Lunæ apparens immediate ante, & poſt Eclipſim dimenſa : 32′ 30″ proximè.

3] *The ſame at* Padua, *by* S. J. Poleni. N°. 395. *p.* 158.

Temp. Appar.			*Phaſes.*
H.	′	″	
16	16	44	Penumbra diluta.
	18	54	Penumbra denſior.
	21	19	Umbra ad Lunæ limbum.
	31	35	Attingit Mare Humorum.
	35	47	Attingit Grimaldum.
	38	34	Diſtat a Tychone diametro Tychonis ipſius, & Grimaldum tegit tertia ejuſdem Grimaldi parte.
	50	40	Fere attingit Pitatum.
17	3	41	Lansbergium tegit.
	7	45	Attingit Reinholdum.
	15	56	Attingit Fracaſtorium, & Galilæum.
	25	53	Attingit Mare Fœcunditatis.
	39	6	Umbra proxima eſt ad Reinholdum, tegitque partem tertiam Maris Fœcunditatis.
	46		Grimaldus emergit.
	54	53	Grimaldus jam diſtabat ab umbrâ diametro majore ſui integra.
18	5	44	Gaſſendus totus modo extra umbram, Mare Fœcunditatis dimidia circiter parte detectum, inter dehiſcentes nubes videbantur.
			Quæ nubes ad Finitorem deinde coactæ occiduam Cœli partem penitus obumbravère; neque Luna poſterius apparuit.
			Hæc Obſervatio Tubo optico optimæ notæ longo pedes Pariſienſes ſeptem habita fuit.

Theſe Obſervations were made by a nine Foot Glaſs. The Curious Obſerver having adjuſted a monthly Pendulum Clock by a Meridian Line on the 30th of *January*, and further corrected it by the Meridian, *February* the 6th 1728-9.

XVIII.

1] *An Eclipſe of the Moon at* Caſtle Dobbs *near* Carricfergus *in* Ireland, *Feb.* 2d. 172$\frac{8}{9}$. *by* A. Dobbs, *Eſq*; N°. 410. p. 140.

Appar. Time. P. M.			*Phaſes.*
H.	′	″	
6	27	0	Penumbra obſerved.
	29	30	Moon's Limb immerged.
	33	0	Eaſtern Limit of Palus Mareotis immerged.
	35	50	Mons Climax. immerged.
	42	40	Mons Porphyrites immerged.
	50	0	Inſula Melis immerged.
	52	40	Mons Ætna immerged.
	54	20	Inſ. Sardinia immerged.
	56	20	Inſ. Rhodus immerged.
	58	0	Inſ. Corſica immerged.
7	0	30	Mons Sinai's Eaſtern Limit immerged.
	2	0	Mons Sinai totally immerged.
	6	50	Inſ. Besbicus, Eaſtern Limit immerged.
	11	50	Mons Horminius immerged.
	13	50	Promontorium Acheruſium immerged.
	22	0	Mare Caſpium, Eaſtern Limit immerged.
	24	20	Palus Mæotis, Eaſtern Limit immerged.
	27	30	Palus Mæotis, totally immerged.
	30	15	Moon totally immerged.
9	8	30	Moon's Eaſtern Limb emerged near Mons Acabe.
	13	0	Palus Mareotis emerged.
	15	0	Mons Climax. emerged.
	23	30	Mons Porphyrites emerged.
	29	10	Mons Sinai emerged.
	33	30	Mons Ætna emerged.
	49	0	Inſ. Besbicus emerged.
10	1	0	Mare Caſpium, Eaſtern Limit emerged.
	5	0	Palus Mæotis emerged.
	10	0	A Penumbra obſerved, the Moon's Limb emerging.
	11	0	The Limb evidently emerged.

From the Beginning to the End of the Eclipſe 3h. 44′.
Totally Eclipſed——— 1h 38′ 15″.

2) *The ſame at* Rome, *from* J. B. Carbone N°. 410. p. 170.

Temp. Ver. H.	′	″	*Immerſiones.*	
7	44	22	Initium Eclipſis.	
	46	16	Grimaldi.	
	48	8	Kepleri.	
	54	20	Copernici	Initium.
	54	46		Medium.
	55	10		Finis.
8	11	57	Tychonis	Initium.
	13	7		Medium.
	13	48		Finis.
	19	0	Manilii.	
	20	50	Menelai.	
	23	0	Dionyſii.	
	25	44	Plinii.	
	31	6	Maris tranquillitatis	Medium.
	33	1		Totum.
	34	41	Procli	Initium.
	35	29		Finis.
	36	1	Maris Criſium	Initium.
	39	44		Finis.
	43	17	Lunæ totalis Immerſio.	

Temp. Ver. H.	′	″	*Emerſiones.*	
10	21	38	Primi Limbi Lunaris.	
	23	37	Riccioli.	
	24	7	Grimaldi	Initium.
	25	4		Finis.
	34	39	Ariſtarchi	Initium.
	36	8		Finis.
	41	11	Tychonis	Initium.
	42	5		Finis.
	47	10	Heliconis	Initium.
	48	14		Finis.
	54	33	Platonis	Initium.
	54	57		Finis.
	57	54	Ariſtotelis.	
11	2	5	Menelai.	
	4	33	Maris Serenitatis	Medium.
	9	15		Finis.
	14	36	Poſſidonii.	
	16	7	Cleomedis.	
	10	20	Maris Criſium.	Medii.
	17	36		Totius.

Finis Eclipſis 11^h 20′ 41″.

Eodem die diſtantia meridiana Centri ſolaris a vertice non correcta per Refractionem obſervata eſt 55° 9′ 31″ in Gnomone, cujus meridianam Eclipſis ſolaris in pavimentum projecta pertranſiit tempore 2′ 15″, & diameter apparens ſolis micrometri partes 2945 intercepit, quarum Luna paulò ante Eclipſim obſervata intercepit 2903.

Obſervationes habitæ ſunt Tubo optico pedum Romanorum 8 ¼.

3] *The ſame at* Paris. N°: 410. *p.* 171.

Tempus.			*Phaſes.*
H.	′	″	
7	1	0	Penumbra denſa.
7	3	0	Penumbra denſiſſima.
7	3	0	Eclipſis initium ex aliis phaſibus deductum.
7	8	50	Galilæus obtegitur.
	14	0	Umbra ad Ariſtarchum.
	15	4	Ariſtarchus totus in umbra.
	16	44	Keplerus obtegitur totus.
	18	4	Umbra ad Gaſſendum.
	19	20	Schickardus tectus omninò.
	22	0	Umbra ad Reinholdum.
	22	40	Ad limbum Copernici.
	23	43	Eratoſthenes obtectus.
	25	15	Copernicus totus in umbra.
	27	2	Helicon totus.
	31	50	Umbra ad limbum Tychonis.
	33	8	Tycho dimidius in umbra.
	33	30	Ad limbum præcedentem Platonis.
	33	47	Plato in umbra totus.
	38	7	Ad præcedentem limbum Manilii.
	39	20	Manilius totus.
	41	45	Umbra pervenit ad Menelaum.
	42	35	Ad Menelai dimidium.
	45	22	Ad Plinium.
	49	47	Ad præcedentem Fracaſtorii limbum.
	50	30	Ad Promontorium acutum.
	51	24	Umbra tegit Fracaſtorium.
	54	30	Pertingit ad Proclum.
	55	16	Proclum tegit totum.
	56	17	Ad limbum maris Caſpii.
	58	56	Ad dimidium.
	59	0	Ad limbum Caſpii ſequentem.
8	2	0	Finis dubius.
	3	0	Finis certus.

Tempus.			*Emerſiones.*
H.	′	″	
9	41	18	Emerſionis initium.
	41	33	Grimaldus incipit.
	45	40	Grimaldus emergit totus.
	49	35	Galilæus.
	51	30	Schickardus.
	54	34	Capuanus.
	55	16	Ariſtarchus incipit.
	56	5	Totus extra Umbram.
	58	35	Keplerus totus.
10	0	30	Primus Tychonis limbus.
	1	30	Dimidius Tycho extra umbram.
	2	30	Emergit totus.
	3	40	Lansbergius & Reinholdus.
	5	19	Incipit Copernicus.
	6	43	Copernicus totus.
	7	33	Emergit Eratoſthenes.
	8	0	Totus Helicon.
	12	56	Plato incipit.
	14	15	Totus extra Umbram.
	20	35	Manilius incipit.
	21	28	Totus emergit.
	23	50	Menelaus.
	27	25	Plinius.
	30	19	Dionyſius.
	31	0	Promontorium acutum.
	36	15	Proclus.
	37	26	Incipit Mare Caſpium.
	41	24	Finis dubius.
	42	0	Finis certus.

4] *At* Padua *by Signor* J. Poleni. No. 410. *p.* 173.

Temp. Appar. P. M.			*Phaſes* *Tubo optimo, pedes Pariſienſes ſeptem longo, conſpectæ.*
H.	′	″	
7	44	40	Obſervationem Initii Defectus nubes denſæ impedivere.
7	45	40	Umbra attingit Grimaldum.
7	50	53	Grimaldum tegit totum.
7	53	26	Attingit Mare Humorum.
8	19	34	Tegit Maris Humorum dimidiam partem.
8	38	10	Cooperit totum Mare Criſium.

H.	′	″	
9	26		Per dehiſcentes nubes Luna admodum rubicunda obſervari poterat perſpicue adeo, ut non meminerim aliàs in totali immerſione tam clare Lunam apparuiſſe; quod ita fortaſſe viſum eſt ob atram obſcuritatem, quam circumpoſitæ denſæ nubes efficiebant.
10	15	6	Umbra dilui incipit è regione proximæ emerſionis.
10	26	45	Grimaldus, jam emerſus, ab umbra diſtat tota fere ſua tranſverſa diametro.
10	31	40	Dimidium Mare Humorum diſcoopertum.
10	38	45	Tycho totus emergit.
10	50	12	Apparet Eratoſthenes.
11	13	27	Promontorium Somnii totum diſcoopertum.
11	19	45	Luna infici videtur ſola penumbra.
11	20	56	Finis etiam penumbræ.

XIX.

1] *An Eclipſe of the Moon,* July 29*th.* 1729. *at* Wirtemberg, *by* J. Weidler. N°. 410. *p.* 174.

Tempus.			*Phaſes.*
H.	′	″	
0	1	30	Initium ſub Grimaldo Mane d. 9 Aug. N.S.
0	3	45	Umbra tangit Galilæum.
0	6	0	Ariſtarchum.
0	11	0	Keplerum.
			Poſtea nubes condunt Lunam.
0	54	0	Tegitur mare Criſium dimidium.
0	57	0	M. Criſium totum.
1	1	0	Immerſio totalis.
2	40	30	Emerſio.
2	43	30	Emergere incipit Galilæus.
2	45	0	Ariſtarchus.
2	49	0	Keplerus.
2	54	45	Copernicus.
2	55	30	Plato.
3	1	30	Timocharis.
3	4	0	Manilius.
3	8	30	Emergit totus Tycho.

Emergere

H.	′	″	
3	10	30	Emergere incipit Menelaus.
3	13	0	Plinius.
3	18	0	Cleomedes.
3	23	30	M. Criſium & una M. Nectaris.
3	29	0	M. Nectaris totum emergit.
3	31	30	M. Criſium totum emergit.
3	34	30	Incipit emergere Langrenus.
3	40	0	Finis.
			Exeunte umbra inter Langrenum & Petavium.

2.] *The ſame at* Padua, *by Sign.* J. Poleni. N°. 410. *p.* 176.

Temp. Appar.			*Phaſes.*
H.	′	″	
0	0	28	Initium umbræ ad Lunæ Limbum.
	13	55	Umbra tangit Copernicum.
	15	49	Hunc totum tegit.
	22	24	Attingit Tychonem.
	24	14	Totum Tychonem cooperit.
	28	40	Attingit Manilium.
	30	15	Hunc totum cooperit.
	33	2	Menelaum tangit.
	34	22	Menelaum omnino cooperit.
	49	10	Attingit Mare Criſium.
	54	56	Mare Criſium totum cooperit.
	58	48	Totalis Immerſio.
2	37	38	Lux in Lunæ margine.
	41	20	Grimaldus extra umbram.
3	4	15	Mare Serenitatis emergere cœpit.
	6	16	Tycho totus emergit.
	7	28	Manilius totus diſcoopertus.
	10	30	Menelaus extra umbram.
	13	58	Mare Serenitatis totum emerſit.
	21	48	Promontorium Somnii jam extra umbram.
	23	10	Mare Criſium incipit emergere.
	25	28	Totum Mare Nectaris extra umbram, & dimidium Mare Criſium.
	29	0	Mare Criſium integrum apparet.
	33	20	Langrenus extra umbram.
	38	8	Finis Emerſionis ab omni etiam penumbra.

3] *At* Bononia *by Sign.* Euſtachio Manfredi. N°. 411. *p.* 215.

Temp. Ver.			*Phaſes.*
H.	′	″	
11	56	52	Eclipſis certè incepta.
12	11	33	Initium Copernici.
	12	56	Centrum Copernici, ſed ex alterâ Determinatione 2″ citiùs.
	19	46	Initium Tychonis, ſed ex alterâ Determinat. 2″ citiùs.
	20	54	Medium Tychonis.
	21	43	Totum Tychonem.
	23	43	Initium Platonis.
	24	42	Medium Platonis.
	25	23	Totum Platonem.
	25	55	Inſula in Sinu medio.
	27	35	Totum Manilium.
	29	35	Totum Ariſtotelem.
	32	7	Totum Menelaum.
	35	0	Totum Plinium.
	38	49	Promontorium ſomni.
	39	26	Promontorium Acutum.
	44	16	Totum Fracaſtorium.
	45	42	Totum Proclum.
	46	59	Initium Maris Criſium.
	49	47	Medium Maris Criſium, ſed ex alterâ Determinat. 4″ citiùs.
	52	19	Totum Mare Criſium.
	53	6	Totum Petavium.
12	55	54	Totalis Immerſio Lunæ.
14	34	25	Initium Emerſionis dub.
	37	30	Initium Grimaldi.
	38	20	Totum Galilæum.
	38	28	Totum Grimaldum.
	39	45	Totum Ariſtarchum.
	44	47	Totum Keplerum.
	48	33	Initium Platonis.
	49	37	Medium Platonis.
	50	42	Totum Platonem.
	52	47	Totum Copernicum.
	55	32	Totum Bullialdum.
15	1	56	Initium Tychonis.
	2	36	Medium Tychonis.
	3	50	Totum Tychonem.
	4	50	Totum Manilium.

H.	′	″	
15	7	47	Totum Menelaum.
	11	2	Totum Dionyſium.
	11	37	Totum Plinium.
	18	53	Promontorium acutum.
	20	30	Initium Maris Criſium.
	20	59	Totum Proclum.
	23	34	Medium Maris Criſium.
	35	00	Finis Eclipſis.

4] *At* Rome. N°. 411. *p.* 217.

Tempus.			*Immerſiones.*	
H.	′	″		
12	1	0	Umbra ad ☽ limbum.	
	7	49	Kepleri	Initium.
	9	4		Medium.
	9	50		Finis.
	15	0	Copernici	Initium.
	16	26		Medium.
	17	0		Finis.
	17	11	Heraclidis	Initium.
	17	27		Medium.
	17	40		Finis.
	22	26	Heliconis	Initium.
	22	41		Medium.
	23	7		Finis.
	23	50	Tychonis	Initium.
	24	41		Medium.
	25	25		Finis.
	28	43	Platonis	Initium.
	29	14		Medium.
	29	50		Finis.
	31	5	Manilij	Initium.
	32	0		Medium.
	32	45		Finis.
	35	4	Menelai	Initium.
	35	45		Medium.
	36	8		Finis.
	51	37	Maris Criſium	Initium.
	54	10		Medium.
	56	8		Finis
13	0	16	Totalis Immerſio.	

Tempus.

H.	′	″	*Emerſiones.*	
14	38	24	Lux ad Lunæ limbum	
	43	24	Grimaldi Finis	
	44	34	Kepleri Finis	
	46	14	Heraclidis,	Initium.
	46	54		Medium.
	47	24		Finis.
	49	10	Heliconis,	Initium.
	50	4		Medium.
	50	44		Finis.
	51	24	Platonis,	Initium.
	52	9		Medium.
	52	44		Finis.
15	7	5	Tychonis,	Initium.
	7	13		Medium.
	8	18		Finis.
	26	39	Maris Criſium,	Initium.
	28	38		Medium.
	31	51		Finis.
	38	0	Totalis emerſio.	

Obſervationes habitæ ſunt teleſcopio pedum Romanorum 9, aere innubi ſed vaporoſo. Diameter Lunæ horizontalis capta 15 h. 46′. intercipiebat micrometri partes 2934, quarum verticalis Lunæ diater comprehendebat 2877, at Solis diameter die præcedenti viſa eſt occupare partes 2830.

H.	′	″	
13	0	16	Tempus Immerſionis totalis.
14	38	24	Tempus Emerſionis primæ.
1	38	8	Mora in tenebris.
3	37	0	Duratio Eclipſis.

Solis Meridiani refractione omiſſâ, tangentes in Gnomone, cujus aperturæ horizontalis diameter $\frac{70}{100000}$

7 Auguſti	48190 47040
8 Auguſti	48801 47731

XIX.
5.] *The ſame Obſer. Total at* Barbadoes, *by Mr.* Stevenſon. N°. 416. *p.* 441.

I took care to regulate a very good Clock, and brought it to true Time about 14 Days before the Eclipſe. On the Day it happened, I ſaw the Sun ſet, and found the Clock right according to the mean Time, refraction allow'd. At the Beginning of the Eclipſe, the Moon was clouded.

Appar. Time.			*Phaſes.*
H.	′	″	
7	18	0	2 Digits, about 30° to the left of her Nadir Point.
8	11	0	She totally immerged into the Earth's Shadow, about 30° to the right of her vertical Point.
9	51	0	She emerged, 79° or 80° to the left of her Nadir Point.
10	50	0	The Eclipſe ended, 88° to the right of her vertical Point.

In this and all the other Obſervations of Solar and Lunar Eclipſes I have made for ſeveral Years in *Barbadoes*, I found they always happened 10 Minutes ſooner than my Computation; whence I conclude *Barbadoes* lies 2° 30′. more weſterly, than is generally ſuppoſed.

XX.
An Eclipſe of the Moon Aug. 19, 1728. N. S. *at* Pekin. N°. 414. *p.* 368.

Horol. Corr.			*Phaſes.*
H.	′	″	
			Non multo ante Eclipſim, dimenſa diameter Lunæ erat 30′ 50″.
10	54	0	Penumbra jam inficiebat partes Lunæ primo inumbrandas.
11	2	0	Initium Eclipſis paulo infra Cleoſtratum.
	13	0	Umbra attingit Ariſtarchum.
	14	30	Obtexit totum.
	15	20	Attingit Platonem.
	16	50	Obtexit totum.
	22	20	Attingit Galileum & Timocharim.
	23	20	Pytheam.
	26	30	Keplerum.
	27	30	Ariſtyllum.
	31	30	Hevelium, Copernicum, & Endymionem fere ſimul.
	36	20	Ricciolum.
	38	15	Poſſidonium.

Grimaldum

H.	′	″		
11	40	10	Grimaldum, & Mercurium.	
	41	40	Manilium.	
	43	40	Menelaum.	
	47	0	Plinium, & Geminum.	
	52	0	Umbra ad centrum Lunæ, obtecto Grimaldo toto.	
	54	20	Attingit Mare Criſium	Grimaldi apice auſtrali hærente ad marginem umbræ.
	56	40	Ariadæum	
	57	0	Proclum	
12	0	0	Culminante Luna, recta per medium Tychonem tendens inter Munoſium, & Prophatium ad Copernicum coincidit cum plano Meridiani.	
	2	30	Umbra attingit Promontorium acutum,	Lente admodum emergente Grimaldo.
	4	30	Cenſorinum & Taruntium,	
	6	0	Mare Criſium totum obtectum,	
	15	30	~~Umbra attingit S. Theophilum,~~	
	16	30	S. Cyrillum,	
	21	30	Langrenum, Grimaldo integre emerſo.	
	25	15	S. Catharinam, Ricciolo toto retecto.	
	31	0	Circa medium Eclipſis micrometro dimenſa quantitas obſcurationis erat Sinice digit. 6½ ferme, ſive more Europæo digit. 7¼.	
	34	0	Emerſit Hevelius totus.	
	36	0	Umbra ad Fracaſtorium.	
	43	0	Galileus	integre detecti.
	46	30	Lansbergius	
	52	0	Keplerus	
13	1	0	Ariſtarchus	
	2	0	Copernicus incipit emergere.	
	5	0	Totus detegitur.	
	10	0	Margo umbræ per centrum Lunæ.	
	11	30	Pytheas emerſit.	
	15	0	Eratoſthenes & S. Cyrillus detecti.	
	20	0	Timocharis & S. Theophilus detecti.	
	22	20	Ariadæus	integre emerſerunt.
	25	0	Manilius	
	29	30	Ariſtyllus	
	32	0	Plato	
	33	0	Cenſorinus	
	34	0	Promontorium acutum.	
	38	0	Plinius & Langrenus integrè detecti.	
14	0	0	Finis Eclipſis proximè Beroſum.	
			In fine Eclipſis diameter Lunæ inventa eſt 30′ 38″.	

Durante Eclipſi ſæpius addenſati vapores turbabant faciem Lunæ, ut ejus maculæ, & umbræ margo non ſatis diſtinctè poſſent diſcerni; maxime id accidit ante, & circa finem Eclipſis.

XXI. *A Total Eclipſe of the Moon* Feb. 14*th*. 1729. N.S. *at* Pekin. N°. 416. *p*. 460.

Tota illa nocte continenter modicè ningebat, cœlo tamen ſic tenuiter nubilato, ut Lunares maculæ ſæpius utcunque diſtingui poſſent; quanquam rarius ac difficilius tempore immerſionis: Sub emerſionem enim paulatim cœlum ſerenatum fuit, ut circa finem jam penitus innube exiſteret.

Horologium correctum fuit per altitudines Arcturi & Aquilæ, item ex culminante Spica *Virginis* ac Lance borea *Libræ*. Diameter Lunæ initio eclipſis micrometro dimenſa, erat 32′ 0″. Erantque in linea verticali cum centro Lunæ Pythagoras & Helicon.

Temp. A. M.			*Phaſes.*	
H.	′	″		
2	38	30	Initium eclipſis contra Hevelium.	
	41	0	Umb. ad	Grimaldum.
	42	30		Qui totus immerſit.
	43	0		Galileum.
	47	0		Ariſtarchum.
	48	30		Keplerum.
	50	0		Gaſſendum.
	58	0		Copernicum.
3	3	30		Sinum æſtuum orientaliorem.
	9	0		Tychonem.
	17	30		Menelaum.
	24	30	Poſſidonius totus in Umbra.	
	26	0	Umb. ad	Fracaſtorium.
	31	0		Proclum.
	32	0		Mare Criſium.
	35	30		Langrenum.
	39	0	Immerſio totalis inter Langrenum, & mare Criſium.	
5	17	10	Emerſio prima lucis infra Grimaldum.	
	21	0	Grimaldus prodire incipit.	
	22	25	Totus emerſit.	
	28	0	Emerſit Gaſſendus.	
	30	35	Keplerus.	
	36	40	Umbra per centrum Tychonis.	
	37	20	Totus prodiit.	

Prodiit

H.	′	″		
	40	35		Prodiit Copernicus.
	46	28		Plato incipit emergere.
	48	30		Totus detegitur.
	50	0	Emerſēre	Sinus æſtuum.
	53	50		Archytas.
	55	20		Manilius.
	57	15		Ariſtoteles.
	58	45		Menelaus.
	59	10		Ariadæus.
6	0	50		Fracaſtorius.
	2	30		Reſtat in umbra ¼ diametri Lunæ.
	2	50	Prodeunt	Plinius.
	5	45		Poſſidon. Vitruv. & Cenſorinus.
	10	0		Taruntius.
	10	30		Proclus.
	13	10		Langrenus totus detectus.
	13	30		Mare Criſium incipit emergere.
	16	30		Totum prodiit.
	17	40		Finis eclipſis contra Mare Criſium, exiſtentibus tum in linea verticali per centrum Lunæ Oenopide ac Heraclide.

XXII.

A Lunar Eclipſe Feb. 2, 1730. *N. S. at* Lisbon *by* P. J. Bapt. Carbone, N° 414. *p.* 363.

Temp. Ver. P. M. H.	′	″	*Phaſes.*
13	25	0	Incipit penumbra ſenſibilis.
	40	0	Fit ſpiſſior.
	58	0	Fit ſpiſſiſſima.
14	3	45	Dubitatur de Eclipſis initio.
	4	32	Nunc certo incipere videtur.
	6	0	Jam diſcus Lunæ apparet deficiens.
	9	47	Umbra attingit plagam Borealem Terræ Pruinæ.
	10	25	Pervenit ad Harpalum.
	11	6	Medium Harpali tenet.
	16	15	Attingit Littus Boreale Sinus Iridum.
	18	34	Heraclides totus tegitur.
	22	38	Plato incipit.
	23	50	Medius Plato latet.
	24	54	Totus Plato obumbratur.
	29	40	Umbra ad Ariſtarchum.
	31	55	Ad medium Ariſtarchi.
	33	42	Totum Ariſtarchum occultat.

H.	′	″	
14	34	55	Ariſtoteles obumbrari incipit.
	36	24	Medius Ariſtoteles tegitur.
	37	49	Ariſtoteles totus in umbra.
	39	9	Eudoxus totus.
	43	57	Umbra attingit Endymionem, & Ariſtyllum ſimul.
	44	53	Medius Endymion, & totus Ariſtyllus latet.
	45	48	Endymion totus.
	48	27	Timocharis totus; umbra pervenit ad Littus maris Serenitatis.
	55	50	Ad Lacum Somniorum.
	56	30	Ariſtarchus incipit emergere.
	58	20	Medius Ariſtarchus extra umbram.
15	0	34	Ariſtarchus totus emergit.
	4	25	Poſſidonius incipit obumbrari.
	11	35	Lacus Somniorum totus, & dimidium Poſſidonii occultatur.
	13	12	Timocharis incipit emergere.
	16	5	Timocharis emergit totus; & totus Poſſidonius occultatur.
	27	54	Archimedes totus extra umbram.
	30	49	Poſſidonius incipit emergere.
	32	58	Heraclides totus.
	34	3	Poſſidonius totus.
	40	46	Harpalus totus.
	46	21	Platonis initium.
	47	16	Platonis medium.
	48	33	Plato totus extra umbram.
	50	55	Lacus mortis totus.
	52	37	Ariſtoteles incipit emergere.
	54	29	Ariſtoteles medius extra umbram.
	56	58	Ariſtoteles totus.
16	1	48	Endymionis initium.
	3	14	Endymion totus.
	4	0	Finis Eclipſis.
			Duratio Eclipſis 4 h. 59′ 28″.
			Medium Eclipſis 15 h. 4′ 16″.
			Quantitas Digit. 3 Min. 20. ad Boream.

Hæc

XXIII.

1] *An Eclipſe of the Moon at* Chamxo *in the Province of* Nankin, July 29. *N. S.* 1730. *by* P. Jac. Simonelli. N° 424. *p.* 320.

Hæc Eclipſis *Pekini* ob denſe nubilatum cœlum non potuit obſervari. Eam tamen obſervavit *P. Phil. Jac. Simonelli* in urbe *Chamxo* Provinciæ *Nankinenſis*, quæ *Pekino* ad ortum diſtat paulo plus 4 gr. Æquatoris. i. e. 16 vel 17 min. temporis.

Phaſes.	*Temp.* P. M.		
	H.	′	″
Initium Eclipſis ibi fuit	10	55	0
Finis	12	49	0
Maxima obſcuratio, digitorum Sinicorum 3. 10′			
Itaque medium accidit	11	52	0
Quod calculus pro *Pekino* dabat	11	36	0
Cum differentia ſatis juſtâ	00	16	0

2] *The ſame at* Cochinchina, *by* P. Franciſcus de Lima. N° 424. *p.* 320.

Eandem Eclipſim in Regiâ *Cochinchinæ* obſervavit *P. Franciſcus de Lima.*

Phaſes.	*Temp.* P. M.		
	H.	′	″
Initium annotavit	9	48	0
Et finem	11	50	0
Adeoque medium erat	10	54	0
Medium Eclipſeos *Pekini* ex calculo	11	36	0
Unde exurgit differentia ejus Meridiani a *Pekino*			
Ad Occid. 42′ temporis *i. e.* 10°. 30′ Æquatoris	00	42	0

XXIV.

An Eclipſe of the Moon June 28, 1721. *by Mr.* Robie *in* New England. N° 423. *p.* 272.

About two in the Morning Mr. *Robie* viewed the *Moon* with his eight Foot Teleſcope, and ſhe was untouched.

Time Correct.			*Phaſes.*
H.	′	″	
2	10	0	A thin Penumbra.
2	12	0	The Shadow is plainly entered.
2	18	10	*Palus Mareotis* covered.
2	31	40	*Mons Porphyrites* touched.
2	34	20	—— —— covered.

H.	'	"	
2	47	10	Moon Eclipſed about ſix Digits.
2	49	05	*Beſbicus* juſt touched.
2	50	30	——— ——— covered wholly.
2	53	40	*Byzantium* touched.
2	54	10	——— ——— covered.
3	05	40	*Palus Mæotis* touched.
3	18	30	Moon wholly covered.

There remained a Light on the Weſtern Side of the Moon for ſome Time.

About 3ʰ 50′ in the Morning, the Moon was wholly hid by the Haze and coming on of Day-Light, that nothing could be ſeen of her; although from the Immerſion 'till now ſhe was viſible.

XXV.
1] *Aſtronomical Obſervations by Mr.* Robie *at* Harvard *College in* New England. Nº 423. *p.* 270.

		Southings of the Moon.				
			H.	'	"	
Octob.	5th.	1717	9	32	0	P. M.
Sept.	24	1718	9	38	0	there-abouts.
	25		10	22	32	P. M.
	26		11	26	0	P. M.
Dec.	19		6	45	45	P. M.
	20		7	30	36	
	23		9	54	5	
	25		11	47	33	
Jan.	17	1719	5	52	1	
	19		7	33	1	
	22		10	21	40	P. M.
Feb.	16		6	15	15	
	19		8	59	40	
	21		10	54	30	P. M

2] *The* Semita luminoſa *of Dr.* Childrey *Obſerv.* Dec. 10, 1720. *Fig.* 103.

On *Dec.* 10, 1720, about 8ʰ *P. M.* Mr. *Robie* firſt ſaw the *Light* that ſtrikes up toward the *Pleiades*; and on *Jan.* 6, following, he found it was increaſed, and almoſt reached to the *Pleiades*. And *Dec.* 7, 1721, he obſerved the ſame; and on the 25th he hath given this Figure of it: *b o* is the Part next the Horizon; V the Point toward the *Pleiades.*

This *Glade of Light* is the ſame that Dr. *Childrey* mentions in his *Britan. Bacon.* under the Name of *Semita luminoſa*; and which I ſaw, and gave a Figure of in *Philoſ. Tranſ.* Numb. 305.

An Eclipſe of the Sun Sept. 23d. 1712.

3]N° 423. *p.* 270.

Phaſes.	P. M. H.	′	″
The Beginning	12	23	0
The Middle nearly at	1	47	0
The End	3	5	10
About 9 Digits were eclipſed.			

4]N° 423. *p.* 273.

On an Eclipſe of the Sun, Nov. 27, 1722. Vide *p.* 173.	H.	′	″
At 8^h $55'$ $15''$ the Sun was eclipſed $4\frac{1}{4}$ neareſt; and then the Sun's Diameter was to the Moon's as 1000 to 972.	8	55	15
At 9^h $00'$ $15''$. were hid $4\frac{1}{2}$ Digits nearly, and the Sun's Diameter was to the Moon's as 1000 to 975.	9	00	15

XXVI. *An Eclipſe of the Moon,* Novemb. 1. 1724. N. S. *by Mr.* Maraldi *at* Paris. N° 385. *p.* 186.

Temp. Ver. H.	′	″	*Phaſes.*
2	33	30	Commencement.
2	46	15	L'ombre à Ariſtarque.
2	56	20	L'ombre à Galilée.
3	20	30	— au bord Septentrional de la mer Caſpienne:
3	23	30	L'ombre à Proclus.
4	14	30	Ariſtarque ſort de l'ombre.
4	17	50	Tout Copernic eſt hors de l'ombre.
4	33	34	Timocharis eſt ſorti de l'ombre.
4	44	23	Platon eſt entierement hors de l'ombre.
5	6	30	Fin de l'eclipſe.
2	33	0	La durée de l'eclipſe.

XXVII.

1] *Of* Aldebaran, *at* Harvard *College in* New Eng. *by Mr.* Robie. N° 428. *p.* 270.

1] On *Feb.* 25. 171$\frac{7}{8}$. Mr. *Robie* ſaw the Moon cover *Aldebaran* at about 9^h 18′ P. M. and the Star to emerge at 10^h 20′, P. M. then by his Meridian Inſtrument deſcribed in *Tranſ.* N° 291. being 2′ too ſlow, ſo that 2′ are to be added to the Time mentioned.

2] Spica Virginis *at* Bologna, *by* S. Euſt. Manfredi. Mart. 9. *N. S.* N° 404. *p.* 534.

2] 8^h 50′ 6″, Die 9 Mart. 1727. emerſit Spica Virginis è limbo Lunæ obſcuro.

3] *Occultations of ſeveral fixed Stars in* 1728. *obſerved at* Pekin. N° 414. *p.* 370.

3] 1728. *Januar.* die 2. manè, Luna occultavit Stellam *c Leonis*. Immerſio erat 2^h 35′ 20″ in recta per Tychonem & S. Theophilum; Emerſio fuit 3^h 20′ 40″ in recta per S. Theophilum & Eratoſthenem.

Die 22: Summo manè, Luna tranſivit per *Pleiadas*.

1^h 0′ 25″ immerſit *Taygete* poſt Lunam, in recta cum Bullialdo, & Abulfeda.
1^h 9′ 30″ *Celæno*, à Cuſpide cornu auſtrali pauculis ſecundis diſtans in recta ex Tychone per Clavium, mox diſparuit nimia fluctuatione lucidi limbi Lunæ abſorpta.
1^h 18′ 24″ immerſit *Sterope* in recta cum Bullialdo, & Fracaſtorio.
1^h 25′ 56″ *Maia* in recta ex Tychone per Longomontanum.

Emerſio nullius videri poterat ob nimiam fluctuationem lucis Lunæ inter vapores.

Die 29 veſp. Luna obtexit Stellam *τ Leonis*. Immerſio fuit 9^h 27′ 53″ in recta cum Galileo, & Lanſbergio. Emerſio verò 10^h 24′ 17″ in recta cum Macrobio & Soſigene.

Mart. die 21 veſp. occultavit Luna Stellam *γ Cancri*. Immerſio fuit 8^h 14′ in recta per Copernicum & Boreum marginem Langreni. Emerſio fuit neglecta.

Maii die 24. Summo manè 1^h 51′ 30″ Luna abſorbuit Stellam *τ Scorpionis* proxime Byrgium. Emerſio non fuit obſervata.

Sept. die 14 veſp. Luna occultavit Stellam *η Capricorni*. Immerſio fuit 8^h 11′ 20″ inter Seleucum & Cardanum. Emerſio 9^h 37′ 30″ paulo infra Langrenum.

Die 19 veſp. Luna obtexit Stellam. *δ Piſcium*. Immerſio fuit 8^h 43′ 45″ in recta per Tychonem & Langrenum. Emerſio autem 9^h 5′ 15″ in recta cum Tychone & Keplero.

Oct. die 28 manè Luna occultavit Regulum, ſeu Cor *Leonis*. Immerſio fuit 1^h 39′ 50″ in recta per Ariſtarchum & Gaſſendum. Emerſio 2^h 11′ 15″ in recta per Ariſtarchum & Cardanum.

4] *The Continuation to* Nov. 1729. *at* Pekin. N° 416. *p.* 455.

Nov. die 20 5^h 0′ 42″ manè Luna obtexit ſtellam *ν Leonis*; locus immerſionis erat proxime contra Roccam.

6^{h} 21′ 55″ prodiens Stella ſtabat in recta cum Reinholdo & Grimaldò ; adeoque locus emerſionis prope Beroſum, & tranſitus ferme centralis.

1729. *Mart.* die 8. 11^{h} 18′ P. M. Luna obtexit ſtellam boreo-orientalem trapezii, quod eſt infra pedes aurigæ. 12^{h} 12′ emerſit ſtella è regione Meſſallæ. Die 11. 7^{h} 56′ 3″ veſp. Luna obtexit ſtellam η *Cancri.* Locus immerſionis erat contra Schickardum. Emerſio, quæ fuit contra Petavium, paulo tardius notata eſt 9^{h} 2′ 30″ ; accideret autem proxime 8^{h} 59′.

April. die 2. veſp. Conjunctio Lunæ cum *Pleiadibus.*

8^{h} 23′ 2″ Luna obtexit ſtellulam borealiorem trianguli quaſi æquilateri, quod præcedit Pleiadas : Locus immerſionis contra Phocyllidem. 9^{h} 2′ 23″ abſorbuit ſtellam claram, quæ eſt ſupra Pleiadas ferme in recta linea cum *Taygete* & *Electra:* Locus immerſionis videbatur eſſe contra Cardanum. 9^{h} 9′ 25″ Luna obtexit *Taygeten,* cujus immerſio erat contra Cabæum prope cuſpidem Lunæ auſtralem. 9^{h} 18′ 58″ immerſa eſt præcedens *Steropes,* contra Bartolum. 9^{h} 25′ 27″ immerſa eſt ſequens prope Caſatum.

Emerſiones non poterant videri ob nimiam undulationem lucidi limbi Lunæ atmoſphæram ſubeuntis.

Die 11. 8^{h} 12′ P. M. Luna obtexit ſtellam ν *Leonis* directe contra Schickardum, ſtante Meſſalla in vertice Lunæ. Emerſit Stella 9^{h} 11′ 30″ paulo infra Langrenum, verticem Lunæ obtinente Mercurio.

Nov. die 7. manè tranſitus Lunæ per *Pleiadas,* cum borealium occultatione, ut ſequitur.

	H.	′	″	
	4	51	10	Immerſit *Celæno* contra Zucchium.
	4	53	6	Immerſit *Taygete* contra Crugerum.
	5	17	30	Immerſit *clara Steropes* ſupra Ricciol.
	5	18	20	Immerſit *Maja* contra marg. occ. Schickardi.
dub.	5	21		Immerſit *ſequens* Steropen contra Roccam.
	5	37	10	Emerſit *Celæno* recta contra Petavium.
	6	2	20	Emerſit *Taygete* inter Langrenum, & mare Criſium.
	6	15	30	Emerſit Maja ad bor. Wendelini.

Emerſio *Steropes* ob diluculum nequit videri.

Eodem die veſp. 7^{h} 30′ 34″ ab Luna occultata fuit χ *Tauri* paulo infra Galileum, quæ rurſum emerſit 8^{h} 33′ 15″ paulo ſupra Langrenum.

5]*Occultations of the* Pleiades *and ſome other fixed Stars obſerved in* 1731. *at* Pekin. N° 424. *p.* 319.

P. M.			*Die* 17 Jan. 1731. *Obſervatus eſt tranſitus Lunæ per Pleiadas ut ſequitur.*
h.	′	″	
0	9	40	Immerſit *Electra* in recta per *Platonem* & *Eudoxum*.
10	32	52	Immerſit *Merope* in recta per *Copernicum* & *Meſſallam*.
10	38	15	Emerſit *Electra* in recta per *Thaletem*, & *Eudoxum*.
11	23	52	Immerſit præcedens lucidam *Pleiadum* (triplex Stellula) in recta per *Eratoſth.* & S. *Cyrillum*.
11	26	5	Immerſit lucida, ſeu *Alcyone*, in recta per *Copernicum* & S. Catharinam.
11	47	32	Emerſit *Merope* in recta per Taruntium & S. Theophilum.
12	1	10	Immerſit lucidior ex parvis ad Auſtrum *Atlantis*, in recta per *Bulliald*, & *Cenſorinum*.
12	12	12	Immerſit *Atlas* in recta per *Copernicum* & Jul. Cæſarem.
12	13	57	Emerſit *Alcyone* in recta per Marginem Orient. *Poſſid.* & *Menelaum*.
12	25	3	Immerſit *Pleione* in recta per Copernicum & Ptolomæum.

1731. Die 14 *Mart.* ☽ occultavit Stellam κ in ♉. Immerſio accidit H. 8, 41′ 50″ *P. M.* in recta per Taruntium, & Langrenum. Emerſit H. 9, 51′ a Firmico modicè ad Auſtrum.

Die 20 *Mart.* Luna occultavit Stellam π in ♌. Immerſio fuit H. 11, 13′ *P. M.* in recta per Merſennum, & Bullialdum. Emerſio H. 12, 31′ è regione Firmici.

Die 16 *April.* Luna occultavit Stellam ο, in ♌. Immerſio fuit H. 8, 46′ 30′ *P. M.* in recta per Bullialdum & Cenſorinum. Emerſio H. 10, 5′ 45″ in recta per Taruntium, & Menelaum.

XXVIII. *Obſervations on the Spot* Plato *in the Moon ſeen* Aug. 16. N.S. 1725. *by* S. Bianchini *at* Rome. N° 396. *p.* 181. *Fig.* 104.

Margines elevati in ambitu maculæ perfundebantur luce Solis, & candorem conſuetum oſtendebant: fundus maculæ tenebroſus ſpectabatur, cum ad illum radii ſolares nondum pertingerent. Sed projectio lucis minus candidæ, imò nonnihil rubeſcentis, pervadebat mediam aream maculæ (ut in *Fig.* 104.) non ſecus, ac ſi in latere marginis A Soli obverſo foramen aliquod fuerit, per quod radius Solis admitteretur.

Duplex innuitur cauſa, unde prædictus effectus procedere poſſet; vel ſcilicet foramen in latere Marginis Soli obverſo; vel refractio

alicujus radii ſolaris in ſummitate marginis facta, unde interiores partes ipſius maculæ pervaderet radius. Utrumque ſanè probabile, & utrumque pariter confirmat dari circa Lunam Atmoſphæram noſtræ non abſimilem; ſive enim foramen admittamus, per quod Solis radius introducatur, & hîc ſane neutiquam videri poſſet in obſcuro cavo maculæ, niſi ab exhalationibus lucem reflectentibus fieret conſpicuus; vel admittamus refractionem, & hæc ſine intermedio craſſiori dari nequit. *Hæc obſervavit vir clariſſ. horâ primâ poſt Solis occaſum in monte Palatino, tubo optimo J. Campani Palmorum* 150 *Romanorum.*

XXIX.

1] *Obſervat. on* Saturn *and* Jupiter, *made by the Reverend Mr.* J. Pound, *with Mr.* Hadley's *reflecting Teleſcope.* N° 378. *p.* 382.

Mr. *Bradley*, the *Savilian* Profeſſor of Aſtronomy, and myſelf, have compared Mr. *Hadley*'s Teleſcope (in which the focal Length of the Object Metal is not quite 5 Feet and $\frac{1}{4}$) with the *Hugenian* Teleſcope, the focal Length of whoſe Object Glaſs is 123 Feet: And we find, that the former will bear ſuch a Charge, as to make it magnify the Object as many Times as the latter with its due Charge; and that it repreſents Objects as diſtinct, though not altogether ſo clear and bright; which may be occaſioned partly from the Difference of their Apertures (that of the *Hugenian* being ſomewhat the larger) and partly from ſeveral little Spots in the concave Surface of the Object Metal, which did not admit of a good Poliſh.

Notwithſtanding this Difference in the Brightneſs of the Objects, we were able, with this reflecting Teleſcope, to ſee whatever we have hitherto diſcovered by the *Hugenian*; particularly the Tranſits of *Jupiter*'s Satellites, and their Shades, over the Diſk of *Jupiter*; the black Liſt in *Saturn*'s Ring; and the Edge of the Shade of *Saturn* caſt on his Ring, as repreſented by *Fig.* 4. *Plate* 2. of the forementioned *Tranſact. Numb.* 376. or *Fig.* 105. of this Abridgment.

Fig. 105.

We have alſo ſeen with it ſeveral Times the 5 Satellites of *Saturn*; in viewing of which this Teleſcope had the Advantage of the *Hugenian*, at that Time when we compared them; for it being in Summer, and the *Hugenian* Teleſcope being managed without a Tube, the Twilight prevented us from ſeeing in this ſome of thoſe ſmall Objects, which at the ſame Time we could diſcern with the reflecting Teleſcope.

2] *The ſame by* John Hadley, *Eſq; V. P. extracted from the Minutes of the* R. S. N° 378. *p.* 385.

Mr. *Hadley* gave the *Society* a Relation of ſome of the moſt remarkable Obſervations, which he had made with his Reflecting Teleſcope, before he preſented it to the *Society*.

In obſerving *Jupiter*'s Satellites he has ſeen diſtinctly the Shadows of the firſt and third Satellites caſt upon the Body of the Planet.

In obſerving *Saturn* laſt Spring, at the Time when that Planet was about 15 Days paſt the Oppoſition, he ſaw the Shade of the Planet caſt upon the Ring, and plainly diſcerned the Ring to be diſtinguiſhed into two Parts, by a dark Line, concentric to the Circumference of the Ring. The outer or upper Part of the Ring ſeemed to be narrower than the lower or inner Part, next the Body, and the dark Line, which ſeparated them, was ſtronger next the Body, and fainter on the outer Part towards the upper Edge of the Ring. Within the Ring he diſcerned two Belts, one of which croſſed *Saturn* cloſe to its inner Edge, and ſeemed like the Shade of the Ring upon the Body of *Saturn*; but when he conſidered the Situation of the Sun, in reſpect to the Ring and *Saturn*, he found that Belt could not ariſe from ſuch a Cauſe.

He ſays, that at Times he has ſeen with this Teleſcope three different Satellites of *Saturn*, but could never have the Fortune to ſee all five.

Aug. 1723. Mr. *Hadley* adds, that he has ſeveral Times ſeen the Shadow of the firſt, ſecond, and third Satellites of *Jupiter* paſs over the Body of that Planet, and that he has ſeen the firſt and ſecond appear, as a bright Spot upon the Body of *Jupiter*, and has been able to keep Sight of them there for about a Quarter of an Hour, from the Time of their entring on his Limb.

Jupiter's Satellites have of late Years been ſo ſituated, with regard to the Earth and *Jupiter*, that he has not had ſufficient Opportunity of obſerving the Tranſit of the fourth Satellite, or of its Shadow.

The dark Line on the Ring of *Saturn*, parallel to its Circumference, is chiefly viſible on the *Anſæ*, or Extremities of the Elliptick Figure, in which the Ring appears; but he has ſeveral Times been able to trace it very near, if not quite round; particularly in *May*, 1722, he could diſcern it without the Northern Limb of *Saturn*, in that Part of the Ring, that appeared beyond the Globe of the Planet. The Globe of *Saturn* (at leaſt towards its Limb) reflects leſs Light than the inner Part of the Ring, and he has ſometimes diſtinguiſhed it from the Ring by the Difference of Colour.

The duſky Line, which in 1720 he obſerved to accompany the inner Edge of the Ring croſs the Diſk, continues cloſe to the ſame, though the Breadth of the Ellipſe is conſiderably encreaſed ſince that Time.

XXX. *A Conjunction of* Saturn *with the Moon, obſerved at* Pekin, *Dec.* 6. N. S. 1728. N°. 416. *p.* 456.

Dec. die 6. veſperi, Conjunctio *Saturni* cum Luna, ſed Luna non niſi poſt 7h¼ è nubibus promicante, Captæ ſunt tantum ſequentes diſtantiæ Saturni à propiore limbo Lunæ cujus diameter 30′ 45″.

Hora 7	18″	dist.	17′	55″	♄ in recta ex cuſpide bor. ☽ per	Fracaſtorium.
	25		20	30		Iſidorum.
	33		23	0		Santbecium.
	40		25	10		Petavium.

1724.

XXXI.
Obſervat. of ♄ *at* Southwick *in* Northamptonſhire, *by* G. Lynn, *Eſq*; Nº 373. *p.* 67.

1724. *June 23d*, 10^h 13′ *Saturn* followed a Star (in *Senex*'s *Zodiack*, but without any diſtinguiſhing Mark) 51″ and an half of Right Aſcenſion in Time, and declin'd from it South 40″.

June 25th, 10^h 0′ *Saturn* followed the ſame Star, 13″ of Right Aſcenſion in Time, and declined from it South, 3″ or 4″ only.

XXXII.
A Conjecture concerning the nature and manner of forming Saturn's Ring, *the Appearance and Diſappearance of ſome fixed Stars, by Mr.* Pet. Lewis de Maupertuis. Nº. 422. *p.* 254.

Hæc conſideratio formarum quas pro diverſa gravitatis ad vim centrifugam ratione, fluida induere poſſunt, me induxit ut cogitarem tales planetarum formas forſitan in cœlis reperiri, cum ad hoc celeriori tantum circa axem motu, vel minori materiæ denſitate opus ſit. Etenim quamvis pauci quos novimus planetæ ſatis ad ſphæroidicam formam accedant, cur non alij aliarum formarum ſupra dictarum admitterentur vel circa alios ſoles, vel etiam circa noſtrum? Hi planetæ lentiformes, vel propter diſtantiam, a nobis nunquam conſpicerentur, vel quia in plano Eclipticæ verſarentur, aut in plano parum ad Eclipticam inclinato, cui plano illorum axis revolutionis eſſet rectus, aut fere rectus; nam in hoc ſitu è terra conſpici nequirent.

Cur etiam talis formarum varietas inter fixas locum non haberet? Præſertim cum illas circa axem gyrari, ſolis inſtar noſtri, ſit admodum veriſimile. Forſitan fixæ lentiformes in cœlis dantur. Forſitan planetis admodum excentricis vel cometis cinguntur, qui cum in plano æquatoris fixæ non verſentur, quando ad perihelium accedunt, directionem axis ſtellæ turbant; & tunc quæ nobis propter ſitum non apparet, apparebat ſtella, vel quæ apparebat non apparet. *Et ſic ratio redderetur cur quædam ſtellæ per vices accendi & extingui videntur.*

Sed ſi in quovis ſyſtemate cometa aliquis caudam trahens, fertur in viciniam alicujus potentis planetæ, quid eventurum? Materia quæ à corpore cometæ effluit, circa planetam trahetur; & cometa novam materiam effundente, vel ſufficiente materiæ jam effuſæ copia, orietur fluxus circa planetam continuus: Et quamvis columnæ fluenti vel cylindrica, vel conica, vel quælibet alia forma primùm fuerit, vis ejus centrifuga cum gravitatibus tum a planeta tum a materia fluenti ortis, ſemper eam latiorem & tenuiorem reddet; & columna hæc curvata ad aliquam è formis ſupra definitis in Probl. 2º * accedet. *Et ſic omnium naturæ phænomenorum maxime ſtupendi, Saturni annuli ratio redderetur.*

Interea dum cometæ cauda talem planetæ annulum daret, corpus ipſum cometæ forſan etiam traheretur ſi in diſtantia debita eſſet, & novus planetæ ſatelles fieret. Sic forſan plures cometæ ſatellitibus & annulo Saturnum ditarunt: nam annulum Saturni unius cometæ effluvio tribuendum non videtur, cum umbram in Saturni diſcum projiciat dum materia tamen caudarum cometarum adeo ſit rara ut trans illam lucentes ſtellæ videri queant. *Annulus ergo Saturni ex plurium cometarum caudis conſtare videtur, & quarum materia propter attractionem Saturni denſior facta eſt.*

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* *Vide auctoris diſſertationem in capite ſequente.*

Patet planetam ſatellites, nec tamen annulum acquirere poſſe; nam non omnes cometæ caudam habent: Et ſi cometa caudâ carens trahatur, planetæ ſatellitem ſine annulo dabit.

Summus Newton ſtatuit vapores cometarum in planetas ſpargi: imò etiam hanc communicationem neceſſariam duxit, ut quidquid liquoris conſumitur, reparetur. Viri illuſtriſſimi D. D. Halley & Whiſton, cometas & cometarum caudas planetis infeſtas mutationes, ut polorum variationem, diluvia, & incendia inferre poſſe crediderunt; ſed cometæ benigniores effectus producere poſſunt, & etiam planetis aliquando res miras & utiles dare.

1725.

XXXIII. *An Obſervation of* Jupiter *at* Southwick *in* Northamptonſhire, *by* G. Lynn, *Eſq;* N°. 393. *p.* 67.

1725 *Dec.* 17th at $8^h\ 0'\ 00''$ apparent Time *Jupiter* preceded φ *Aquarii* $4''$ and an half of right Aſcenſion, in time, and declined from it *South*, $11'\ 45''$. This Obſervation (with the reſt at *Southwick*) was made with a thirteen Foot Teleſcope, whoſe aperture was 2. 4 Inches, and Charge 2. 5 Inches.

XXXIV. 1] *Eclipſes of* Jupiter's *firſt* Satellite *obſerved at* Harvard *College in* New Engl. *communicated by the Reverend Dr.* Derham. N° 423. *p.* 270.

$171\frac{16}{17}$.

	H.	′	″	
At	10	48	17	*Feb.* 13. $17\frac{16}{17}$. Mr. *Robie* obſerved an Immerſion of the firſt *Satellite* of *Jupiter*.
At	10	7	30	*Feb.* 8th. I obſerved an Emerſion according to which the difference of Longitude between *Harvard* College and *Upminſter* is $4^h\ 45''$.

$171\frac{7}{8}$.

At	10	45	35	*Mar.* 10th. $171\frac{7}{8}$. Mr. *Robie* obſerved an Emerſion of the firſt Circumjovial.

$171\frac{8}{9}$.

At	10	35	00	P. M. *Jan.* 13th. $171\frac{8}{9}$. the firſt Circumjovial immerged.

Eclipſes

Obſervations of the Eclipſes *of* Jupiter's Satellites, *from* 1700, *to the Year* 1727. *By the Reverend* W. Derham, *M. A. Canon of* Windſor, *and F. R. S.*

Ecl. of Jup. Satelli from 1700 to 1727. N° 402. p. 415.

2] *Eclipſes Primi Satellitis.*

I. Sat. Dies Menſis.	Tempus æquale.	Tempus apparens.	Per Tab. Flamſt. & Caſſini.	Qualis Eclipſis.	Locus Jovis Helioc.	
	H. M S.	H. M. S.	Min. Sec.		Grad.	
		Anno Domini 1700.				
Aug. 13	10. 59. 4	10. 57. 10	59. Fl.	Em.	♒ 15	Teleſcopio 6 pedali.
Dec. 1	— — —	5. 1. 8 — 1. 38	4h. 55′½ C. 4. 58 Fl.	E.	♒ 10	Teleſcopio 16 pedali. Omnes ſequentes Eclipſes Tubo 16 pedali obſervatæ fuere, niſi cum aliter notatur.
		Anno Domini 1701.				
Jun. 15	— — —	13. 23. 0 — 24. 50	13. 21 Fl. 13. 26 C.	Im.	♒ 27	
Jul. 8.	— — —	13. 30. 0	13. 28 Fl. 13. 34 C.	I.	♒ 29	
Oct. 12.	— — —	5. 54. 9 — 54. 19 — 54. 49	5. 59 C.	E.	♓ 8	Bona Obſervatio.
— —19.	— — —	7. 48. 57 — 49. 47	7. 55 C.	E.	♓ 9	Bona.
Dec. 20.	— — —	6. 25. 0	6. 28 Fl.	E.	♓ 14	Dubia.
		Anno Domini 1702.				
Oct. 15	— — —	9. 22. 0 — 22. 15 — 22. 45	9. 23 Fl. 9. 26 C.	E.	♈ 11½	Optima.
— 24	— — —	5. 44. 57 — 45. 21 — 45. 42	5. 47 Fl. 5. 50 C.	E.	♈ 12	Optima.
Dec. 9	— — —	5. 59. 0	6. 5 Fl.	E.	♈ 16½	Aer nebuloſus.

I. Sat. Dies Menſis.	Tempus æquale. H. M. S.	Tempus apparens. H. M. S.	Per Tab. Flamſt. & Caſſini. H. Min.	Eclipſis qualis.	Locus Jovis. Grad.	
		Anno Domini 1703.				
Aug. 8	— — —	15. 21. 30	15. 18 F.	I.	♉ 8½	Dubia ob Nebulas.
—— 24	— — —	13. 43. 6 — 43. 35	13. 38 F. 13. 45 C.	I.	♉ 10	Bona.
Sept. 2	— — —	10. 8. 20	10. 4 F	I.	♉ 11	Nebuloſum Cœlum.
Nov. 28	5. 37. 40	5. 44. 52	5. 43. F.	E.	♉ 19	Non mala.
		Anno Domini 1704.				
Aug. 26	17. 7. 43	17. 9. 53	17. 8 F.	I.	♊ 12¼	Nimia lux, ſed non mala.
— 28	11. 36. 16	11. 38. 18 — 39. 3	11. 37 F.	I.	♊ 13	Bona.
Sept. 4	13. 29. 14	13. 33. 5 — 34. 25	13. 23. F.	I.	♊ 13½	Bona.
Oct. 6	10. 2. 40	10. 16. 40 — 17. 10	10. 13 F.	I.	♊ 16	Nebuloſa, ſed non mala.
Nov. 3	17. 39. 54	17. 54. 55	17. 48 F.	I.	♊ 18¼	Nebuloſum.
Dec. 9		5. 31. 00		E.	♊ 21¼	Dubia.
—— 23	9. 14. 43	9. 9. 17	9. 10 C.	E.	♊ 23	Bona.
		Anno Domini 1705.				
Mart. 2	— — —	9. 46. 3 — 46. 40 — 47. 0	9. 47 F.	E.	♊ 29	Bona.
—— 25	10. 9. 58 10. 11. 5	10. 7. 18 — 7. 52 — 8. 25	10. 10 F.	E.	♋ 0½	Bona.
Sept. 7	16. 39. 15 — 40. 15	16. 45. 26 — 46. 26	16. 52 F.	I.	♋ 15	Bona.
Oct. 30	18. 54. 50 — 55. 20	19. 10. 19 — 10. 49	19. 10 C.	I.	♋ 19	Nimia lux, ideo dubia.
Nov. 22	19. 4. 50	19. 13. 59		I.	♋ 21	
Dec. 15	19. 15. 0	19. 13. 20	19. 11 F.	I.	♋ 22¼	Dominus Gray hanc immerſionem Cantuariæ 19h. 15′ obſervavit.
		Anno Domini 1706.				
Mart. 7	— — —	7. 27. 0	7. 26 F.	E.	♋ 29½	Emerſum inveni.
—— 30	7. 46. 40 — 47. 37	7. 45. 25 — 46. 20	7. 48 F.	E.	♌ 3	Bona.
Apr. 29	9. 59. 2	10. 3. 5	10. 3. F.	E.	♌ 3¼	Nebuloſum.

I. Sat. Dies Menſis.	Tempus æquale.	Tempus apparens.	Per Tab. Flamſteed. & Caſſini.	Qualis Eclipſis.	Locus Jovis.	
	H. M. S.	H. M. S	H. Min.		Grad.	
		Anno Domini 1707.				
Feb. 15	10. 17. 52	10. 4. 43	10. 3 F.	E.	♌ 26¾	
— — 24	6. 39. 52 — 41. 32	6. 28. 20 — 30. 0	6. 28 F.	E.	♌ 27¼	Bona.
Mar. 26	8. 47. 53 — 48. 6	8. 45. 36 — 45. 49	8. 46 F.	E.	♌ 29¾	Ventus fortis tubum motitavit.
Maii 11	9. 14. 43 — 16. 7	9. 18. 43 — 20. 7	9. 20 F.	E.	♍ 3¼	Bona.
		Anno Domini 1708.				
Jan. 31		17. 35. 0		I.	♍ 23½	Immerſus fuit ante.
		Anno Domini 1709.				
Maii 18	9. 7. 26 — 8. 26	9. 10. 47 — 11. 47	9. 18 F. 8. 59 C.	E.	♎ 1¾	Non mala.
Jun. 10	9. 22. 21 — 24. 0	9. 21. 9 — 23. 8	9. 27 F. 9. 12 C.	E.	♎ 3½	Bona.
		Anno Domini 1710.				
Maii 14	10. 6. 57	10. 10. 26	10. 14 F.	E.	♏ 26	
Jul. 15	— — —	8. 46. 0	8. 49 F.	E.	♐ 2	Immerſum vidi.
Aug. 23	7. 22. 2	7. 22. 50	7. 28 C.	E.	♐ 5	Non mala.
		Anno Domini 1711.				
Aug. 19	— — —	8. 23. 40 — 24. 30 — 25. 00	8. 32 F. 8. 30 C.	E.	♑ 4	Bona.
		Anno Domini 1713.				
Oct. 27	8. 10. 28	8. 26. 19	8. 35 F.	E.	♓ 14	Bona.
Dec. 28	7. 4. 18	6. 56. 30	7. 5 F. 7. 1 C.	E.	♓ 19¼	Bona.
		Anno Domini 1714.				
Oct. 23	9. 59. 0	10. 1. 1	10. 5 F.	E.	♈ 17	Dubia.
Nov. 1	— — —	6. 20. 0	6. 20. F.	E.	♈ 18	Nubilum ideo dubia licet tubo 34 pedali obſervationem feci.

I. Sat. Dies Menſis.	Tempus æquale.	Tempus apparens.	Per Tab. Flamſteed. & Caſſini.	Qualis Eclipſis.	Locus Jovis.	
	H. M. S.	H. M. S.	H. M.		Grad	
		Anno Domini 1717.				
Feb. 1	6. 25. 3 — 26. 18 — 26. 48	6. 10. 15 — 11. 30 — 12. 00	6. 14 F. 6. 14 C.	E.	♋ 1	Bona.
— — 8	8. 21. 21 — 21. 51 — 22. 21	8. 7. 0 — 7. 30 — 8. 0	8. 9 F. 8 12 C.	E.	♋ 1½	Bona.
		Anno Domini 1725.				
			Per Tabul. D. Bradley.			Sequentes Obſervationes Teleſcopio 12½ pedali optimo factæ fuere.
Nov. 27	— — —	9. 8. 36 — 9 0	9. 6. B.		♓ 22	Windeſoriæ, dubia.
		Anno Domini 1726.				
Januar. 5	7. 40. 19 — 41. 0 — 41. 30	7. 29. 49 — 30. 0 — 31. 0	7. 31 B.	E.	♓ 25	Bona, Upminſtri.
Aug. 5	14. 50. 17 — 51. 7	14. 46. 28 — 47. 18	14. 47 B.	I.	♈ 15	Bona.
Sept. 15	7. 44. 17 — 45. 37 — 46. 7	7. 53. 10 — 54. 30 — 55. 0	7. 53 B.	I.	♈ 19	Bona.
Octob. 8	— — —	10. 25. 0	10. 7 B.	E.	♈ 21	Obſervatio incerta propter Jovis vicinitatem.
— — 17	— — —	6. 46. 30	6. 46 B.	E.	♈ 21½	
Dec. 2	— — —	7. 2. 0 — 3. 0	7. 5 B.	E.	♈ 25	Windeſoriæ, dubia.
— — 25	7. 12. 57 — 13. 57	7. 7. 0 — 8. 0	7. 9 B.	E.	♈ 28	Upminſtri, bona.

Obſervationes Eclipſium Secundi Satellitis Jovis.

II. Sat.		Anno Domini 1700.				
Oct. 27	— — —	8. 24. 0	8. 23	E.	♒ 6¼	Sequentes Obſervationes Teleſcop. 16 pedali factæ fuere. Dubia propter vapores. D. Flamſteedii Miniſter obſervavit circa 8. 16′ p. m.

II. Sat. Dies Menſis.	Tempus æquale.	Tempus apparens.	Per Tab. Flamſteed.	Qualis Eclipſis.	Locus Jovis Helioc.	
	H. M. S.	H. M. S.	H. M.		Grad.	
		Anno Domini 1701.				
Jun. 29	— — —	10. 50. 0	10. 52	I.	♒ 28½	Dubia ob vapores.
Jul. 31	Inter	9. 43. 0 10. 3. 0	10. 33	I.	♓ 1	Nubiloſum.
Oct. 21	— — —	7. 39. 35 — 40. 0 — 41. 0	7. 51	E.	♓ 8¼	Bona.
— — 28	— — —	10. 18. 2 — 20. 0	10. 29	E.	♓ 9	Bona.
Nov. 22	— — —	7. 26. 18 — 27. 0	7. 34	E.	♓ 11½	Bona.
		Anno Domini 1702.				
Aug. 26	— — —	9. 46. 0	10. 17	I.		Immerſum inveni.
Sept. 9	— — —	15. 0. 51 — 1. 51	15. 35	I.	♈ 8	Bona.
Oct. 15	— — —	7. 5. 21 — 6. 22 — 7. 22	7. 30	E.	♈ 11½	Bona.
— — 22	— — —	9. 40. 38 — 41. 0 — 42 13	10. 8	I.	♈ 12	Bona.
		Anno Domini 1703				
Aug. 20	— — —	9. 50. 0	10. 11	I.	♉ 9¼	Immerſum inveni.
Oct. 5	— — —	15. 3. 0	15. 19	I.	♉ 24	Immerſus ante.
Dec. 19	8. 41. 9	8. 38. 7	9. 6	E.	♉ 20½	Bona.
		Anno Domini 1704.				
Aug. 20	12. 15. 29	12. 15. 34	12. 32	I.	♊ 12	Bona.
Oct. 5	— — —	17. 10. 44 — 11. 14	17. 32	I.	♊ 16	Bona.
— — 16	— — —	9. 2. 19	9. 32	I.	♊ 10	Haud mala.

II Sat. Dies Menſis.	Tempus æquale.	Tempus apparens.	Calculatio Flamſteed.	Qualis Eclipſis	Locus Jovis Helioc.	
	H. M. S.	H. M. S.	H. M.		Grad.	
		Anno Domini 1705.				
Jan. 12	— — —	7. 47. 0	8. 14	E.	♊ 24¾	Emerſit ante.
— — 20	10. 30. 23 — 31. 14	10. 16. 9 — 17. 0	10. 50	E.	♊ 25¼	Bona.
Feb. 14	7. 45. 31	7. 32. 0	8. 1	E.	♊ 27½	Vaporoſus aer: dub.
Mar. 25	10. 15. 10 — 16. 5	10. 12. 30 — 13. 25	10. 36	E.	♋ 0	Incerta propter vicinitatem primi Sat.
Apr. 26	— — —	10. 4. 0	10. 29	E.	♋ 3¾	Vaporoſus Horiz. dub.
Sept. 29	16. 26. 11 — 26. 26	16. 39. 11 — 39. 26	16. 48	I.	♋ 16½	Bona.
Oct. 31	15. 55. 20 — 56. 26	16. 10. 52 — 11. 49	16. 27	I.	♋ 19	Bona.
Dec. 20	9. 49. 30 — 50. 10	9. 45. 36 — 46. 16	10. 11	I.	♋ 23	Bona.
		Anno Domini 1706.				
Apr. 20	— — —	8. 58. 30	9. 31	E	♌ 3	Fuerat emerſus.
		Anno Domini 1707.				
Mar. 13	— — —	7. 59. 0	8. 18	E.	♌ 28¼	Emerſit ante.
— — 20	10. 28. 14	10. 23. 94	11. 5	E.	♌ 29	Dubia.
Apr. 14	7. 33. 17	7. 35. 39	8. 9	E.	♍ 0½	Dub. propt nim. lucem.
— — 21	10. 9. 33 — 10. 59	10. 12. 51 — 14. 17	10. 45	E.	♍ 1½	Bona.
		Anno Domini 1710.				
Mar. 4	— — —	17. 5. 0	— — —	I.	♍ 21½	Nubilum, & incerta.
		Anno Domini 1711.				
Jul. 15	— — —	9 16. 0	9. 6	E.	♑ 1½	Nebuloſum & dub.
		Anno Domini 1712.				
Oct. 12	7. 35. 6 — — —	7. 50. 30 — 51. 15 — 52. 30	7. 58	E.	♒ 10	Bona.
		Anno Domini 1714.				
Nov. 1	— — —	6. 34. 7	6. 55	E.	♈ 18	Incerta ob nubes.
Dec. 3	— — —	6. 14. 26 — 15. 0	6. 34	E.	♈ 21½	Bona.
		Anno Domini 1716.				
Dec. 22	— — —	4. 55. 0	— — —	E.	♊ 27½	Emerſio ante fuit.

II. Sat. Dies Menſis.	Tempus æquale.	Tempus apparens.	Calculatio Flamſt. & Molyn.	Qualis Eclipſis.	Locus Jovis Helioc.	
	H. M S	H. M. S	H. M.		Grad.	
		Anno Domini 1717.				
Jan. 30	— — —	7. 24. 0	7. 46.	E.	♋ 1	Emerſum inveni.
		Anno Domini 1725.				
Oct. 30	— — —	9. 16. 23 — 17. 0 — 17. 18	8. 46 M.	E.	♓ 19	Bona obſervatio Teleſcopio $12\frac{1}{2}$ pedali opt.
		Anno Domini 1726.				
Aug. 18	{ 8. 52. 25 — 53. 55	8. 54 52 — 56. 22	8. 58 M.	I.	♈ 17	Bona.
Oct. 17	— — —	6. 12. 0	5. 53 M.	E.	♈ $21\frac{1}{2}$	Dubia.
Nov. 25	— — —	{ 8. 27. 15 — 28. 0	8. 16	E.	♈ 15	Non mala, Windeſoriæ.

Obſervationes Eclipſium Tertii Satellitis Jovis.

III.Sat.						
		Anno Domini 1700.				
Oct. 19	— — —	6. 23. 15	6. 1	I.	♒ 6	
		Anno Domini 1701.				
Jul. 11	— — —	10. 18. 0	10. 11	I.	— —	Dubia.
Sep. 28	— — —	{ 10. 25. 27 — 26. 0 — 26. 42	10. 12	E.	♓ $6\frac{1}{2}$	Bona.
		Anno Domini 1702.				
Jul. 26	— — —	10. 51. 13	10. 33	E.	♈ 4	Dubia.
Aug. 2	Inter	{ 11. 55. 0 12. 5. 0	11. 35	I.	♈ 5	Nubes interrumpebant.

III. Sat.

III. Sat. Dies Menſis.	Tempus æquale.	Tempus apparens.	Calculatio Flamſteedii.	Qualis Eclipſis	Locus Jovis Helioc.	
	H. M. S.	H. M. S.	H. M.		Grad.	
		Anno Domini 1703.				
Jul. 12	— — —	14. 46. 0	15. 32	I.	♉ 6	Fuit Immerſus.
Aug. 24	— — —	13. 43. 20 — 44. 20 — 44. 51 16. 6. 36	13. 4 15. 40	I. E.	♉ 10	Optima obſervatio Immerſionis: at ſomno invadente, nimis ſerò Emerſionem vidi.
Sept. 22	— — —	8. 9. 53	7. 50	E.	♉ 12¾	
— — 29	— — —	9. 57. 44 — 58. 5 12. 8. 17	9. 17 11. 52	I. E.	♉ 13	Immerſio accuratè viſa, ſed Emerſio dubia propter vicinitatem Jovis.
Oct. 6	13. 46. 13 — 47. 33 — 48. 33	14. 0. 30 — 1. 50 — 2. 50	13. 19	I.	♉ 14	Bona.

In Obſervationibus Aug. 24. Sep. 22, 29, & Oct. 6. notandum eſt, Latitudinem tertii Satellitis majorem fuiſſe quam FLAMSTEEDIUS, aut CASSINUS conjectârunt. Nam iſte Satelles uſq; ad extremum Poli Jovis marginem evagatus eſt, & (antequam in ejus umbram prorſus immerſus eſt) diu in ejus penumbrâ latuit: & in eadem umbrâ non ultra duas horas permanſiſſe autumo, quamvis poſt Obſervationes Aug. 24. & Sept. 29, moram diuturniorem fuiſſe videatur. Sed in priore obſervatione, veram non vidi Emerſionem: & in poſteriore (cum ✱ ⊙ ♃ fuerit) Emerſio Jovis limbo tam propinqua fuit, ut difficile fuerit eam cum Teleſcopio 16 pedali verè obſervare.

		Anno Domini 1704.				
Sep. 14	Inter	16. 58. 0 17. 10. 0	16. 32	E.	♊ 14¼	Nubilum cœlum.
Oct. 20	10. 14. 34	10. 28. 40 — 30. 33	9. 48	I.	♊ 17	Bona.
Nov. 3	18. 12. 16	18. 27. 17	17. 45	I.	♊ 18¾	Bona, licet aer nebuloſ.
		Anno Domini 1705.				
Jan. 14	10. 7. 5 — 9. 4	9. 53. 57 — 55. 56	— — —	I.	♊ 24¾	Bona.
Feb. 26	— — — 10. 9. 32	9. 58. 7 — 58. 42	— — —	I.	♊ 28½	Bona.
Oct. 6	Inter	17. 18. 0 — 27. 0	— — —	E.	♋ 17	
Nov. 25	— — —	7. 41. 0	— — —	I.	♋ 21	Incerta per Nebulas denſas.

III Sat. Dies Menſis.	Tempus æquale.	Tempus apparens.	Calculatio Flamſteedii.	Qualis Eclipſis.	Locus Jovis Helioc.	
	H. M. S.	H. M. S.	H. M.		Grad.	
		Anno Domini 1706.				
Mar. 13	— — —	8. 52. 27 — 54. 0	7. 36	E.	♌ 0	Bona.
— — 20	— — —	9. 24. 0	9. 3	I.	♌ 0½	Dubia ob ♃ vicinitatem
Nov. 4	— — —	16. 59. 27	16. 50	I.	♌ 0¾	Non mala.
		Anno Domini 1707.				
Maii 17	— — —	8. 17. 0	— — —	E.	♍ 3¼	Emerſum inveni.
Nov. 26	18. 41. 37	18. 49. 15	18. 41	E.	♍ 18½	
Dec. 3	19. 19. 38 — 20. 46	19. 24. 1 — 25. 9	19. 25	I.	♍ 19	Bona.
		Anno Domini 1709.				
Feb. 5	17. 35. 32 — 37. 4	17. 21. 0 — 22. 0	17. 26	I.	♎ 21½	
		~~Anno Domini 1711.~~				
Jul. 14	— — —	9. 45. 0	9. 41	E.	♒ 2	Dubia.
		Anno Domini 1712.				
Sept. 16	— — —	6. 15. 0 — 20. 0 9. 50. 3 — 52. 0 — 52. 30	6. 28 10. 5	I. E.	♒ 7¼	Dubia ob nimiam lucem. Bona.
Oct. 29	— — —	6. 26. 30 — 28. 0	6. 45	I.	♒ 11½	Bona.
		Anno Domini 1713.				
Dec. 26	6. 38. 30	6. 32. 0	6. 46	E.	♓ 19½	Ventus dubiam reddidit.
		Anno Domini 1714.				
Sep. 17	8. 26. 56 — 28. 56	8. 36. 9 — 38. 9	8. 40	I.	♈ 13¼	Bona, Tubo 34 pedali.
Oct. 23	7. 22. 0 — 23. 0 — 25. 0	7. 24. 0 — 25. 0 — 27. 0	7. 38	E.	♈ 17	Bona, Tubo 16 pedali.
		Anno Domini 1716.				
Dec. 5	— — —	— — —	7. 37	E.	— —	Hic Satelles magnam habet Latitudinem, & ni fallor, nullam paſſa eſt Eclipſin hâc nocte.

III. Sat. Dies Menſis.	Tempus æquale.	Tempus apparens.	Calculatio Flamſteedii.	Qualis Eclipſis	Locus Jovis Helioc.	
	H. M. S.	H. M. S.	H. M.		Grad.	
		Anno Domini 1717.				
Jan. 17	— — —	5. 52. 23 — 53. 45 8. 48. 38	— — —	I. E.	♋ 5 0	Bona. Mala.
		Anno Domini 1726.				
Jan. 5	6. 41. 10 — 42. 30	6. 30. 40 — 32. 0	— — —	E.	♓ 25¼	Bona, Teleſcop. 12½ pedali.
Dec. 15	7. 13. 29 — 14. 42	7. 12. 17 — 13. 30	7. 21 M.	E.	♈ 15¾	Bona.

Obſervationes Eclipſium Quarti Satellitis.

		Anno Domini 1701.				
Jun. 11	— — —	14. 20. 0	14. 35	I.	♒ 27	Immerſum inveni.
Sep. 3	— — —	13. 59. 0	14. 21	E.	♓ 4¼	Idem per nebulas.
		Anno Domini 1704.				
Sep. 30	— — —	9. 21. 42	9. 40	E.	♊ 15¼	Nebuloſum, ideo dubia.
Dec. 6	— — —	9. 52. 44	— — —	E.	♊ 21½	Dubia ob proximitatem Jovis, & parvitatem Satel.
		Anno Domini 1705.				
Feb. 11	8. 38. 5	8. 24. 8	8. 23	I.	♊ 27	Non mala.
		Anno Domini 1706.				
Mar. 20	9. 3. 2	8. 58. 40	9. 36	I.	♌ 0¼	Non mala.
Sep. 20	— — —	16. 24. 0	16. 17	I.	♌ 15	Immerſum inveni.
		Anno Domini 1712.				
Aug. 20	Inter.	8. 29. 0 — 39. 0	10. 11	I.	♒ 5	
Sept. 6		7. 43. 0 — 44. 0 — 45. 0	9. 20	E.	♒ 7	Bona.

Remarks on the foregoing TABLES.

As exact Tables to calculate the *Eclipſes* of the *Circumjovials*, would be of very great Service to find the *Longitude of Places*; ſo I have ſome Hopes that theſe Obſervations of ſome of them, in more Revolutions than one of *Jupiter* in his Orb, may be of Uſe to correct, or make ſuch Tables.

The greateſt Chaſms in them were cauſed by ſome dangerous Fits of Sickneſs, which ſo impaired me, that I have not dared, ever ſince, to venture upon Obſervations at unſeaſonable Hours of the Night.

As to my Manner of obſerving, it was for the moſt Part with a 16 Foot Teleſcope, and afterwards with an excellent one not inferior to it, of 12½ Feet, that, at *Jupiter*'s Light, bears an Aperture of 2½ Inches, and a Charge of about 2 Inches.

And as to the *Time*; I made uſe of an excellent and well-adjuſted *Clock*, corrected at Noon, by the Meridional Tranſits of the Sun, obſerved with the *Inſtrument* deſcribed in the *Philoſ. Tranſact.* N° 291, which ſhews the Noon-time to one or two Seconds. This Way ſome of my ſkilful Friends ſuſpected to be fallacious, and not comparable to taking the Time by Altitudes of the Sun, or fix'd Stars. For a Trial therefore, we obſerved ſome Eclipſes that were agreed on; which when compared, we found ſo nicely to agree, as to ſhew to a Second of Time, or very nearly ſo, the Difference of the Meridian of the *Obſervatory*, and that of *Upminſter*.

The greateſt Part of the Eclipſes, that were the moſt accurately made, may eaſily be diſtinguiſhed by the two, or more Numbers of the Time of Obſervation: The firſt of which ſhews the Moment of the Beginning of the Eclipſe; the following, the Times when farther advanced: As in an Emerſion, the firſt Number ſhews the Time, when the Satellite appears like a ſmall obſcure Spot; the following Numbers, when brighter, or quite emerged out of *Jupiter*'s Shadow; and ſo contrariwiſe in an Immerſion.

For greater Certainty and Satisfaction, I have noted which Obſervations were good, which doubtful, or bad: Even the latter of which may be of Uſe in ſome Caſes, where better are wanting.

The calculated Times of the Eclipſes I have inſerted, where I had them from others, or could calculate them my ſelf, as being of good Uſe to amend the Tables of Mr. *Flamſteed*, *Caſſini*, or others, taken Notice of in the Column on Purpoſe. And for the ſame Reaſon I thought good to add the Place of *Jupiter* alſo.

And laſtly, I mention the Length and Power of the Teleſcope I uſed; becauſe Obſervations may differ ſeveral Seconds, by the different Length and Goodneſs of the Teleſcope uſed; a long and good Teleſcope ſhewing the Satellite, when the Shadow of *Jupiter* doth but juſt touch it: Whereas a ſhort, or bad one, doth not ſhew it, until one Half, or more, of the Satellite is enlightened. Which Difference is moſt remarkable in the Eclipſes of the two outermoſt Satellites, in their greateſt Latitudes; at which Times they go into, and come out of *Jupiter*'s Shadow, in an oblique and longer, not a direct and ſhorter Path: An Inſtance of which may be ſeen in the Obſervations of the Eclipſes of the Third Satellite in the Months of *Auguſt* and *September*, 1703.

3] *Eclipſes of* Jupiter's *Satellites, from* 1721 *to* 1729. *by Sign.* Bianchini *and others.* No 407. *p.* 35.

Days of the Month.		Time of Obſervation.			Satel. Eclip.		Place where obſerved.
		Anno Dom. 1721.					
		H.	'	"			
Apr.	3	15	4	32	Im.	1	At *Rome.*
Jun.	21	8	46	0	Em.	1	*Rome.*
		Anno Dom. 1722.					
Jun.	9	13	20	0	E.	1	*Rome.*
——	18	9	36	30	E.	1	At *Albano.*
Jul.	11	9	49	10	E.	1	*Rome.*
——	27	8	7	30	E.	1	*Rome.*
Aug.	19	8	26	20	E.	1	*Rome.*
		Anno Dom. 1723.					
Mar.	26	17	14	50	I.	1	*Rome.*
Apr.	11	15	31	45	I.	1	*Rome.*
May	3	15	48	51	I.	1	*Rome.*
— — —		15	43	0			At *Ingolſtad* by F. *Grammatici.*
— —	27	18	56	0	I.	1	*Rome.*
Jun.	5	12	16	30	I.	1	*Rome.*
— —	12	14	11	39	I.	1	*Rome.*
Jul.	23	9	11	40	E.	1	*Rome.*
		7	46	0			*Lisbon*, by F. *Carbone.*
— —	30	11	7	20	E.	1	*Rome.*
Aug.	8	7	32	0	E.	1	At *Ocritoli* in *Viâ Flaminiâ.*
— —	15	9	35	0	E.	1	At *Aſſiſi* in *Ombria.*
Sep.	7	9	50	45	E.	1	*Urbino.*
		8	21	48			*Lisbon.*
— —	23	8	17	54	E.	1	At *Muceria* in *Ombria.*
Oct.	16	8	36	10	E.	1	At *Albano* in the *Via Appia.*
		Anno Dom. 1724.					
		H.	'	"			
Jun.	8	14	3	28	I.	1	*Carbone* at *Lisbon.*
— —	15	15	56	27			
— —	23	13	42	50			*Rome.*
— —	30	15	34	29	I.	1	*Rome.*
		14	8	55			*Lisbon.*
Aug.	10	10	45	20	E.	1	*Rome*, but doubtful.
— —	17	12	40	45	E.	1	*Rome.*
— —	26	9	6	45	E.	1	*Rome.*
Sep.	11	7	30	53	E.	1	*Rome.*
— —	18	9	28	16	E.	1	*Rome.*
— —	25	11	25	55	E.	1	*Rome.*
		9	59	21			*Lisbon.*
Oct.	11	9	53	8	E.	1	*Albano.*
— —	14	9	31	0	E.	3	From the *Limb* of ♃.
		11	7	0	I.	3	into ♃ Shadow. *Albano.*
		—	9	30			
— —	27	8	16	0	E.	1	*Albano.*
Nov.	12	5	33	10	E.	1	*Rome.*
— —	19	8	25	5	E.	1	*Rome.*
— —	30	6	14	0	I.	1	At *Pekin* in *China*, by F. *Koegler* the *Jeſuit.*
Dec.	5	6	42	25	E.	1	*Rome.*
		Anno Dom. 1725.					
Jun.	19	15	17	10	I.	1	*Rome.*
July	5	13	32	20	I.	1	*Albano.*
— —	7	14	55	30	I.	1	*Pekin.*
— —	21	11	45	22	I.	1	*Rome.*
		10	39	35			Mr. *Molyneux* near *London.*
— —	28	13	39	10	I.	1	*Rome.*
		12	12	26			*Lisbon.*

Days of the Month.	Time of Obſervation.	Satel. Eclip.	Place where obſerved.
	H. ′ ″		
Nov. 15	9 53 50	E. 1	*Rome.*
	8 24 50		*Lisbon.*
— — 14	6 15 15	E. 1	*Rome.*
Dec. 17	6 20 30	E. 1	*Rome.*
	Anno Dom. 1726.		
Jul. 17	13 28 46	I. 1	*Rome.*
	13 24 45		*Ingolſtad.*
	12 1 52		*Lisbon.*
Aug. 2	11 40 0	I. 1	St. *Quirico* in *Tuſcany.*
	11 41 20		*Ingolſtad.*
— — 9	13 36 0	I. 1	*Siena* in *Tuſcany.*
	12 13 30		*Lisbon.*
— — 16	15 28 29	I. 1	*Florence.*
	14 8 46		*Lisbon.*
	15 29 0		*Bologne.*
Aug. 25	11 54 24	I. 1	*Bian.* At
	11 54 26		*Man.* *Bol*
	11 56 18		*Ingolſtad.*
	11 19 55		*Paris.*
	10 32 57		*Lisbon.*
Sept. 26	8 41 0	I. 1	St. *Quirico.*
	8 39 20		*Ingolſtad.*
	8 3 20		*Paris.*
Oct. 1	16 7 45	I. 1	St. *Quirico.*
Nov. 20	7 46 30	E. 1	*Rome.*
	6 20 19		*Lisbon.*
— — 27	9 39 25	E. 1	*Rome.*
Dec. 6	6 0 16	E. 1	*Rome.*
	5 58 0		*Bologne.*
	5 24 0		*Paris.*
	Anno Dom. 1727.		
Mar. 8	6 42 50	E. 1	*Rome.*
Aug. 5	15 18 27	I. 1	*Rome.*
	15 0 8	I. 1	*Rome.*
	14 21 12		*Paris.*
Sept. 6	12 0 0	I. 1	*Rome.*
	11 55 15		*Bologne.*
	11 19 43		*Paris.*
Oct. 15	10 41 30	I. 1	*Albano.*
— — 20	6 5 54	I. 1	*Albano.*
— — 22	12 33 23	I. 1	*Albano.*

Days of the Month.	Time of Obſervation.	Satel. Eclip.	Place where obſerved.
	Anno Dom. 1728.		
Jan. 15	13 13 46	E. 1	*Rome.*
Feb. 16	9 46 56	E. 1	*Rome.*
Mar. 26	8 32 7	E. 1	*Rome.*

XXXIII. 4] *Eclipſes from* 1726, *to* 1727. *obſerved at* Bologne, *by* Euſt. Manfredi. Nº 407. *p.* 36.

* Obſervations made at the Obſervatory of *Bologne*, by Signor *Euſtachius-Manfredi.*

Days of the Month.	Time of Obſervation.	Satel. Eclip.	Place where obſerved.
	Anno Dom. 1726.		
Aug. 16	15 29 0	I. 1	Dubious.
— — 25	11 54 24	I. 1	Dubious.
Nov. 27	9 35 11	E. 1	Dubious.
Dec. 4	11 27 45	E. 1	Dubious.
— — 26	5 47 4	I. 3	Dubious.
— —	7 56 23	— —	The third began to emerge.
— —	29 59 26	E. 1	
— — 31	6 18 54	E. 2	Juſt begun.
	Anno Dom. 1727.		
Jan. 2	9 45 27	I. 3	Dubious.
	11 53 38	E. 3	
— — 5	7 51 54	E. 1	
— — 7	8 54 12	E. 2	
Feb. 7	5 50 5	I. 3	Dubious.
	7 52 54	E. 3	
— — 8	8 37 59	E. 2	Air thick.
Aug. 21	13 34 39	I. 1	
Sept. 6	11 55 17	I. 1	
— — 17	10 48 59	I. 3	
	12 40 30	E. 3	
Oct. 13	16 5 45	I. 1	
— — 22	12 29 42	I. 1	
— — 23	8 55 34	E. 3	
— — 30	11 1 9	I. 3	Dubious.
Nov. 5	9 5 15	I. 2	Dubious.
— — 30	8 44 13	E. 2	
	Anno Dom. 1728.		
Jan. 17	8 41 8	E. 3	
Feb. 16	9 43 11	E. 1	
— — 29	6 40 45	I. 3	Dubious.
	8 50 40	E. 3	

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* The Obſervations of Sig. *Bianchini* were made with a Teleſcope of *Campani*'s grinding, of 23 ½ *Roman* Palms; thoſe of *F. Carbone* by another of the ſame Make, Length and Goodneſs. The Obſervations at *Paris* were made by Monſ. *Maraldi*. They were all communicated by Sir *Thomas Dereham* at *Florence*, put in this View by the Reverend Dr. *Dereham*, who ſuppoſes there is a miſtake in the Obſervations of *Nov.* 30. 1724. that it was an Emerſion, not an Immerſion.

1723.

5] *Eclipſes of the firſt Satellite, at* Liſbon, *by* F. Carbone. Nº 385. *p.* 185. *N. S.*

	H.	′	″
1723.			
Emerſiones.			
Die 23. Julii	7	47	00
Die 7 Septemb.	8	21	48
1724.			
Immerſiones.			
Die 8 Jun. *mane*	2	03	28
Die 15	3	56	27
Die 30	2	08	51
Emerſiones.			
Die 2 Sept.	9	36	57
Die 9	11	34	26
Die 25	9	59	21
Die 4 Octob.	6	26	44
Die 18	10	21	20
Die 3 Novemb.	8	42	30

Præc. Obſer. habitæ ſunt Teleſcopio Palm. Rom. 30. Joſ. Campani.

6] *At* Rome, Albano, *&c.* *by Sig.* Bianchini. Nº 396. *p.* 176.

Fig. 106.

Fig. 107.

T. V. poſt Med. Noct.				
H.	*M.*	*S.*		1724.
2	48	30	8 Jun.	In *Caſtro Viſcardi*, ſupra Vulſinium in eodem ferè Meridiano cum Vulſiniis (vulgò *Bolſena*) Satelles ſecundus B ex umbra Jovis emerſerat, & ita configuratus viſebatur cum cæteris. *Fig.* 106.
3	0	30		Satelles tertius C ſubit limbum Jovis è directo faſciæ mediæ.
3	26	20		Satellitis intimi A incipit lumen imminui.
3	27	10		Totalis ejuſdem immerſio, reliquis Satellitibus B, D, & faſciis perſpicuè apparentibus.
			24 Jun.	*Romæ*, obſervata eſt tum immerſio ſecundi Satellitis, tum primi, ita configuratis Jovialibus. *Fig.* 107.
1	39	0		Satellitis ſecundi B incipit lumen debilitari.
1	40	20		Omnimoda ejuſdem immerſio.
1	41	50		Etiam Satellitis primi A lumen imminui jam incipit.
1	42	50		Lux ejuſdem omninò diſparet, cæterorum lumine, & faſciarum adſpectu nitidiſſimè perſeverante.
3	24	29	*Kal.* Jul.	Illuceſcente Aurora, *Romæ* immerſio totalis intimi Satellitis Jovis clariſſimè ſpectata eſt; cum ante minuta ſecunda horaria 55″ circiter, lumen ejuſdem cœperit debilitari, cæteris Satellitibus & faſciis perſpicuè apparentibus, cœlo clariſſimo.

18 *Aug.*

H.	M.	S.		
0	40	45	18 *Aug.*	*Romæ*, intimus Jovis Satelles ex umbra cœpit emergere, & pleniſſimè lucebat in A,
0	41	25		inter limbum Jovis, & ſecundum Satellitem B, qui a limbo Jovis diſtabat ſemidiametro circiter Jovialis diſci. Satelles A emerſit ab umbra inter duas faſcias corporis Jovialis inferiores, ſitu everſo in Teleſcopio.
				Obſervatio fuit clariſſima & diligentiſſima, cœlo clariſſimo.
11	25	55	23 *Sept.*	*Romæ*, intimi Jovis Satellitis initium Emerſionis.
11	27	5		Totalis recuperatio luminis.
9	53	8	11 *Oct.*	*Albani*, ejuſdem intimi Satellitis initium Emerſionis.

Fig. 108.

7] *Eclipſe of* Jup. Satell. *at* Pekin *in* 1724, *and* Nº 405. *p.* 553.

Tempus Verum.					
H.	M.	S.			1724.
6	9	o p. m.	Nov.	5.	Satelles 3$^{us.}$ immerſus eſt in ♃ umbram.
6	44	o Veſp.		20.	Satelles 2$^{us.}$ prodiit ex umbra ♃.
6	14	o Veſp.		30.	Sat. 1$^{us.}$ ex ♃ umbram emerſit.
6	19	o Veſp.	Dec.	23.	Emerſio Satell. 1. ex umbra ♃.
					1725.
2	29	0	Jun.	23.	Satelles 3. ſubiit Jovis Umbram.
2	55	30	Julij	9.	Sat. 1$^{us.}$ in ♃ Umbr.
11	27	0	Aug.	9.	Imm. Satell. 1. in ♃ Umbr.
11	45	0		31.	Satell. 1 & 2$^{us.}$ ♃ in ☌ proxima penè in unum coaleſcebant. Non potuit diſcerni quiſnam prior Jovis umbr. ſubingreſſus ſit.
6	51	30 Veſp.	Sept.	19.	Emerſ. Sat. 1. ex ♃ Umbra.
10	45	Veſp.	Oct.	2.	Emerſ. Sat. 1 ex ♃ Umbra.
12	42	Mane.		10.	Idem Satell. 1 emerſit.
7	9	Veſp.		11.	Ejuſdem Emerſio.
6	46	Veſp.		15.	Satell. 3$^{us.}$ prodiit a tergo Jovis.
7	4				Dein diſparuit in Umbr. ♃.
10	20				Tandem ex eâdem Umbra emerſit.
9	9	Veſp.		19.	Satellitis 1. Emerſio.
9	6	Veſp.		20.	Satel. 2$^{us.}$ emerſit ex ♃ Umbra.
11	6	Veſp.		25.	Emerſ. 1. Sat. ex ♃ Umbra.
11	45	30 Veſp.		27.	Emerſ. 2. Satell. ex Umbra ♃.
7	27	40 Veſp.	Nov.	3.	Emerſ. 1. Satellitis.
5	42	30 Veſp.		19.	Ejuſdem Sat. 1. emerſio.
6	26	30 Veſp.		20.	Sat. 3$^{us.}$ cœpit emergere ex ♃ Umbr.

1724.

8] *Eclipſes of* Sat. Jupit. *in* 1724, 1725, 1726. *at* Southwick *in* Northamptonſhire, *by* G Lynn, *Eſq;* N° 393. *p.* 66.

1724.

Novemb. 8th, 7^h 37′ 7″ the firſt Satellite of *Jupiter* began to emerge: The ſame Day at 6^h 24′ 20″. The third Satellite began to immerge.

1725.

July 31ſt, 10^h 43′ 20″. The third Satellite immerged, that is I quite loſt Sight of it, at a little above a Semidiameter from *Jupiter*, but it began ſenſibly to abate of its Light above three Minutes before.

Auguſt 9th, 11$_h$ 51′ 20″. I loſt Sight of the ſecond Satellite; but it began ſenſibly to abate of its Light, about two Minutes before.

Auguſt 18th, 9^h 25′ 50″. The firſt Satellite immerged very near *Jupiter*'s Body.

The ſame Night, both my ſelf and Son plainly ſaw the Shadow of the third Satellite paſs over *Jupiter*'s Body, like a ſmall black Patch, tracing along the Middle of his bright Belt, above the moſt Southern Black one, and was in his Axis, as near as I could gueſs by the Eye, at 10^h 25, or 30′.

N. B. We could ſee it for about the middle Half of its Track, but not near *Jupiter*'s Edges.

Octob. 11th, 6^h 31′ 45″: The third Satellite began to emerge, and was full three Minutes and a half, before it was at its greateſt Luſtre, which I could then well judge of, by comparing it with the firſt Satellite, which was juſt a little above it, but nearer *Jupiter*. It came out of the Shadow, about half a Diameter from *Jupiter*'s Edge.

Decemb. 26th, 5^h 51′ 12″. The ſecond Satellite began to emerge.

1727.

Janu. 5th, 6$_h$ 28′ 30″. The third Satellite began to emerge.

Theſe Obſervations were made with a thirteen Foot Teleſcope whoſe Aperture was 2.4 Inches, and charged 2.5 Inches, all by apparent Time.

9] *Eclipſes of the firſt* Satellite *of* Jup. 1725, 1726. *by* F. Carbone *at* Liſbon. N° 394. *p.* 90.

Temp. Ver.				
H.	*M.*	*S.*		1725.
12	12	26	Jul. 28	Immergi viſus eſt intimus Satelles, in umbram Jovis veram.
12	11	35		Cœperat vero debilitari lumen.
15	0	10	Sep. 12	Emerſit ab umbra vera Jovis, cœlo ſatis ſereno; verùm ob Jovis cum Sole oppoſitionem, quæ ſeptem ante diebus contigerat, adeò Planetæ diſco proximus erat Satelles, ut ab ejus nimia claritate offuſcari aliquantulùm potuerit in primo ſui ab umbra egreſſu;

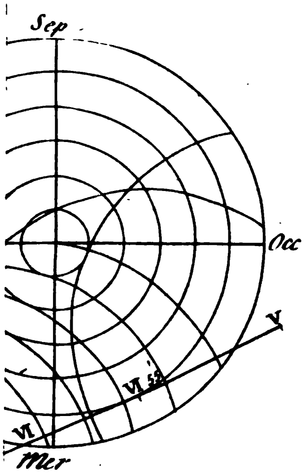
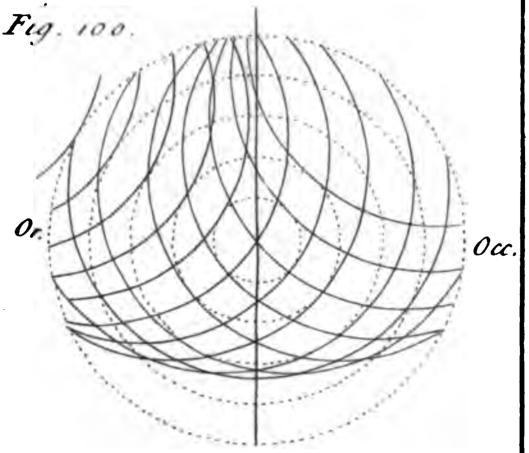


Fig. 100.



2.

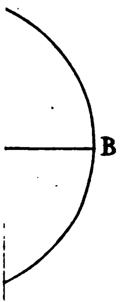


Fig. 103.

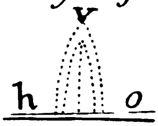


Fig. 104.

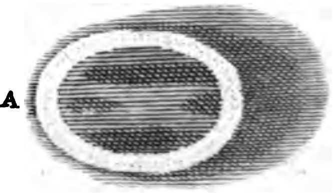


Fig. 106.

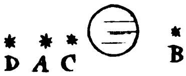


Fig. 107.

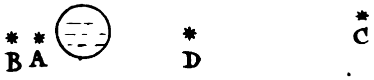
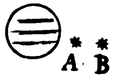


Fig. 108



Toms sculp

H.	*M.*	*S.*		
				egreſſu; ac proinde dubitari poteſt de aliquot ſecundis.
9	28	7	Sep. 14.	Cœpit ab umbra emergere.
9	29	0		Integrum verò lumen recuperavit.
11	24	55	21.	Initium Emerſionis ab umbra vera.
11	26	0		Totalis recuperatio luminis.
8	11	10	Oct. 23.	Initium Emerſionis.
8	12	10		Totalis recuperatio luminis.
6	30	4	Nov. 8.	Viſus eſt Satelles clareſcere in penumbra, cum aliqua tamen incertitudine, quoad pauca ſecunda, de vero initio emerſionis, ob aeris à vento trepidationem.
8	24	50	15.	Diſtinctè obſervatum eſt initium Emerſionis.
8	25	50		Recuperatio integra luminis.
8	32	40	Dec. 8.	Initium Emerſionis.
8	33	30		Totalis luminis recuperatio.
				1726.
4	58	50	Jan. 9.	Primò clareſcere viſus eſt, ſed luce tenuiſſima, ob aeris claritatem à crepuſculis, ac proinde non ſatis conſtat de vero initio emerſionis, ſaltem quoad ſecunda.
6	51	10	16.	Satis clarè ac diſtinctè primò emergere viſus eſt, aere omninò pacato, ac ſereno.
6	52	15		Totalis luminis recuperatio.

10] *Eclipſes of the firſt Sat. of* Jup. *Obſer. at* Toulon, *by* F. Ant. Laval *in* 1725, 1726. N° 394. *p.* 100.

		H.	'	"	
					1725.
Sept. 23.	Emerſio	6	56	42	T.V.
Oct. 16.	Emerſio	7	15	17	
Nov. 8.	Emerſio	7	31	33	
15.	Emerſio	9	26	52	
Dec. 17.	Emerſio	5	56	34	
Dec. 24.	Emerſio	7	49	18	dub.
	1726.				
Jan. 9.	Emerſio	6	2	3	dub.

11] *Eclipſes of the firſt* Sat. *at* Lisbon, 1726. *by* F. Carbone. N° 401. *p.* 408.

Temp. Ver.				*Immerſiones.*
H.	'	"		
15	24	0	Maij 23.	Debilitatio lucis.
15	24	40		Totalis Immerſio.
13	46	29	Jul. 1.	Debilitatio lucis.
13	47	47		Immerſio totalis.
15	40	30	8.	Debilitatio lucis.
15	41	40		Immerſio totalis.
12	0	45	17.	Debilitatio lucis.
12	1	52		Totalis Immerſio.

H.	′	″			
12	13	30	Aug.	9.	Immerſio totalis.
14	7	33		16.	Attenuatio luminis.
14	8	46			Totalis Obſcuratio.
16	3	10		23.	Attenuatio lucis.
16	4	23			Immerſio totalis.
10	31	40	Aug.	25.	Debilitatio luminis.
10	32	57			Totalis Obſcuratio.
12	27	57	Sept.	1.	Attenuatio lucis.
12	29	29			Totalis Immerſio.
8	53	47		10.	Debilitatio lucis.
8	54	54			Totalis Obſcuratio.
16	21	32		15.	Immerſio in Umbram.
10	50	12		17.	Attenuatio luminis.
10	51	39			Totalis Immerſio.
12	46	38		24.	Debilitatio lucis.
12	47	45			Totalis Obſcuratio. Dubia.
11	8	34	Oct.	10.	Immerſio totalis. Nonnihil dubia.
11	39	41		26.	Initium Emerſionis.
11	41	2			Integra luminis reſtauratio.
6	8	52		28.	Emerſio ab Umbra.
6	10	4			Emerſio à Penumbra.
8	6	3	Nov.	4.	Initium Emerſionis.
8	7	14			Totalis Emerſio.
9	57	39		11.	Initium Emerſionis.
9	58	49			Integra lucis reſtauratio.
0	20	19		20.	Emerſio ab Umbra.
6	21	29			~~Emerſio à Penumbra.~~
10	5	18	Dec.	4.	Emerſio ab Umbra. Dubia.
10	6	34			Totalis reſtauratio luminis.

12] *Eclipſes of* Jup. Satell. 1726, *at* Ingolſtad, *by the* Jeſuites. Nº 405. *p.* 557.

Temp. Ver.					*Immerſiones & Emerſiones.*
H.	*M.*	*S.*			
6	40	30	Jan.	6.	Satelles ♃ emerſit, Teleſcop. 14. ped.
15	4	20	Jun.	9.	Immerſ. Intimi; Teleſcop. 23 ped.
13	24	45	Jul.	17.	Immerſio ejuſdem; dub.
15	16	40		20.	Immerſio 2^{i}. in ♃ Umbr. Tel. 9 ped.
11	41	20	Aug.	2.	Immerſ. Intimi. Teleſcop. 12 ped.
12	25	6		14.	Immerſ. 2^{i}. eodem Teleſcop.
11	56	19		25.	Immerſ. Intimi. Teleſc. 23 ped.
11	43	17		26.	Incipit emergere ex ♃ Umbr. Satelles 3^{us}.
13	51	52	Sept.	1.	Immerſ. Intimi. Teleſcop. 23.
13	17	32		2.	Totalis Immerſ. Satellitis 3^{i}. in Umbram ♃.
15	45	9			Eodem die 1^{a}. Emerſ. 3^{ii}. è ♃ Umbra Tel. 10.

Sept.

H.	'	"			
9	40	circ.	Sept.	9.	Immerſ. 2i. Teleſc. 14.
15	50	30			Eodem die Imm. Intimi. Teleſc. 23.
17	20	30 circ.			Eodem die Imm. plena Satel. 3ii. in ♃ Umbram.
10	19	0			Immerſ. Intimi.
					Hæ duæ Eclipſes obſervatæ Biturgi *reſidentiæ* Collegii Ingolſtadienſis; *quam aliàs definivi in ortum vergere* 1' 40" *ab* Ingolſt. *Meridiano.*
10	17	10		10.	Immerſio intimi. Teleſc. 14.
	39	20		26.	Immerſ. ejuſdem, eod. Teleſc.

The following Obſervations were made with common Teleſcopes of 13, 15, 20½, and 22 Foot.

13] *Eclipſes of the* Satell. *of* ♃ *at* Peterſburgh, *in* 1726, 1727, 1728. *by Mr.* De Liſle. No 407. *p.* 37.

1726.	D.	H.	'	"	*What Eclipſes.*		*Teleſcope*	
July	10	12	47	0	Im. of the 1ſt		15 Foot	a little doubtful.
Aug.	9	14	51	30	Immer.——	1	15 and 22	doubtful near 15".
	18	11	15	46	Immer.——	1	15——	
		11	15	52	Immer.——		20½ ——	
Sept.	10	11	32	51	Immer.——	1	15——	
		11	32	56	Immer.——		20½ ——	
	22	16	13	20	Immer.——	2	15——	
Oct.	19	12	21	46	Em.——	1	15——	
	28	8	47	8	Em. ——	1	15——	to ſome Seconds.
Dec.	6	7	11	18	Em.——	1	20½ ——	ſomewhat doubtful.
		10	30	31	Em.——	2	20½ ——	exact.
		10	30	38	Em.——		15——	exact.
	29	7	15	36	Em.——	1	20½ ——	exact.
		7	15	48	Em.——		15——	exact.
1727.								
Jan.	2	10	59	46	Immer.——	3	15——	Air foggy.
		11	0	17	Immer.——		20½ ——	
	7	10	9	56	Em.——	2	20½ ——	
		10	10	4	Em.——		15——	
Feb.	1	7	17	15	Em.——	2	20½ ——	exact.
Aug.	5	11	52	23	Immer.——	3	22——	to a few Seconds.
	7	10	59	27	Immer.——	1	22——	♃ was low.
	8	13	37	9	Immer.——	2	22——	
	21	14	50	30	Immer.——	1	22——	
	30	11	19	18	Immer.——	1	22——	
Sept.	2	10	43	57	Immer.——	2	22——	
	6	13	11	24	Immer.——	1	22——	doubtful.
	9	13	21	35	Immer.——	2	22——	
	10	9	34	30	Emer. ——	3	22——	
	15	9	36	32	Immer.——	1	22——	
Oct.	31	10	8	48	Immer.——	1	15——	doubful.
Dec.	2	8	46	30	Em.——	1	22——	
	12	14	6	0	Em.——	3	22——	

1728.	D.	H.	′	″	*What* Eclipſe.	Teleſcope.	
Jan.	8	12	14	44	Emer. — — 2	15 Foot.	to ſome Seconds.
		12	33	34	Em. —— 1	13 ——	ſomewhat doubtful.
	10	5	58	7	Em. —— 3	22 ——	to ſome Seconds.
		7	0	12	Em. —— 1	22 ——	the wind incommoded.
	17	7	56	31	Imm. —— 3	13 ——	
		8	53	4	Em. —— 1	22 ——	exact.
		9	55	14	Em. —— 3	22 ——	
Feb.	16	10	59	26	Em. —— 1	22 ——	to ſome Seconds.
	18	5	28	20	Em. —— 1	15 ——	the day not cloſed.
	27	6	40	5	Em. —— 2	22 ——	
	29	8	0	29	Imm. —— 3	22 ——	the *Satellite* appeared and diſappeared at different Times.
Mar.	10	11	18	19	Em. —— 1	13 and 15	
Apr.	12	8	16	12	Imm. —— 3	15 ——	
		10	30	40	Em. —— 3	15 ——	♃ was low.

1727.

14] *Eclipſes of* Jup. Satellites, *at* Bologna, *in* 1727. *by Sign.* Euſt. Manfredi. Nº 404. *p.* 534.

Jan. 2. *N. S.*	h. 9. 45′. 47″. h. 11. 53′. 38″.	Immerſ. 3^{ii}. Satel. Tel. ped. 14. Emerſio ejuſdem. Dubia.
5.	h. 6. 51′ 54″.	Emerſ. 1^{i}. Satell. Teleſc. pedum 11. Bononienſ. Clar.
7.	h. 8. 54′. 12″.	Emerſ. 1^{i}. eodem Teleſc. Clariſſ.
Feb. 7	h. 5. 50′. 5″. h. 7. 52′. 54″.	Imm. 3^{i}. eodem Teleſc. Emerſ. ejuſdem Teleſc. ped. 14.
8.	h. 8. 37′. 59″.	Emerſ. 2^{i}. Teleſ. ped. 11. aëre nonnihil nebuloſo.
Aug. 21.	h. 13. 34′.. 39″.	Imm. 1^{i}. eodem Teleſc.
Sep. 6.	h. 11. 55′. 15″.	Imm. 1^{i}. eodem Teleſc.
17.	h. 10. 48′. 59″. h. 12. 40′. 30″.	Imm. 1^{i}. dub. eodem Teleſc. Emerſ. ejuſdem. ſubdub. eodem Tel.

1727.

15] *Eclipſes of the firſt* Satellite *of* Jup. *at* Lisbon, 1727. *by* F. Carbone. Nº 404. *p.* 535.

Oct. 15.	h. 9. 10′. 54″.	Immerſionem Intimi Jovis Satellitis obſervavi Teleſcopio pedum 22.
Nov. 7.	h. 9. 25′. 47″.	Ejuſdem Satellitis Immerſionem eodem Teleſcopio.

SATELL.

16] *Eclipſes of* ♃ Sat. *at* Pekin *in* 1727, and 1728. N° 414. *p.* 366.

SATELL. I.

		D.	H.	′	″	
Immerſiones.	Nov.	2	10	21	10	Veſp.
		10	0	14	26	Mane.
		11	6	44	10	Veſp.
Emerſiones.	Dec.	3	2	30	42	Mane.
		10	4	22	5	Mane.
		11	10	50	0	Veſp.
		13	5	17	50	Veſp.
		19	0	40	44	Mane.
		20	7	8	20	Veſp.
		26	2	32	33	Mane.
		27	9	0	0	Veſp.
1728.	Jan.	3	10	51	50	Veſp.
		5	5	20	0	Veſp.
		11	0	45	18	Mane.
		12	7	13	27	Veſp.
		19	9	5	40	Veſp.
		26	10	59	0	Veſp.
		28	5	27	20	Veſp.
	Feb.	4	7	22	0	Veſp.
		11	9	16	40	Veſp.
		18	11	12	30	Veſp.
		20	5	41	50	Veſp.
	Mart.	21	7	58	55	Veſp.
Immerſiones.	Sept.	20	1	12	12	Mane.
	Oct.	4	5	6	0	Mane.
		13	1	50	0	Mane.
		20	3	26	15	Mane.
		27	5	19	30	Mane.

SATELL. II.

Imm. 1727.	Nov.	6	4	5	40	Mane.
Emerſiones.	Dec.	1	3	40	45	Mane.
		4	5	2	0	Veſp.
		11	7	37	42	Veſp.
		18	10	11	13	Veſp.
		26	0	47	39	Mane.

SATELL. II.

		D.	H.	'	"	
1728.	Jan.	5	4	42	0	Veſp.
		12	7	16	16	Veſp.
Emerſiones.		19	9	51	0	Veſp.
	Feb.	13	7	3	45	Veſp.
		20	9	46	0	Veſp.
Immerſio.	Oct.	30	3	34	10	Mane.

SATELL. III.

		D.	H.	'	"	
1727.						
Incipit emergere	Nov.	21	7	57	0	Veſp.
		28	11	53	0	Veſp.
1728.						
Immerſ. tot.	Jan.	3	5	43	40	Veſp.
Emerſ. prima			7	42	0	Veſp.
Immerſ. tot.		10	9	42	52	Veſp.
Emerſ. prima			11	42	20	Veſp.
Immerſio totalis.	Feb.	22	9	42	30	Veſp.
	Oct.	9	6	6	30	Mane.

17] *Eclipſes of* Jup-Sat. *at* Pekin, *in* 1728, and 1729. N° 416. *p.* 457.

Immerſiones SATELL. I.

		D.	H.	'	"	
1728.	Nov.	5	1	42	45	Mane.
		12	3	36	15	Mane.
		13	10	4	10	Veſp.
		19	5	28	20	Mane.
		20	11	55	56	Veſp.
		28	1	47	50	Mane.
		29	8	16	35	Veſp.
	Dec.	6	10	8	0	Veſp.
		12	5	30	45	Mane.
		15	6	27	0	Veſp.
		22	8	17	0	Veſp.

Emerſiones SATELL. I.

		D.	H.	'	"	
1728.	Dec.	31	6	50	15	Veſp.
1729.	Jan.	7	8	40	40	Veſp.
		16	5	0	0	Veſp.
		22	0	24	10	Mane.
		23	6	52	20	Veſp.

	30	8	46	15	Veſp.
Feb.	15	7	5	0	Veſp.
Mart.	10	7	21	40	Veſp.
	17	9	19	50	Veſp.
	24	11	16	15	Veſp.

Immerſiones SATELL. I.

	D.	H.	′	″	
Nov.	1	2	58	45	Mane.
	15	6	45	0	Mane.
	17	1	12	15	Mane.

Immerſiones SATELL. II.

	D.	H.	′	″	
Nov.	6	6	8	45	Mane.
Dec.	1	3	3	20	Mane.
	8	5	35	55	Mane.
	18	9	25	0	Veſp.

Emerſiones SATELL. II.

	D.	H.	′	″	
Jan.	2	5	21	30	Mane.
	5	6	37	0	Veſp.
	19	11	44	15	Veſp.
	27	2	20	0	Mane.
Feb.	6	6	14	18	Veſp.
	13	8	49	0	Veſp.
	20	11	28	45	Veſp.
Mart.	10	6	9	0	Veſp.
	17	8	49	40	Veſp.
	24	11	30	10	Veſp.
Maii	20	8	49	30	Veſp.

Immerſiones SATELL. II.

	D.	H.	′	″	
Nov.	17	11	52	25	Veſp.

Immerſiones SATELL. III.

1728.	D.	H.	′	″	
Nov.	6	10	4	10	Veſp. diſparuit plenè immerſus in umbr.
	7	0	47	15	Mane, cœpit rurſum emergere.
	21	6	1	5	Mane, plenè immerſus.

1729.

	D.	H.	′	″	
1729. Jan.	24	5	24	20	Veſp. plenè diſparuit in umbra.
	24	8	21	40	Veſp. denuò cœpit promicare,
	31	9	25	36	Veſp. integrè immerſus fuit.
Feb.	1	0	21	0	Mane rurſum prodire cœpit.
Mart.	15	9	33	0	Veſp. plene immerſus in umbram.

1729. Immerſiones SATELL. IV.

	D.	H.	′	″	
Jan.	16	6	30	0	Veſp. circiter, ingreſſus eſt umbram.
	16	9	24	0	Veſp. cœpit rurſum ſenſim emicare.
Mart.	24	6	46	20	Veſp. plenè diſparuit in umbra.
	24	10	10	20	Veſp. denuò promicare cœpit.

SATELL. I.

18] *The ſame continued at* Pekin *in* 1729 *and* 1730, *by* Ign. Koegler *and* Andr. Pereyra. *S.J.* № 420. *p.* 182.

		D.	H.	′	″		
1729. Immerſiones.	Dec.	1	4	56	00	a. m.	
		8	6	45	47	a. m.	
		10	1	14	30	a. m.	dub.
		17	3	4	5	a. m.	
		18	9	32	10	p. m.	
		25	11	22	15	p. m.	
		31	6	44	6	a. m.	
1730.	Jan.	2	1	12	26	a. m.	
		9	3	3	45	a. m.	
		10	9	31	00	p. m.	
		17	11	22	30	p. m.	
		25	3	33	30	a. m.	
Emerſiones.	Feb.	2	11	54	15	p. m.	
		10	1	48	0	a. m.	
		17	3	44	20	a. m.	
		18	10	11	40	p. m.	
		26	0	7	45	a. m.	
		27	6	36	40	p. m.	
	Mart.	6	8	32	30	p. m.	
		13	10	29	0	p. m.	
		21	0	25	50	a. m.	
		29	8	53	26	p. m.	
	Apr.	5	10	49	55	p. m.	
	Maii	14	9	28	45	p. m.	
	Jun.	22	7	55	30	p. m.	dub.
Immerſio.	Nov.	4	6	0	0	a. m.	

SATELL. II.

		D	H.	'	"		
1729. Immerſiones.	Dec.	27	1	41	30	a. m.	
1730.	Jan.	3	4	10	45	a. m.	
		12	7	57	15	p. m.	
Emerſiones.	Feb.	7	7	47	27	p. m.	
		22	0	58	50	a. m.	
	Mart.	1	3	36	20	a. m.	dub.
		11	7	33	15	p. m.	
		18	10	13	36	p. m.	
		26	0	51	45	a. m.	
	Apr.	12	7	30	48	p. m.	
	Maii	21	10	6	50	p. m.	

SATELL. III.

		D.	H.	'	"		
1729. Immerſiones. 1730.	Dec.	6	1	14	0	a. m.	dub.
		13	5	8	0	a. m.	
	Jan.	10	8	46	30	p. m.	
		18	0	42	0	a. m.	
Emerſiones.	Feb.	15	8	6	50	p. m.	
		23	0	5	6	a. m.	
	Mart.	30	8	14	46	p. m.	
	Apr.	6	8	41	0	p. m.	
	Maii	12	8	22	0	p. m.	

SATELL. IV.

			D.	H.	'	"		
1729.	Imm.	Dec.	1	1	12	40	a. m.	
	Emerſ.			5	48	0	a. m.	
1730.	Emerſ.	Feb.	6	5	38	0	a. m.	dub.
	Imm.		22	6	45	15	p. m.	
	Emerſ.			11	30	0	p. m.	

SATELL. I.

		D.	H.	'	"
1730. Immerſiones.	Nov.	3	18	0	p. m.
		12	14	20	
		19	16	12	
		26	18	3	
	Dec.	5	14	22	54
		12	16	11	30
		19	18	0	45

19] *Eclipſes of* Jup. Satel. *in* 1730, *and* 1731, *at* Pekin, *by* F. Ign. Koegler *and* And. Pereira. N° 424. *p.* 316.

		D.	H.	′	″
1730.	Dec.	21	12	28	40
		28	14	18	10
1731.	Jan.	4	16	8	45
		6	10	35	20
Immerſiones.		11	17	59	30
		13	12	27	10
		20	14	17	30
		27	16	10	12
	Feb.	3	18	2	36
		12	14	25	dub.
		14	8	54	20
	Mar.	2	9	30	
		9	11	27	40
		16	13	23	30
		18	7	52	40
Emerſiones.	Apr.	1	11	45	20
		3	6	15	
		17	10	8	40
		24	12	4	30
	Maii	3	8	29	50

SATELL. II.

		D.	H.	′	″
1730.	Nov.	25	16	5	30
	Dec.	2	18	37	dub.
		20	12	49	45
Immerſiones.		27	15	21	5
1731.	Jan.	3	17	49	50
		14	9	30	45
		28	14	37	30
	Feb.	4	17	10	
		15	8	59	
	Mar.	19	11	29	20
Emerſiones.	Apr.	13	8	35	
		20	11	16	

SATELL. III.

			D.	H.	′	″
1730.	Immer.	Nov.	21	16	16	30
	Emer.	Dec.	20	11	27	50
	Immer.		27	11	49	30
	Emer.			15	21	17

			D.	H.	′	″
1731.	Immer.	Jan.	3	15	43	15
	Emerſ.			19	16	dub.
	Immer.	Feb.	8	11	25	30
	Immer.		15	15	23	
	Emer.	Mar.	9	6	50	30
	Emer.		16	10	50	50
	Emer.		23	14.	51	30
	Emer.	Apr.	21	6	56	20
	Immer.		28	7	28	30
	Emer.			10	55	30
	Immer.	Maii	5	11	30	30

SATELL. IV.

			D.	H.	′	″
1730.	Immer.	Dec.	20	18	50	45
1731.	Immer.	Jan.	6	12	38	12
	Emer.			17	6	45
	Emer.		23	10	54	
	Immer.	Mar.	31	6	inter	30′ & 35″
	Emer.			10	43	40

Obſervations on Jupiter, Mars, *and* Venus.

XXXIV. *Obſervations of* Jupiter *at* Pekin, *in* 1727, *by* Ign. Koegler. N° 405. *p.* 553.

Maii die 10. Hora 4, mane, diſtantia Jovis a Stella Φ Aquarii 9′ 5″. Jupiter erat ad occaſum; eſtque diſtantia computanda à centro.

Die 11. Eâdem hora; *Jupiter* jam prætergreſſus Stellam Φ, ab ea diſtabat ad ortum boreum 1′, 10″. Scil. a centro. Horâ 5^{ta.} Diſtantia centrorum 1′ 36″.

Die 12. Hora 4. diſtantia Jovis à Stellâ Φ, 10′ 10″.

Die 13. Eâdem hora, 19′ 50″.

XXXV. 1] *An Occultation of* Mars *by the Moon, at* Toulon, *by* F. Ant. Laval. N° 394. *p.* 101.

Jan. die 18. 1726. *N. S.* *Martis* occultatio à *Lunâ*, Temp. Ver. Hor. 7. Min. 23. Veſpere. Non ſatis certa.

Emerſio *Martis* Hor. 8. Min. 21. Sec. 34. certa.

2] *At* Ingolſtad, Jan. 19. 1726. *by the* Jeſuites. N° 405. *p.* 556.

Tempora.			*Phaſes.*
H.	′	″	
6	52	0	Jan. 19. *Mars* per vapores tranſlucens ſtabat ad Lunæ limbum lucidum.
6	54	0	— — — Erat penitus immerſus.
7	54	25	— — — Centrum Martis emergit è ☾ limbo obſcuro.
7	54	35	— — — Totus Mars extra Lunam.

Tranſitus Martis fuit in linea ex centro Grimaldi per extremitatem boream *Langreni* ducta. Inde habita ratione librationis Lunaris, collecta centrorum diſtantia minima, 2′ 30″, Marte auſtraliore. Semidiameter Lunæ apparens hora 9 erat 16′ 55″.

Obſervatio facta eſt Teleſcopiis 10 & 12 pedum.

3] *Another at* Ingolſtad. Nº 405. p. 558.

Tempora.			*Phaſes.*
H.	′	″	
5	25	17	Aug. 1. Evanuit *Mars* ex oculis in *Lunæ* limbum obſcurum.
6	1	53	—— —— Emerſio 1ª· *Martis* ad Zoroaſtrum.
6	1	59	—— —— Emerſio totalis *Martis* facta obſervatione Teleſcopiis 12, 14 & 16 pedum.
			—— —— Diameter *Lunæ* apparens hora 7½, erat 32′ 47″.

4] *Another at* Pekin, 1731. Nº 424. *p.* 318.

Nov. die 14. 1730. Circa hor. 4. p. m. Luna obtexit Martem. Immerſio, claro adhuc die, videri non potuit: obſervata tamen eſt Emerſio, quæ accidit hor. 4. 54′ proximè Furnerium.

Obſervations of Venus.

XXXVI. 1] *An Occultation by the Moon at* Bononia, Sep. 18 *N. S.* 1727. *by* S. Euſt. Manfredi. Nº 404. *p.* 535.

Tempora.			*Phaſes.*
H.	′	″	
0	27	21	Sept. 18. Incipit Venus occultari poſt limbum Lunæ obſcurum.
0	28	13	—— —— Immerſio totalis ♀.
1	16	45	—— —— Incipit ♀ emergere è limbo ☾ lucido.
1	17	50	—— —— Emerſit tota.
1	46	24	—— —— Nunc limbus præcedens ☾ illuminatus præcedit ♀ in circulo horario 21″ temporis; & limbus Borealis ☾ item illuminatus eſt auſtralior ♀ 29″ temporis.

2] *Another at* Berlin, Sep. 19. p. m. N. S. 1729. *by Mr.* Kirck. Nº 412. *p.* 256.

Contigit Acceſſus Lunæ ad Venerem 2 h. 2′ 16″. Occultatio totalis 2 h. 3′ 1″. Idem, per Teleſcopium octodecim Pedum notavit Venerem ferè in Quadraturâ poſitam, cum prope Lunæ diſcum accederet, figuram mutâſſe, & Falcis cuſpides amiſiſſe; unde Ovalis vel Elliptica figura oriebatur: Quod ſpectaculum pro comprobandâ Lunæ Atmoſphærâ laudari poſſe *D. Kirchius* cenſet.

April

XXXVII. *A Conjunction of* Venus *and the* Moon, April 2, 1729. *at* Wittemberg, *by Mr.* F. Weidler. N° 412. *p.* 252.

April 2. 1729. At 7 H. 13′ Mr. *Weidler* obſerved *Venus* placed in ſuch manner near the *Moon*, that the Horns of the *Moon* were in the ſame right Line with *Venus*, which was then diſtant from the Southern Cuſp of the *Moon* 1 Deg. 10′. At 7 H. 30′. he meaſured the Diſtance of *Venus* from the Eaſtern Cuſp of the *Pleiades* to be 2 Deg. 15′, and the Horn of the Moon at the ſame Time was diſtant from the ſame Cuſp 1 Deg. 53′, 45″; the intermediate Diſtance of the Horns of the *Moon* was 29′ 30″.

XXXVIII. *Meridional Altitudes of* Venus *obſerved at* Toulon *by* F. Ant. Laval, *in* 1725. N° 394. *p.* 101.

Altitudines Meridianæ apparentes Veneris.

1725.	Gr.	′	″		Gr.	′	″
Mart. 20.	36	34	30	Dec. 7.	23	59	30
April 21.	51	43	0	21.	28	21	0
Maii 8.	59	35	0	24.	29	30	0
Sept. 8.	44	30	30	1726.			
21.	37	57	0	Jan. 9.	36	29	0
24.	36	26	30	19.	41	19	0
Oct. 18.	26	28	45	31.	47	14	0
Nov. 8.	21	50	0	Feb. 3.	48	40	30

A Tranſit of Mercury.

XXXIX. 1] *The mean Motion of* Mercury, *and his Nodes determined by a Tranſit of that Planet over the Sun's Disk* Octob. 29*th*. 1723. *by Dr.* E. Halley. N° 386. *p.* 228.

The Tranſit of the Planet *Mercury*, over the Diſk of the *Sun*, being one of the moſt curious and uncommon Appearances that the Heavens afford, our Aſtronomers, both at home and abroad, made due Preparation to obſerve, with the utmoſt Exactneſs, that which happened on the 29th of *October*, 1723, which I had predicted in the Year 1691 (*Phil. Tranſ.* N° 193.) would be, in Part, viſible in *England*. And the Sky proving, more than ordinary, favourable at that Time, we were enabled to obſerve the Ingreſs on the *Sun*'s Limb, with the greateſt Accuracy.

Accordingly, the ſame Day, *Octob.* 29. *ſtyl. vet.* at *Greenwich* in the *Royal Obſervatory*, I firſt perceiv'd, with my 24 Foot Tube, the *Planet* making a ſmall Notch in the *Sun*'s Limb at $2^h\ 41'\ 23''$ *T. app.* And at $2^h\ 42'\ 26''$ he was wholly enter'd, making an interiour Contact, the Light of the *Sun*'s Limb juſt beginning to appear behind his dark Body; which notwithſtanding the Slowneſs of the Motion, was, in a Manner, inſtantaneous. Then, applying the *Micrometer* to the ſaid 24 Foot Tube, I open'd it ſo as to take in 16′ 15″ equal to the *Sun*'s Semidiameter at that Time; and cauſing the northern Edge of the *Sun*, to move exactly along one of the Pointers, I waited till the Center of *Mercury* came to move along the other, as I found it to do at $3^h\ 1'\ 16''$ *T. app.* But *Refraction* contracting this Difference of Declination about 5 Seconds (the *Sun* being then but about 11° high) I concluded that the Centers of the *Sun* and *Mercury*, were truly in the ſame Parallel of Declination at 3^h 3′ *T. app. proxime*.

At *Wanſted* in *Eſſex*, my worthy Collegue, the Reverend Mr. *James Bradley*, *Savilian* Profeſſor of *Aſtronomy*, obſerved with the *Hugenian Teleſcope*, of above 120 Foot long, the total Immerſion, or interiour Contact of the Limbs, at 2^h 26′ 45″ *T. æq.* that is 2^h 42′ 38″ *T. app.* twelve Seconds later than I found it at *Greenwich*; moſt of this Difference being due to the Difference of our Meridians. And applying the *Micrometer* to that vaſt *Radius*, he meaſured the Diameter of the *Planet* 10″ 45‴. At 2^h 48′ 57″ he found the Difference of Declination between the ſouthern Limbs of the *Sun* and *Planet* by the *Micrometer*, in a fifteen Foot Tube, to be 15′ 19″. Wherefore, allowing the obſerved Semidiameter of the *Planet*, and the *Refraction*, the ſaid Difference was neareſt 15′ 30″, and conſequently, *Mercury* more ſoutherly than the *Sun*'s Center in reſpect of Declination 0′ 45″.

Mr. *George Graham*, in *Fleet-ſtreet*, obſerved the firſt Impreſſion on the *Sun*'s Limb at 2^h 41′ 9″ *T. app.* and at 2^h 42′ 19″ *Mercury* was intirely within the Diſk. At 3^h 6′ 41″ he meaſured with a *Micrometer*, in a twelve Foot Tube, the Diſtance of his Center from the neareſt Limb of the *Sun* 2′ 13″. And again, at 3^h 25′ 24″ their Diſtance was found 3′ 57″. At 3^h 34′ 43″ he meaſured the Difference of Declination, from the northern Limb of the *Sun* 14′ 57″, which, corrected by *Refraction*, becomes 15′ 4″, that is, 1′ 11″ more northerly than the *Sun*'s Center.

In the *Obſervatory* at *Paris*, *Signor Maraldi* obſerved the firſt Appearance of *Mercury* on the *Sun*'s Limb at 2^h 50′ 13″ *T. app.* and the interiour Contact at 2^h 51′ 48″. And Mr. *de Liſle*, obſerving a-part, concluded the ſame at 2^h 51′ 37″, but ſuſpects it might have been ſome few Seconds later. This Gentleman has communicated his Obſervation at large, from whence we ſhall only borrow the following obſerved Latitudes.

	H.	′	″		′	″
At	2	56	20	*Latitudo Borea Mercurii*	3	36
	3	0	40		3	42
	3	10	20		3	46
	3	16	12		3	55

At *Bononia*, in *Italy*, Signor *Manfredi* obſerved *Mercury* indenting the *Sun*'s Limb at 3^h 26′ 22″; and that he was gotten entirely within, at 3^h 27′ 45″. And theſe are the Obſervations moſt to be depended on, that we have received from abroad.

In order to deduce from this *Phænomenon*, ſo accurately obſerv'd, what may contribute to the Perfecting of the Theory of *Mercury*'s Motion, which (as appears by the near Agreement of our Numbers with this and many other Obſervations of him) ſeems to need but very little Correction; I carefully computed, from our Tables, the Motion of the Planet in five Hours, and found his apparent Motion on the *Sun*, to be in Longitude 29′ 21″ Retrograde, and that his Latitude encreas'd northerly 4′ 17½″ in the ſame Time; whence the Horary Motion in Longitude 5′ 52″; and in Latitude 0′ 51½″, and thence the Angle of the viſible Way with the Ecliptick 8° 19′, and the Horary Motion in that Way 5′ 56″. Again, the Angle of the Ecliptick with the Meridian, being in this Place 73° 24′, the viſible Way of *Mercury*, made an Angle of 65° 5′ with the Meridian paſſing through the Center of the *Sun*, whence the Horary Change of Declination becomes exactly 2′ 30″.

Theſe *Data* I chooſe rather to take from the Theory, than from immediate Obſervation; becauſe there is always an unavoidable, though ſmall Uncertainty, in what we obſerve, yet greater than there can be in the Computation for ſo ſmall a Space of Time, eſpecially now the Theory is, as I ſaid before, ſo very near the Truth.

This premiſed, let us now enquire the true Time of the central Ingreſs, and the Latitude of the Planet at that Time. And firſt, by my own Account, *Mercury* was gotten into the Parallel of the *Sun*'s Center, 21½ Minutes after the central Ingreſs, in which Time he aſcended to the *Northward* 0′ 54″, and ſo much, therefore, was he more *Southerly* than the *Sun*'s Center at his Ingreſs. Mr. *Bradley* 7½ Minutes after the ſaid Ingreſs, in which the Planet aſcended 0′ 19″ found his Declination 0′ 45″ South, and therefore at the Ingreſs, his Declination was 1′ 4″ South. And by Mr. *Graham*'s Obſervation; *Mercury* was more northerly than the *Sun*'s Center 1′ 11″, 53′ 20″ after the central Ingreſs; but in that Time, *Mercury* aſcended 2′ 13″, wherefore, according to him, at the Ingreſs the Planet had 1′ 2″ South Declination. We ſhall not therefore err above a Semidiameter of *Mercury*, if we aſſume his Declination at that Time, to have been preciſely one Minute.

Now the *Sun*'s Semidiameter being then 16′ 15″, one Minute is the *Sine* of 3° 32′ in the Arch of the *Sun*'s Limb; and conſequently, the Point of this Ingreſs was 13° 4′ more northerly than the Ecliptick; whence the Latitude of *Mercury* was then 3′ 40″ North, and Difference of Longitude 15′ 50″, by how much he, at that Time, follow'd the *Sun*'s Center.

If therefore, to the Arch of 13° 4′, we add the Double of 8° 19′, or of the Angle which the viſible Way made with the Ecliptick, we ſhall have 29° 42′ for the Point on the *Sun*'s weſtern Limb, at which the Planet made his *Exit*, likewiſe to the North of the *Ecliptick*. Hence the Chord, deſcribed in the whole Tranſit, was of 137° 14′; and the Chord itſelf 30′ 16″; and the neareſt Diſtance to the *Sun*'s Center 5′ 56″. Now the Horary Motion in this Chord, being 5′ 56″, the whole Duration of this *Mercurial Eclipſe* becomes

$5^{h}\,6'$ in reſpect of the Center of the *Planet*; and therefore the neareſt Approach of their Centers was at $5^{h}\,14'\,30''$ at *Greenwich*, and the *Exit* at $7^{h}\,47'\frac{1}{2}$ both viſible in our *American Plantations*, had there been any curious Perſon there qualified to obſerve them.

It follows likewiſe, by the obſerved Diameter of *Mercury*, $10''\,45'''$ that he was very little leſs than two Minutes of Time in paſſing the Limb; and, by the given neareſt Diſtance to the *Sun*'s Center, it is concluded that he was in Conjunction, in Point of Longitude, at $5^{h}\,23'\,15''$, having then preciſely $6'\,00''$ North Latitude. Nor can it be doubted, but that all this would have been found exceeding near to Truth, had not the too early ſetting of the *Sun* deprived all *Europe* of the deſirable Sight.

There being a very remarkable *Period* of the Motion of *Mercury* in 46 Years, in which Time, he makes 191 Revolutions about the *Sun*; this Tranſit of ours is found to have been preceded by two others at that Interval: The firſt, in the Year 1631, when *Gaſſendus* at *Paris*, on the 28th Day of *October*, *ſtyl. vet.* was the firſt that ever obſerved this Appearance of *Mercury* within the *Sun*'s Diſk, and found him to paſs off at $10^{h}\,28'$ *mane*. The ſecond was *Octob.* 28, 1677, when myſelf had the good Fortune to obſerve both the Ingreſs and Egreſs of the Planet in the Iſland of St. *Helena*; the middle Time, when he was neareſt to the *Sun*'s Center, being there but $3'\,50''$ paſt Noon, and the viſible Duration of the Tranſit of the Center of the Planet $5^{h}\,14'\,20''$; which was ſome ſmall matter contracted by Parallax, and moſt likely might have been $5^{h}\,15'\,00''$; without it. Now in $5^{h}15'$, *Mercury* deſcribed the Chord of $146^{\circ}\,52'$ in the *Sun*'s Limb, being $31'\,9''$, and conſequently the neareſt Diſtance to the Center was $4'\,38''$, or the Sine of $16^{\circ}\,34'$ the *Sun*'s Semidiameter being Radius; that is, $1'\,18''$ leſs than we found it in 1723. Hence alſo it follows, that the true Conjunction in Longitude was 7 *min.* of Time later than the neareſt Approach of the Centers, *viz.* at $0^{h}\,10'\,50''$ at St. *Helena*, or at $0^{h}\,35'$ paſt Noon at *Greenwich*: And, that the North Latitude of the Planet, at that Time, was $4'\,41''$.

Suppoſing, therefore, the neareſt Diſtance of the Centers in the Tranſit of 1631, to have been $3'\,20''$, that is, $1'\,18''$ leſs than in 1677, we ſhall find that *Mercury* then deſcribed a Chord of $156^{\circ}\,20'$, traverſing the Diſk of the *Sun* in $5^{h}\,21'\,30''$; ſo that ſuppoſing his *Exit* at $10^{h}\,28'$ at *Paris*, that is $10^{h}\,18'\,40''$ at *Greenwich*, he enter'd on the *Sun* at $4^{h}\,57'\,10''$ in the Morning; and was neareſt his Center at $7^{h}\,38'$ *T. app.* but in the ſame Longitude with him at $7^{h}\,43'$, or *Octob.* 27 $19^{h}\,43'$ *T. app* having then $3'\,22''$ North Latitude.

And here, I think I may, without Vanity, advertiſe the Reader, that above thirty Years ſince, *viz.* in *Philoſoph. Tranſ.* N° 193, for the Month of *March*, &c. $169\frac{0}{1}$, I predicted, by Help of the two former,

former; this laſt Tranſit, with a ſurpriſing Exactneſs, even beyond my Hopes, making the Time of the middle, or neareſt Approach of the Centers of the *Sun* and *Mercury*, *Anno* 1723, *Octob.* 29^{d} 5^{h} 19' *T. app.* which we found by Obſervation at 5^{h} $14'\frac{1}{2}$, only $4'\frac{1}{2}$ Minutes ſooner; and, in Latitude, *Mercury* was but ſix Seconds more ſoutherly than I then had computed it; the Error, in Longitude, being little more than two Diameters of this exceeding ſmall Planet; and, in Latitude, but a ſingle Semidiameter thereof. So, that for the Future, Aſtronomers may truſt my Table of theſe Tranſits, in *Tranſact.* N° 193, to a few Minutes of Time, and not wait with the Uncertainty of Hours, nay Days, as has lately been done.

But, in order to obtain a yet further Degree of Exactneſs by Help of this Obſervation, it may be moſt expedient to compare with it the Ingreſs I obſerved at St. *Helena*; becauſe, in that, as well as in this, the Latitudes of the Planet being very ſmall, a little Error in them will not ſo much affect the Longitudes. Suppoſing therefore, that *Anno* 1677, *Octob.* 27° 21^{h} 26' 15" at St. *Helena*, or 21^{h} 50' 15" *T. app.* at *Greenwich*, the Center of *Mercury* entered on the *Sun*, and that, at that Time, he was $8\frac{1}{4}$ Degrees on the *Sun*'s Limb, to the North of the Ecliptick (according to what is above concluded) it follows, that he had then 2' 20" North Latitude, and 16' 5" greater Longitude than the *Sun*'s Center; as in this preſent Tranſit, *Octob.* 29° 2^{h} 41' 30" *T. app.* at *Greenwich*, he had 3' 40" North Latitude, and 15' 50" more Longitude.

Now the apparent Geocentrick Differences of Longitude, are to the real Heliocentrick Differences, as the Planet's true Diſtance from the *Sun*, to his Diſtance from the *Earth*; that is, in both Caſes, as 313 to 676; wherefore, in 1677, *Mercury* wanted 34' 45" of the Conjunction with the *Sun*; and, in 1723, but 34' 13", at the Times of his apparent Ingreſs on the Diſk. And, equating the Times, I find, that the *Sun*, *Anno* 1677, *Octob.* 27^{d} 21^{h} 34' 20" *T. æq.* was, in ♏ 15° 36' 55", and, conſequently *Mercury*'s Heliocentrick Place ♉ 15° 2' 10": And, *Anno* 1723, *Octob.* 29^{d} 2^{h} 25' 30" *T. æq.* the *Sun* was in ♏ 16° 39' 43", and therefore *Mercury*, at that Time, in ♉ 16° 5' 30".

Mercury therefore, in 46 Years with 11 Intercalations, and beſides 1^{d} 4^{h} 51' 10", has made 191 Revolutions to the Equinoctial Points, and over and above 1° 3' 20". But, by the *Scholion* to *Prop.* XIV. *Lib.* III. *Natur. Philoſoph. Principia Math.* the Motion of the *Aphelion* of *Mercury*, from the Equinox in that Time, is 40' 18"; ſo that there remains 23' 2" of *True Anomaly* to be reduced to the *Mean*: Now the Mean Anomaly of *Mercury*, in both Caſes, being 5 *ſig.* 12°, 23' 2" of True Anomaly gives 15' 24" Mean Anomaly; which added to 40' 18" becomes 55' 42", for the Mean Motion above ſo many Revolutions: And this is to be encreaſed by 8" to reduce it to the Plane of *Mercury*'s Orb, in all 55' 50".

Hence, doubling the Interval, in 92 *Julian* Years $1^d\ 9^h\ 42'\ 20''$, the Mean Motion of *Mercury* from the Equinox is $0^s\ 1^\circ\ 51'\ 40''$, from which, taking $5^\circ\ 44'\ 50''$ the Motion in $1^d\ 9^h\ 42'\ 20''$, we have his Motion in 92 *Julian* Years $11^s\ 26^\circ\ 6'\ 50''$, and in 100 Years, $2^s\ 14^\circ\ 2'\ 13''$, which is but $20''$ more than I had ſome Years ſince printed it, in my Aſtronomical Tables ſhortly to be publiſhed, and differs but one Hour's Motion therefrom in 3000 Years.

The forementioned Proportion of the Diſtances, *viz.* 313 to 676, is alſo between the Latitudes ſeen from the *Earth* and the Inclinations, or Heliocentrick Latitudes of the Planet: So that $2'\ 20''$, at the Ingreſs of 1677, gives $5'\ 2''$; and $3'\ 40''$ in 1723, becomes $7'\ 55''$ for the Latitudes at the *Sun*. And the Inclination of the Orb of *Mercury* to the Plane of the Ecliptick (determined by accurate Obſervations near his northern Limit) being $6^\circ\ 59'\ 20''$, we compute the Diſtance of the Planet from his Node, in the former $0^\circ\ 41'\ 7''$, and, in the latter, $1^\circ\ 4'\ 37''$; which, being deducted from his Heliocentrick Places reſpectively, leave the Place of the aſcending Node, in 1677, ♉ $14^\circ\ 21'\ 3''$; and, in 1723, ♉ $15^\circ\ 0'\ 53''$: So, that in 46 Years the Node is found $39'\ 50''$ forwarder in the Ecliptick; which is but $1'\ 30''$ more than the Præceſſion of the Equinox in the ſame Time. We may therefore ſafely aſſume the Plane of the Orb of *Mercury* to be immoveable in the Sphere of fix'd Stars, and its aſcending Node to be $0^s\ 15^\circ\ 41'$ from the *firſt Star of* Aries. Nor can ſo very ſlow a Motion (ſuppoſing ſuch to be) be fully defined, but by the utmoſt Care and Diligence of future Aſtronomers, after the Obſervation of many Ages.

As to the reſt of the Theory of this Planet's Motion, I make his mean Diſtance from the *Sun*, 38710 ſuch Parts as the mean Diſtance of the *Sun* and *Earth* is 100000; and his greateſt Equation $23^\circ\ 42'\ 37''$. The *Epocha* of his middle Motion, *ineunte Anno* 1723, *ſtyl. vet.* from the Equinoctial Point, I make ♐ $19^\circ\ 9'\ 31''$; and that of his Aphelion to the ſame Time ♐ $13^\circ\ 3'\ 34''$: The Aphelion moving *ſecundum Seriem Signorum*, ſeven Minutes in eight Years. And theſe Numbers I preſume, may repreſent the Motion of *Mercury*, with an Exactneſs equal to that of any of the other Planets; perhaps as near as the *Sun*'s Place by any Tables, or thoſe of the fixed Stars by any Catalogue yet extant.

It were to be wiſhed, that ſome good Obſervation, like this, had been made of the like Tranſit of *Mercury* at his other Node in *April*, where he was ſeen indeed *April* 23°, 1661, but ſo imperfectly, that neither Ingreſs nor Egreſs was any where obſerved; and, tho' it be certain, that he traverſed the *Sun* on *April* 26°, 1674; and again *April* 24, 1707, yet we were ſo unfortunate, that the Conjunction in both happened ſo near Midnight, that he eſcaped unſeen by all the Aſtronomers of *Europe*, excepting ſingly Mr. *Roemer* at *Copenhagen*, whoſe Obſervation I have lately received by the Favour of

Mr. *De l' Iſle* the Aſtronomer, communicated in the Words of the Manuſcript Journal of Obſervations of the ſaid Mr. *Roemer.* “*Hodie ſexto Maii* (*Anno* 1707) *hora matutina* 4^{h} 19′ *ſpectabatur Mercurius in extremo margine Solis jamjam exiturus*; *altus ſupra imum ſolis marginem* ¼ *diametri ſolaris,* & *ad ſiniſtram in Tubo* (*ſc. invertente.*) *Accuratius hæc determinare non licuit ob moram nimis brevem.*” It was great Pity, that he did not, at leaſt, eſtimate, how many Diameters of his Body he was diſtant from the Limb of the *Sun*, or what Part of a Diameter, if ſo near: But having examined this Obſervation, I find that the *Sun*, at that Time, was but juſt riſen, or rather riſing, and ſoon after entered into a Cloud, ſo that the Limb of the *Sun* could not be diſtinctly ſeen, it always undulating and ſparkling much, when ſo near the Horizon; in which Circumſtance, a juſt Obſervation could hardly be made.

Let us now ſee how our Numbers, corrected as above, will repreſent this Obſervation. *Anno* 1707, *April* 24° 16^{h} 19′ at *Copenhagen* is 15^{h} 28′, at *Greenwich*, but 15^{h} 24′ 20″ *T. æq.* To this Time, I find the *Sun*'s true Place ♉ 14° 50′ 1″, and his Diſtance from the *Earth* 101005. The correct *Epocha* of *Mercury*'s mean Motion, for the Year 1707, is 3^{s} 13° 18′ 45″, to which adding, for the reſt of the Time, 3^{s} 19° 9′ 28″, we ſhall have his middle Motion at the Time of the Obſervation ♏ 2° 28′ 13″; and, taking his *Aphelion* in ♐ 12° 49′ 49″ therefrom, we have his mean Anomaly 10^{s} 19° 38′ 24″, and thereby the Equation to be added 12° 39′ 41″, and thence the Place of *Mercury* in his Orb ♏ 15° 7′ 54″. But the correct Place of the deſcending Node is ♏ 14° 46′ 25″, and therefore *Mercury*, being 21′ 29″ paſt the Node, had 2′ 36″ South Latitude at the *Sun*; and his Place, reduced to the Ecliptick, was ♏ 15° 7′ 45″, that is, 17′ 44″ paſt the Conjunction of the *Sun*, which diminiſhed in the Proportion of 5567 to 4533, or of the Diſtance of the Planet from the *Earth* to his Diſtance from the *Sun*, becomes 14′ 27″; and by ſo much was he paſt the Conjunction as viewed from the *Earth*. Again, by the ſame Proportion, his Geocentrick Latitude, at that Time, was 2′ 7″ South; and therefore, his apparent Diſtance from the *Sun*'s Center, was 14′ 37″; that is, but 1′ 18″ from his weſtern Limb; ſo that he might well be ſaid to be, *jamjam exiturus*.

But, that *Mercury* ſhould at that Time be ſo far northerly, as Mr. *Roemer*'s Words import, was abſolutely impoſſible; and, I am apt to believe, that ſo acute an Aſtronomer as Mr. *Roemer* was, could not himſelf be the Obſerver, but ſome Perſon leſs acquainted with theſe Matters; which the Words *ſpectabatur Mercurius*, inſtead of *Mercurium vidi*, ſeem to import. If he had then had North Latitude, he muſt needs have been ſeen in the *Sun* in *April* 1720, which we are aſſured he was not.

Laſtly, it may not be amiſs to advertiſe, that on the laſt Day of *October* 1736, *Mercury* will again traverſe the northern Part of the *Sun*'s Disk, both Ingreſs and Egreſs being viſible to all *Europe*.

XXXIX. 2] *An Obſervation of* Mercury *at* Wittenberg. *Mar.* 5, 1729, *by Mr.* F. Fred. Weidler. Nº 412. *p.* 252.

March 4, 1729; the Planet *Mercury* was fartheſt from the Sun, and remained ſome Time above the Horizon. Making uſe therefore of a twenty-two Foot Teleſcope, Mr. *Weidler* obſerved its Phaſe almoſt biſected, and its Diameter appeared equal to a third Part of the Diameter of *Venus*, this Planet being above the Horizon, and ſeen at the ſame Time.

XL. 1] *Obſervations upon a Comet ſeen at* Berlin, *in* 1718, *by Mr.* Chriſt. Kirck. Nº. 375. *p.* 238.

Monere hic debeo obſervationes Cometæ à me inventi, in *Novis Literariis Lipſienſ.* non eſſe accuratas; nam die 23 Januarii mane, Cometa cum θ & φ Caſſiopeæ (non vero ♂ & φ) conſtruebat triangulum æquicrurum; & veſperi φ Perſei, Cometa & θ Caſſiopeæ ad ſenſum erant in linea recta. Pleniorem Cometæ hiſtoriam jam paratam habeo, ex qua hæc breviter attingam. Obſervavi eum a die 18 *Jan.* ad 5 *Februarii*. Loca ejus ex obſervationibus ad horam 10 veſpertinam cujuſque diei, quo Cometa obſervari potuit, reducta, hæc Tabella exhibet.

	Longitudo.			Latitudo.		
	°	′		°	′	
18 *Jan.*	27	26	♋	69	18	S.
21 *Jan.*	16	25½	♉	48	42	S
23 *Jan.*	9	28½	♉	39	45	S.
26 *Jan.*	5	25½	♉	32	55	S.
27 *Jan.*	4	41	♉	31	24	S.
28 *Jan.*	4	4	♉	30	13	S.
30 *Jan.*	3	4	♉	28	23½	S.
31 *Jan.*	2	43	♉	27	40	S.
1 *Feb.*	2	25	♉	27	1	S.
2 *Feb.*	2	10	♉	26	22	S.
5 *Feb.*	1	39	♉	24	53	S.

Via ejus tranſiit ſupra tergum Urſæ minoris, prope Polarem, per crura & genua Cephei, Caſſiopeæ & Andromedæ. Nodus ejus deſcendens fuit in 21½ gradu Arietis, cum aliqua mutatione: Angulus orbitæ cometicæ & Eclipticæ 69½ grad. circiter, etiam cum

aliqua variatione. Via Cometæ 2 fere gr. à Polo mundi tranſiit, & Æquatorem ſecavit in 20½ gr. à puncto æquinoctiali. Perigæum Cometæ fuit in 6° 6′ ♍. cum latitudine ſeptentrionali 62° 7′. Cometa in Perigæo fuit, *D.* 18 *Jan.* hor. 3. min. 9. mane. Motus Cometæ diurnus in orbita propria, in Perigæo (12 ſcilicet horis ante, & 12 poſt Perigæum) 22° 8′; ultimis vero diebus apparitionis 32′. Suppoſita Terra quieſcente, & Cometa in recta linea trajiciente, motus Cometæ fuit 391 partium, qualium diſtantia minima Cometæ à Terra 1000. De Parallaxi Cometæ nihil certi affirmare poſſum, niſi quod multum ſupra Lunam fuerit elevatus Cometa. Probabiliter vero conjicio, illum intra Planetarum orbes exſtitiſſe, imo in Perigæo multo propiorem nobis fuiſſe Martis Sphærâ. Sit enim ſemidiameter orbitæ Terræ 10000 partium, erit ita motus diurnus Martis 139 vel 140. Si vero Cometam in orbita Martis exſtitiſſe ſuppono, cum latitudine 62° 7′ & motu diurno 22° 8′ ejus velocitas eſſet 2847 partium, ſi ſcilicet ſimul fuiſſet in oppoſitione Solis; cum autem differentia Longitudinis Solis & Cometæ in Perigæo tantum fuerit 141° 40′, motus diurnus Cometæ evadit 3200 part. & proportio motus Cometæ ad motum Martis ut 23 ad 1. Quare colligo Cometam intra ſphæram Martis exſtitiſſe. Si vero quis Cometam ad Saturni orbitam evehere vellet, deberet ipſi velocitatem tribuere, quæ eſſet ad velocitatem Saturni ut 600 ad 1; & quod uno die majus ſpatium percurriſſet, quam Terra dimidio anno abſolvere ſoleat. Ne dicam de diametro Cometæ, quæ non multo minor exiſtere debuiſſet tribus diametris Solis.

Comparationem inſtitui hujus Cometæ cum aliis, & invenio Cometam, quem *Regiomontanus* anno 1472 vel 1475. menſe *Jan.* & *Febr.* obſervavit, viam tenuiſſe non multo diverſam à via noſtri Cometæ; tranſiit enim per Urſam minorem & Cephei femora, per pectus vel collum Caſſiopeæ & cingulum Andromedæ; ac velocitas ejus maxima uno die fuit 40 grad. Anno 1556, alius Cometa eſt obſervatus, cujus Nodos *Camerarius* in 11° ♎ & ♈ ponit, & qui prope pedes Urſæ minoris, per Cepheum, ſupra Caſſiopeam, & per partes ſuperiores Andromedæ tranſiit, motu valde veloci in Perigæo. Quod ſi *Regiomontanus* Cometam anno 1475 obſervavit, (de quo tamen Aſtronomi valde dubitant) admirabilis eſſet convenientia inter hoſce tres Cometas: intervallum enim prioris à medio eſſet 81 annorum, & à medio Cometâ ad ultimum 162 ann. ut ita revolutio Cometæ poſſet eſſet 81 annorum; nec etiam Hiſtoria aliorum Cometarum hiſce male reſponderet.

The ſmall Comet which was ſeen in theſe Parts of *Europe*, in the Months of *October*, *November*, and *December*, 1723. was firſt obſerved in *England* by Dr. *Halley*, on *October* 9. between 7 and 8 of the Clock in the Evening; it appearing then to the naked Eye not much unlike a Star of the third Magnitude. Looking at it through a Te-

2] *Obſervations upon a Comet ſeen* 1723, *by the Rev. Mr.* Bradley. N° 382 *p.* 41.

a Teleſcope, he ſaw ſome ſmall Teleſcopical Stars near it, whoſe Situation he noted together with the Comet's, in order to ſee which way it tended. About 9 he again viewed the Comet, and found it conſiderably moved from its former Station, having now paſſed a ſmall Star, which at the time of the firſt Obſervation was on the other ſide of it. Comparing the two Situations of the Comet together, he perceived that its apparent Motion at that time was about 8 or 9 Minutes in an Hour, in a Direction towards *Sagitta*; and that the Comet paſſed very near, if it did not wholly eclipſe the forementioned ſmall Star, whoſe place he afterwards found to be in ♒ 7° 22′ 15″ with 5° 2′ N. Latitude. From the Situation of the Comet at the time of the firſt Obſervation, he judged that it was in Conjunction with the Star at 8 *h*. 5′. equal Time. Note that the equal, and not the apparent Time, is likewiſe made uſe of in all the following Obſervations.

The next Day he was pleaſed to communicate to me the Subſtance of what he had obſerved, whereby I was enabled, the Night following, to ſee the Comet at *Wanſted*. The Clouds hindered me from obſerving it in the manner that I had deſigned; but I had Time enough to meaſure its Diſtance (with a Micrometer in a Teleſcope of 7 Foot) from a Star in *Aquarius*, marked ε by *Bayer*. At 6 *h*. 21′ the obſerved Diſtance between this Star and the Comet was 1° 13′ 53″, and a great Circle paſſing through the Star and Comet, made an Angle with the Vertical Circle of 60°¼. The Comet was more ſoutherly and weſterly than the Star. By this Obſervation the Comet preceded the Star in Right Aſcenſion 1° 3′ 50″ being 39′ 5″ more Southerly; ſo that the Comet's Right Aſcenſion was 307° 6′ 40″ and its Declination 11° 8′ 15″ S.

The Place of ε here aſſumed is according to the *Britiſh* Catalogue, as are alſo the Places of the other Stars hereafter mentioned from which the Comet was obſerved. The Right Aſcenſions and Declinations, which are here ſet down, of ſeveral ſmall Stars that are not in that Catalogue, were determined by obſerving the Differences of Right Aſcenſion and Declination between thoſe ſmall Stars and others that were in the Catalogue, and had nearly the ſame Declinations.

The ſame Evening, at 7 *h*. 3′ a ſmall Star that was more eaſterly than the Comet, and had about the ſame Declination with it, was diſtant from it 35′ 40″. About the ſame time another ſmall Star that had nearly the ſame Right Aſcenſion with the Comet, but was more ſoutherly, was diſtant from it 39′ 58″. The Places of theſe two Stars I have not yet obſerved.

The next Night proved cloudy, ſo that I could not ſee the Comet again till *October* 12. when the Air being very ſerene and clear we had an Opportunity of comparing it with two or three ſmall Stars that were near it; my Uncle, the Reverend Mr. *Pound*, aſſiſting in this and moſt of the following Nights Obſervations.

At 7 *h.* 22' a ſmall Star, whoſe Right Aſcenſion was found 304° 40' 23" and its Declination 7° 8' 22" S. preceded the Comet in Right Aſcenſion 26' 21" being 10' 42" more Northerly. Hence the Comet's Right Aſcenſion was 305° 6' 44" and its Declination 7° 19' 4" S.

At 8 *h.* 50' the Comet was in the ſame Parallel of Declination with another ſmall Star, whoſe Right Aſcenſion was found 305° 9' 56" and its Declination 7° 13' 20" S. and preceded the ſaid Star 6' 20" in Right Aſcenſion. Hence the Right Aſcenſion of the Comet was 305° 3' 36" and its Declination 7° 13' 20" S. Theſe Obſervations were made with a Teleſcope of 15 Foot furniſhed with a Micrometer, as were alſo all thoſe of the following Nights.

The next Night, *October* 13. 6^h. 58' the Comet followed a ſmall Star, 4' 10" in Right Aſcenſion, being more Northerly than the Star 11' 45". The Clouds did not permit us to obſerve the Place of this Star; but its Right Aſcenſion muſt be about 304° 22' and its Declination 6° 10' S.

October 14. the Comet was near two Stars which are the 66th and 67th of *Aquila* and *Antinous* in the *Britiſh* Catalogue, and at 8^h 57' it followed the ſouthermoſt of them 20' 37" in Right Aſcenſion, being 29' 8" more ſoutherly. Hence the Comet's Right Aſcenſion was 303° 49' 10" and its Declination 4° 43' 54" S.

October 15. 6^h 35' the Comet preceded the northermoſt of the ſaid Stars 23' 6" in Right Aſcenſion, being more ſoutherly than the Star 4' 15". Hence the Right Aſcenſion of the Comet was 303° 24' 40". its Declination 3° 51' 3" S.

October 21. 6^h 22' a ſmall Star, whoſe Right Aſcenſion was found 301° 7' 17", and its Declination 0° 11' 50" S. preceded the Comet 41' 6" in Right Aſcenſion, being 5' 50" more ſoutherly. Hence the Comet's Right Aſcenſion was 301° 48' 23" and its Declination 0° 6' 0" S.

October 22. 6^h 24' a ſmall Star, whoſe Right Aſcenſion was found 301° 59' 47" and it Declination 0' 32' 43" N. followed the Comet ½ a Minute in Right Aſcenſion, being 13' 43" more northerly. Hence the Comet's Right Aſcenſion, was 301° 39' 17" and its Declination 0° 19' 0" N.

October 24. 8^h 2' a ſmall Star, whoſe Right Aſcenſion was found 301° 24' 57" and its Declination 1° 9' 22" N. preceded the Comet 0' 37" in Right Aſcenſion, being 5' 12" more Northerly. Hence the Comet's Right Aſcenſion was 301° 25' 34"; and its Declination 1° 4' 10" N.

October 29. 8^h 56' a ſmall Star, whoſe Right Aſcenſion was found 301° 6' 20" and its Declination 2° 51' 0" N. preceded the Comet one Minute in Right Aſcenſion, being 23' 40" more Northerly. Hence the Comet's Right Aſcenſion was 301° 7' 20" and its Declination 2° 27' 20" N.

October 30. 6ʰ 20′. The ſame Star had exactly the ſame Right Aſcenſion with the Comet, being 11′ 33″ more Northerly. Hence the Comet's Right Aſcenſion was 301° 6′ 20″ and its Declination 2° 39′ 27″ N.

November 5. 5ʰ 53′ a ſmall Star, whoſe Right Aſcenſion was found 300° 35′ 00″ and its Declination 3° 45′ 30″ N. preceded the Comet 33′ 0″ in Right Aſcenſion, being 2′ 8″ more Southerly. Hence the Comet's Right Aſcenſion was 301° 8′ 0″ and its Declination 3° 47′ 38″ N.

November 8. 7ʰ 6′ a bright Star (placed by *Hevelius* in *Roſtro Aquilæ*, but not inſerted in the *Britiſh* Catalogue) whoſe Right Aſcenſion at this time was found 302° 21′ 30″ and its Declination 4° 28′ 40″ N. followed the Comet 1° 7′ 40″ in Right Aſcenſion, being 13′ 3″ more Northerly. Hence the Comet's Right Aſcenſion was 301° 13′ 50″ and its Declination 4° 15′ 37″ N.

November 14. 6ʰ 20′ a Star, whoſe Right Aſcenſion was found 301° 27′ 10″ and its Declination 4° 59′ 40″ N. preceded the Comet 5′ 35″ in Right Aſcenſion, being 5′ 50″ more Southerly. Hence the Comet's Right Aſcenſion was 301° 32′ 45″ and its Declination 5° 5′ 30″ N.

This was the laſt Time that I obſerved the Place of the Comet 'till after the Full Moon; my Affairs calling me to *Oxford*, where I had no Convenience for making ſuch Obſervations.

Dr. *Halley* and Mr. *Graham* continued to obſerve the Comet 'till *November* 20; and according to both their Obſervations that Evening at 7ʰ 45′ the Comet followed β in *Collo Aquilæ* 6° 33′ 55″ in Right Aſcenſion, being about 4′ more Northerly than the Star. Hence the Comet's Right Aſcenſion was 301° 59′ 50″ and its Declination 5° 48′ 55″ N.

The Light of the Moon daily increaſing, prevented them from making any more Obſervations, the Comet being by this time grown ſo faint, as to become in a manner imperceptible while the Moon ſhone bright. And the faint Appearance which it made before the Moon obſtructed the Sight of it, gave little Hopes of its being to be ſeen again after the Full Moon. Notwithſtanding which on *December* 3. (being then near *Cirenceſter* in *Glouceſterſhire*) I was tempted by the Serenity of the Evening, and the Uſe of a very good Teleſcope of 10 Foot, to look for it again before the Moon roſe; and I found it among ſome ſmall Teleſcopical Stars; but it appear'd ſo faint and dull, as made it doubtful, whether what I took for the Comet might not be a ſmall Star with a little Hazineſs about it. But this Doubt was cleared two Nights after; when I perceived that the Comet was moved from its former Situation, towards a bright Teleſcopical Star, from which I afterwards took its Difference of Right Aſcenſion and Declination, upon my Return to *Wanſted*, on *Dec.* 7. This Star's Right Aſcenſion was then found 303° 39′ 20″

and its Declination 7° 32′ 30″ N. And *Decemb.* 7. 6h 45′ the Comet followed it 3′ 15″ in Right Aſcenſion, being 14′ more Northerly than the Star. Hence the Comet's Right Aſcenſion was 303° 42′ 35″ and its Declination 7° 46′ 30″ N.

This was the laſt Night that I ſaw the Comet, though I believe I might have continued to have obſerved it, had not an uninterrupted Succeſſion of cloudy Evenings prevented ſo long, that it became uncertain where to look for it.

The forementioned Obſervations are the Principal of all that were made at *Wanſted*; and moſt of them being taken from Stars which are not in the *Britiſh* Catalogue, whoſe Places therefore are here determined, only by comparing them with ſome that were; it cannot be ſuppoſed that the Comet's Places deduced from them are altogether exact. For which Reaſon I have all along ſet down, not only the Place of the Comet and Star where it was known, but alſo the Particulars of the Obſervation, that if any hereafter ſhould be willing to examine the Track of this Comet more nicely, they may know where to find the Stars from which it was obſerved. The Places of the Stars here ſet down are abundantly ſufficient for that Purpoſe, as will appear from the following Table, which contains the Longitudes and Latitudes of the Comet deduced from the foregoing Obſervations, together with the Places of the Comet calculated from the Theory of Gravity, for the Times of Obſervation on the ſeveral Days therein mentioned, as alſo the Differences between the obſerved and computed Places. Thoſe Differences not exceeding one Minute, ſhew that the Obſervations are not only conſonant to each other, but that the Places of the Stars are likwiſe near the Truth, ſince the Comets Places deduced from them are found all along to agree ſufficiently near with the Theory of Gravity; the Truth of which having long ſince been eſtabliſh'd by its great Author Sir *Iſaac Newton*, and my worthy Collegue Dr. *Halley*, needs not the Confirmation of ſo ſhort a Series of Obſervations as was made of this Comet. But ſhort as it is, I preſume 'twill be no eaſy Matter to account for the Obſervations with the ſame Degree of Exactneſs any other way, than by that Theory, according to which the following Computations are made.

1723. Temp. Æquat				Comet. Long. Obſervat.				Lat. Bor. Obſerv.			Comet. Long. Comput.				Lat. Bor. Comput.			Differ. Long.	Differ. Latit.
	D.	H.	′		°	′	″	°	′	″		°	′	″	°	′	″	″	″
Octob.	9	8	5	♒	7	22	15	5	2	0	♒	7	21	26	5	2	47	+ 49	− 47
	10	6	21		6	41	12	7	44	13		6	41	42	7	43	18	− 30	+ 55
	12	7	22		5	39	58	11	55	0		5	40	19	11	54	55	− 21	+ 5
	14	8	57		4	59	49	14	43	50		5	0	37	14	44	1	− 48	− 11
	15	6	35		4	47	41	15	40	51		4	47	45	15	40	55	− 4	− 4
	21	6	22		4	2	32	19	41	49		4	2	21	19	42	3	+ 11	− 14
	22	6	24		3	59	2	20	8	12		3	59	10	20	8	17	− 8	− 5
	24	8	2		3	55	29	20	55	18		3	55	11	20	55	9	+ 18	+ 9
	29	8	56		3	56	17	22	20	27		3	56	42	22	20	10	− 25	+ 17
	30	6	20		3	58	9	22	32	28		3	58	17	22	32	12	− 8	+ 16
Nov.	5	5	53		4	16	30	23	38	33		4	16	23	23	38	7	+ 7	+ 26
	8	7	6		4	29	36	24	4	30		4	29	54	24	4	40	− 18	− 10
	14	6	20		5	2	16	24	48	46		5	2	51	24	48	16	− 35	+ 30
	20	7	45		5	42	20	25	24	45		5	43	13	25	25	17	− 53	− 32
Dec.	7	6	45	♒	8	4	13	26	54	18	♒	8	3	55	26	53	42	+ 18	+ 36

In order to determine the Orbit of this Comet, I ſuppoſed it to deſcribe a *Parabola* agreeable to what is delivered in the third Book of Sir *Iſaac Newton*'s *Princip. Math.* and then I found the Inclination of the Planes of the Orbit and Ecliptick 49° 59′. The Place of the Aſcending Node ♈ 14° 16′. The Place of the Perihelion ♉ 12° 52′ 20″. The Diſtance of the Perihelion from the Node 28° 36′ 20″. The Logarithm of the Perihelion diſtance 9.999414. The Logarithm of the Diurnal Motion 9.961007. The Time of the Comet's being in its Perihelion, *Sept.* 16. 16^h 10′ equal Time. In its Orbit thus ſituated, the Motion of the Comet was Retrograde or contrary to the Order of the Signs.

From theſe Elements, by the Help of Dr. *Halley*'s general Table for Comets (to which they are adapted) I computed the Places in the foregoing Table; which agreeing with the obſerved Places as near as the Obſervations themſelves agree one with another, ſhew that it would be a vain Attempt to pretend to determine the true Ellipſe in which this Comet moves, or its Periodical Revolution, from ſo ſmall a Part of its Orbit as that was, which it deſcribed between the firſt and laſt of the foregoing Obſervations; this therefore muſt be left to Poſterity, eſpecially ſince it is certain, that this Comet is not one of thoſe of which Obſervations have hitherto been tranſmitted to us, ſufficient to determine the Situation of their Orbits.

The *Nucleus* of this Comet was very little, for it appeared but of a ſmall Diameter when I firſt ſaw it, although it was then above three times nearer to the Earth, than the Sun is at its mean Diſtance. Its Tail was then hardly diſcernable with the naked Eye, but thro' a Teleſcope one might perceive a faint Light extending itſelf above a Degree from the Body

I have not yet heard that this Comet was ſeen before *October* 6. although it was in a proper Situation to have been obſerved in the Morning, moſt part of *September*, eſpecially from the Time it was in its Perihelion, 'till near the End of that Month. For about that Time it croſſed the Milky-way between the Maſt of the Ship and the Head of the great Dog, paſſing between the bright Stars in the Body and Tail of the great Dog, towards the Head of the Dove, where it was about *September* 29. being by that time got ſo far towards the South Pole, as not to riſe above our Horizon. From thence it paſſed under the Tail of *Xiphias* within about 15° of the South Pole of the Ecliptick; and moving on between the Head of *Hydrus* and the bright Star in *Eridanus* called *Acharnar*, it went by the Stars in the Body and Neck of the Crane about *October* 5. when it came again above our Horizon. From hence paſſing under the Tail of the Southern Fiſh, and between the Stars in the Shoulder of *Capricorn*, it croſſed the Ecliptick, *October* 8. in about 8° ½ of *Aquarius*. From thence it moved on by the Hands of *Aquarius* and *Antinous* towards the Head of the *Eagle*, according to its Courſe before deſcribed.

The Comet was in Oppoſition to the Sun *October* 1. when it had near 74° Southern Latitude, and altered its Longitude two Signs in a Day. About *October* 3. it was in its Perigæon, or neareſt Diſtance to the Earth, being then almoſt ten times nearer to it than the Sun is at its mean Diſtance; and its apparent Motion was then about 20° in a Day, and when I laſt ſaw it, 'twas above twice as far off as the Sun.

2] *Obſervations of the ſame at* Witham *in* Eſſex, *by the Rt. Hon. the Lord* Paiſley. Nº 382. *p.* 50.

His Lordſhip being at *Witham* in *Eſſex*, where he had the Advantage of a very clear Sky, firſt diſcovered this Comet on *Friday* the 11th of *October* laſt about 7 in the Evening; it then appeared not much unlike a Star of between the 4th and 5th Magnitudes, but a Hazineſs round the Head, and ſome Light ſtreaming from it on that Side that was oppoſite to the Sun, induced him immediately to look upon it as a ſmall Comet; which his Obſervation the next Evening abundantly ſatisfy'd him of. His Lordſhip was very particular in the Notice he took of its Appearance, and was pleaſed to communicate the three curious annexed Figures [*Fig.* 109, 110, 111.] of it, repreſenting it on three ſeveral Nights, *viz.* the 11th, 13th and 15th of the ſame Month; ſome time after which the Tail became ſo inconſiderable as hardly to deſerve any farther Deſcription; as will be readily judged from the Decreaſe of it between the 11th and 15th Days of the Month. The Tail was viſible on the 11th to near a Degree's Diſtance from the Body, as his Lordſhip found by comparing it with ſome known Diſtances in the Heavens; it was of a duſky Light not unlike a Cloud growing darker and darker towards its Extremity, as is expreſs'd in the firſt Figure, where, as well

Fig. 109. 110, 111.

as in the two following, the white Speck in the Head is intended to expreſs the Brightneſs of a ſmall Star; from the Compariſon of which with the Tail the Brightneſs of the latter may in ſome ſort be collected: The Tail appear'd ſharper, and not ſo much ſpread in the two following Obſervations, and in the laſt did not exceed one third Part of the firſt Length; it was then of a much darker Colour, which made the Difference between that and the Head more obſervable, the Head yet appearing ſufficiently bright. For ſome following Nights his Lordſhip's Obſervations were interrupted by cloudy Weather, after which the Comet was ſo far diminiſh'd, as only to be known by its Motion, its Appearance being no ways diſtinguiſhable from that of a ſmall nebuloſe Star.

3] *The ſame obſerved at* Albano, *by* Sig. Fr. Bianchini, *and at* Lisbon, *by* F. Carbone *and* Domin. Capaſſo. Nº 382. *p.* 51.

Fig. 112.

Die 17 *Octobris*, poſtquam Jovialium Comitum ſitum obſervâſſem fortè in Conſtellationem *Capricorni* oculos conjeci; cumque aſtra ſingula percurrerem, in quandam veluti nebuloſam ſtellam incidi, cæteris ſane grandiorem, quam tamen ibidem loci nunquam antea obſervâram. Rei novitatem perſcrutaturus, eo Teleſcopium direxi, ſtatimque Cometen eſſe deprehendi; ſiquidem tenuiſſimæ nebulæ globus apparuit, ejuſque in medio veluti lucidus nucleolus. Idem quoque nudis oculis diſcernere licuit; & præter nebulam, ſeu Cometæ atmoſphæram, brevem quoque caudam, quæ ad orientem vergebat, eratque hujuſmodi. [*Fig.* 112.]

Ne me igitur ea occaſio præteriret, conſuetas circa illum obſervationes inſtitui, ut ejus Longitudinem, Latitudinem, propriumque motum deprehenderem.

Et quidem prima nocte, (die nempe 17 ſupradicta,) tranſiit per Meridianum (qui penè cum Romano coincidit) circa horam ſeptimam 44′ poſt Merid. ejuſque diſtantia à Zenith, 69° 29′.

Hora 8. 11′ 30″ diſtantia Cometæ à *Fomabant Aquarii*, intercepta eſt, 20° 33′ & hora 8. 17′ 30″ diſtabat à Stella β in humero dextro *Aquarii*, 21° 8′. Proindeque verſabatur Cometes in 11° 54′ *Aquarii*, cum Latitudine Auſtrali ab Ecliptica 11° 10′ circiter.

Fig. 113.

Die 21. erat adeo proximus Stellæ ι in Lino ſupra manum ſiniſtram *Aquarii*, quam ipſa ι eſt proxima Sellæ μ minori in eodem Lino, conſtituebatque Cometes cum utraque Stella ι, μ rectam Lineam, ſic [*Fig.* 113.] Ex hac igitur obſervatione, & ex Aſcenſionis rectæ nec non declinationis differentiâ inter Cometen, & ſupradictam Stellam ι quam diligentiſſime obſervavi, infertur locus Cometæ fuiſſe in 6° 45′ *Aquarii* cum Latitudine Boreali ab Eclipt. 8° 5′.

Hinc etiam infertur qualis Cometæ motus proprius fuerit, & quale iter; per planum ſcilicet circuli maximi ſecantis Eclipticam in gradu 9 *Aquarii*, & conſtituentis cum eadem Ecliptica angulum 80 graduum circiter.

Reliquis diebus eadem ſemper proportione movebatur, magiſque in dies elongari à terra viſus eſt.

Parallaxim nullam ſenſibilem, etſi pluries intentaverim, deprehendere potui; proindeque maxima ejus diſtantia à terra credenda eſt.

Hactenus Illuſtriſſimus Dominus *Franciſcus Bianchini* in Mathematicis Scientiis apprimè eruditus, & in obſervando, quoad noverim, accuratiſſimus. Ejus obſervatio à noſtra, mea ſcilicet, ac Prioris Dominici *Capaſſi* vix in uno aut altero minuto quoad latitudinem diſcrepavit, cæterùm omninò conformis. Quapropter nec illam hic arbitror apponendam.

4] *The ſame obſerved at* Bombay, *by Mr.* William Saunderſon. Nº 397. *p.* 213.

In the Month of *October* 1723, being riding at *Bombay*, a Brightneſs in the Heavens appeared in a Right Line, (or but very little to the Eaſtward of one) with *Lyra* and the Bright Star in the *Eagle*, being about 50° diſtant from the laſt; and on *Monday* the 7th following it had advanced 10° toward the *Eagle*, moving towards it in the forementioned Direction, from the S. E. Quarter. I took the following Diſtances between 9 and 10 at Night, as in this Table.

October.		Diſt. from the *Eagle*'s Heart.		
Days.		°	′	
1	7	40	00	South
2	10	23	50	
3	11	20	30	
4	13	17	40	
5	15	14	40	
6	19	11	40	

At firſt it looked only like one of the White Spots called the *Magellanic Clouds*, the Space filling the Field of a Six-foot Glaſs. Afterwards I ſaw the Head in the Center of the illuminated Space, which did not look with much Brightneſs; but appeared largeſt on the 10th of *October*, decreaſing gradually both in its Bulk and Motion from that time until the 25th, at which time I could find no Appearance of it with the forementioned Glaſs. *N. B.* From the 20th to the 25th it had nearly the ſame Place in the Heavens, ſeeming to move directly from the Earth.

5] *An Account of a Comet ſeen* Feb. 29, 173½, *by Mr.* J. Dove. Nº 425. *p.* 393.

The 29th of *February*, at about half an Hour paſt ten at Night, I judge (having a good Obſervation at Noon) we were in Lat. 34° 28′ South, and Long. 12° 35′ Weſt from Cape *Bonne Eſperance*, the Moon ſhining very bright, being near the Full, we ſaw ſomething very bright riſe about Weſt, which I judge to be a Comet: It ſet about Eaſt, paſſing from Weſt to Eaſt in about five Minutes, between the Moon and our Zenith, and to the Southward of *Spica Virginis*; it carried a Stream of Light after it about 40° long, and 1° or 1° $\frac{1}{2}$ broad; the Brightneſs of the Moon outſhined the Comet as it came near it.

XLII. *An Account of a Book entitled*, Heſperi & Phoſphori nova Phænomena, *&c. auctore* Fran. Blanchino, *by* J. Hadley, *Eſq*; V. P. N° 410. *p.* 158.

The Deſign of this Treatiſe, is to give an Account of ſome new Aſtronomical Diſcoveries relating to the Planet *Venus*, which the Author diſpoſes under four Heads; *viz.*

1. The Deſcription of the duſky Spots obſerved in her Diſk.
2. Her Rotation round an Axis, the Poſition of which is determined by the apparent Motion of thoſe Spots, together with the Time of her Revolution.
3. The Paralleliſm of that Axis to itſelf in all Parts of the Planet's Orbit.
4. Obſervations in order to determine the Horizontal Parallax of *Venus*, and conſequently thoſe of the Sun and other Planets.

He takes Notice of five remarkable Sports in her whole Surface, the two ſmalleſt of which are placed, one near each Pole, the other three lie along the Æquator, and cover good Part of a Zone, extended to about 30 Deg. of Latitude on each Side. He repreſents them to be much like the larger dark Spots in the Moon, which are uſually called *Seas*, but conſiderably fainter, ſo as not to be eaſily diſcernable even to a ſharp-ſighted Obſerver, without the Aſſiſtance of a Teleſcope, capable of repreſenting diſtinctly the Planet under an Angle equal at leaſt to that under which the Moon appears to the naked Eye, and with an Aperture of 3 or 4 Inches of the *Roman* Palm. He then proceeds to give the Deſcription of a Machine contrived by him to repreſent to the Sight the Motion of the Earth and *Venus* in their Orbits, and by the Means of a Lamp placed in the Center, to ſhew the Phaſes of the Planet, and Appearance of the Curve Lines deſcribed by the Revolutions of the Spots round the Axis.

This Revolution he makes greatly different from thoſe of the *Earth* and *Mars* (the two Bodies next in order of the Planetary Syſtem) both in the Poſition of the Axis and Time of the Period. He places the *Colurus Solſtitiorum*, or Plane paſſing through the Axis of the Planet and Tropical Points of its Orbit, about the 20th Degree of *Leo* and *Aquarius*, and gives the Planes of its Æquator and Ecliptick an Inclination to each other of about 75 Degrees. He determines the Time of the Revolution to be about 24 Days and 8 Hours, inſtead of 23 Hours, as it has been generally taken to be from ſome Obſervations made by Mr. *Caſſini* in the Year 1666 and 1667, but which he himſelf did not ſeem much to rely on. Now, both theſe Periods may be very conſiſtent with the ſame Obſervations, provided that one of the Obſervers did not continue his Obſervations for any conſiderable Time at once. For if the exact Situation of any Spot be obſerved at any given Hour one Day, and at the ſame Hour the ſucceeding Day be found advanced about 15 Degrees or $\frac{1}{24}$ of the whole Revolution, it may ſtill remain doubtful, whether the Spot has moved only through thoſe 15 Deg. in that Day, or has made one or more entire Revolutions beſides in that Time. This the Author was aware of, and therefore waited for an Opportunity of attending to the Motion of a Spot as long at once as the Vicinity of *Venus* to the Sun would admit of. Accordingly, *Feb.* 26, 1726. a little after Sun-ſet, he obſerved a Spot near the Center of her Diſk, where its Motion is moſt perceptible in a ſhort Time, and about 3 Hours after, perceived the ſame Spot not ſenſibly removed; from which he concluded, the Period of its Revolution could not be ſo ſhort as one Day, ſince, if it were ſo, the Change of Place of the Spot muſt have been very ſenſible in that Time. It were to be wiſhed the Author had had Opportunities of confirming this Period by more Obſervations, eſpecially ſince it was neceſſary to begin them ſoon after Sun-ſet, and continue them till *Venus* was near the Horizon; the Strength of the Twilight in the firſt Caſe, and the Thickneſs of the Atmoſphere through which the Planet muſt be ſeen in the latter, rendring the Obſervations very difficult.

The next Article of his Obſervations, is the Continuance of the Axis in the ſame Paralleliſm, through the whole Orbit of the Planet. This is ſo neceſſary and obvious a Conſequence of the eſtabliſhed Laws of Motion, that there needs no more to be ſaid about it.

The 4th Article contains an Account of ſome Obſervations made to determine the Parallax of *Venus* in the Year 1716. The Method he uſed for this Purpoſe, was to take the ſeveral Diſtances of Time between the Appulſe of the Limb of *Venus* and of *Regulus* (which Star ſhe paſs'd by about that Time) to a horary Circle very near the Meridian, and to another about 6 Hours after, which he meaſured by the Pulſes of a Watch, of which 143 went to 1 firſt Minute of Time. He likewiſe obſerved the Alteration of thoſe Diſtances taken at the ſame Hour ſeveral Days one after another, and allowing a proportional Alteration for the Time between the two Obſervations, he computed what the Difference of their right Aſcenſion ought to have been in the latter of them, if there were no Parallax; then comparing this Difference with that obſerved, he concluded the Diſagreement to be the Parallax of right Aſcenſion. This Method the Author ſeems to depend on ſo much, as to think that an equal Degree of Exactneſs is hardly to be expected from any other hitherto practiſed: But if we conſider that the whole Parallax of right Aſcenſion amounts by his Obſervations to no more than 4 Pulſes of his Watch, and that he allows a Poſſibility of an Error near one of thoſe Pulſes in taking each of the Tranſits, it is evident that if ſuch an Error be actually committed in each of the Obſervations on which the finding of the Parallax depends, and all of them happen to conſpire the ſame Way, the Reſult of all together may poſſibly be greater than the whole Parallax found. Upon the whole, he makes the Horizontal Parallax of *Venus* at that Time to have been 24″ 20‴, and that of the Sun 14″ 18‴; but as he takes no Notice of the Latitude of the Place in deducing the Horizontal Parallax from that of right Aſcenſion, they both ought to be encreaſed on that Account by about $\frac{1}{3}$, or in Proportion of 3 to 4. If therefore there be no other Miſtake in his Numbers, the Horizontal Parallax of the Sun, as deduced from his Obſervations, ſhould be about 19″.

He concludes with giving ſome Cautions to thoſe who may attempt hereafter to repeat theſe Obſervations, both in Regard to the Time proper for it, and the Choice and Conſtitution of the Teleſcope to be made uſe of. For greater Eaſe of the Obſerver, there is at the End a double Table, containing the Heliocentric and Geocentric Motions of *Venus* for eight Years; after which Space of Time, the Earth and *Venus* return very nearly to the ſame Situation.

For a Teleſcope of 100 *Roman* Palms he allows an Aperture of 3 or 4 Inches of that Palm, with an Eye-glaſs whoſe focal Length may be from 7 to 11 of the ſame, but what he directs in longer Inſtruments to increaſe the Breadth of the Aperture and focal Length of the Eye-glaſs in the ſame Proportion with the Inſtrument, muſt certainly be the Effect of ſome Miſtake: For in this Caſe, a longer Teleſcope will magnify no more than the ſhorter, but only have the Strength of Light in the Object encreaſed in Proportion to the Square of the Length.

An Account of Books and Papers omitted.

N° 389. *p.* 350. 1. *Hiſtoria Cæleſtis Britannica*, tribus voluminibus contenta, Auctore Joanne Flamſteedio Aſtronomo Regio.

N° 419. *p.* 109. 2. A Catalogue of the Eclipſes of the 4 *Satellites of Jupiter* for the Year 1732. by *James Hodgſon, F. R. S.* and Maſter of the Royal Mathematical School at *Chriſt's Hoſpital, London.*

CHAP. IV.

SURVEYING.

A New Contrivance for taking Levels, by the Rev. J. T. Deſaguliers, *L.L.D. F. R. S.* Nº 385. *p.* 165.

THAT the Air Thermometer is alſo a Barometer, has been obſerved long ago; and, becauſe the Liquor in it will riſe and fall, as well by the Change of the Weight of the Air, as by the Air's Rarefaction by the Heat and Cold, this Inſtrument has no longer been made uſe of as a Thermometer, and, in its ſtead, Spirit of Wine Thermometers, hermetically ſealed, have, been uſed ever ſince.

But, becauſe the Errors of the Air Thermometer (or its Difference from the Spirit Thermometer) depend only upon the Change of the Weight of the Atmoſphere from what it was, when the two Thermometers were ſet at the ſame Degree of their reſpective Scales; the late Dr. *Hook* contrived an Inſtrument, that he called a Marine Barometer, made of a Combination of the two abovementioned Thermometers; in ſuch Manner, that a third Scale being made uſe of, to obſerve the Difference of the two Thermometers, thereby the Change of the Air's Gravity, and conſequently Storms, Rains, and fair Weather, might be foretold at Sea, where the Quickſiver Barometer becomes uſeleſs by the ſhaking of the Ship.

Dr. *Halley*, has in the *Philoſophical Tranſactions*, propoſed Mr. *Patrick*'s pendent Barometer for taking the Level of diſtant Places, becauſe the *Mercury*, in the Tube of the ſaid Barometer, does ſometimes riſe and fall a Foot, or a Foot and a Half; if therefore the Motion of the *Mercury* in this Barometer, be five times more ſenſible than in the common one, $\frac{1}{10}$ of an Inch of Fall of the *Mercury*, will anſwer to an Height of 18 Feet; and therefore ſuch an Inſtrument might be of Uſe in taking the Levels of diſtant Places. But I know by many Experiments, that this will not anſwer in Practice; becauſe as the Tube of ſuch a Barometer is of a very ſmall Bore, the Attraction of Coheſion, whereby the *Mercury* is apt to adhere to the Tube, will diſturb the Motion of the *Mercury* cauſed by the different Preſſure of the Atmoſphere; ſo that ſetting up this Barometer ſeveral Times ſucceſſively in the ſame Place, it will often differ a Tenth of an Inch, or more; and if it be ſhaken, as is commonly done to ſet it right, the *Mercury* will ſometimes part, and a Drop of it fall from the reſt; ſo that it is leſs to be depended upon for this Uſe, than the common Barometer.

Mr. *Stephen Gray* has often made a very ſenſible Barometer. Into a Bottle C B, he fixes a Tube A B, of a very ſmall Bore, open

Fig. 114.

at both Ends, and cemented tight to the Neck of the Bottle at C; then having warmed the Bottle with the Hand to drive ſome of the Air out of it, he immerges the End A into Water, tinged with *Cochineal*, ſo that as the Air cools in the Bottle CB, ſome of the red Water is forced into the Bottle; then ſetting the Bottle upright again, the Liquor in the Bottle will ſtand at B, (above the End of the Tube) and that in the Tube at D; but if it ſhould ſtand higher or lower than D, it may be brought to that Place by ſucking or blowing at A. If the Inſtrument, thus prepared, be firſt ſet on the Ground, and a ſpringing Ring of fine Wire ſlipped on the Tube down to D, by Way of Index, and then ſet upon any Table, or other Place, ſcarce a Yard higher, one may obſerve that the Liquor is riſen ſenſibly. I have ſeen it riſe a Quarter of an Inch, when the Bottle was ſet but a Yard higher than where it ſtood before; ſo that the Column of Atmoſphere, that preſſed down the Tube, whilſt the Machine was on the Ground, being ſhortened only three Feet, was ſo overbalanced by the Expanſion of the Air in the Bottle at B, that the Liquor roſe a Tenth of an Inch above D. There is, indeed, a great Uncertainty in this Inſtrument; for ſince it is a Thermometer, as well as a Barometer, the Warmth of the Hand that touches it, or even comes near it, will make it riſe, if the Air in the Bottle was cold before. Mr. *Gray* therefore contrived to put the Bottle CB, into the Veſſel FE, which he filled with Sand; that in raiſing the Inſtrument, and moving it up and down, the Air in CB might continue in the ſame State, and the Machine be only a Barometer during the Experiment.

This ſeems to bid fair for an Inſtrument, whereby the different Levels of Places may be taken; but upon a nice Examination, it will be liable to Error. For, though Sand is not altered in its Heat or Cold ſuddenly; yet in two or three Hours, as it is carried into a warmer or a colder Place, it will become hotter or colder, and the leaſt Degree of Heat or Cold, communicated to the Air CB, will alter the Height of the Liquor at D, when the Inſtrument is made ſo ſenſible as I have mentioned. Then if, in carrying the Inſtrument, it ſhould be accidentally inclined, ſo that the Liquor in the Bottle ſhould not cover the Bottom of the Tube at B, ſome Liquor may fall out of the Tube at B, or ſome Air may get into it: each of which Accidents, will quite ſpoil the Experiment. But if this Machine be made portable, without any Inconveniency, and be ſecured againſt the Action of Heat and Cold (or, which is the ſame, if the Alterations by Heat and Cold be exactly allowed for) it will be of very great Uſe and Certainty, in taking the Levels of diſtant Places, provided they be not ſo far diſtant from each other, that it requires above ſix Hours Time to carry the Inſtrument from one Place to another: nay, very diſtant Places, even at two or three Days Journey from one another, may be taken tolerably well with two Inſtruments, nicely adjuſted to each other, if they be taken Fig. 115.

notice of by two Obſervers at the ſame Hour, in fair Weather, and when there is no Wind.

Now ſuch an Inſtrument, I hope, I have contrived, whereby the Difference of Level of two Places, which could not be taken in leſs than four or five Days with the beſt Teleſcope Levels, may be taken in as few Hours.

To the Ball C is joined a recurve Tube B A of a very fine Fig. 116. Bore, with a ſmall Bubble at Top at A, whoſe upper Part is open. It is evident from the Make of this Inſtrument, that if it be inclined in carrying, no Prejudice will be done to the Liquor, which will always be right, both in the Ball and the Tube, when the Inſtrument is ſet upright. If by Heat, the Air at C be ſo expanded, as to drive the Liquor to the Top of the Tube, the Cavity A will receive the Liquor, which will come down again and ſettle at D, or near it, according to the Level of the Place where the Inſtrument is, as ſoon as the Air at C returns to the ſame Tenor in reſpect to Heat and Cold. To preſerve the ſame Degree of Heat, when the different Obſervations are made, the Machine is fixed in a Tin Veſſel F E, filled with Water up to *g b*, above the Ball; and a very ſenſible Thermometer has alſo its Ball under Water, that one may obſerve the Liquor at D in each Experiment, when the Thermometer ſtands at the ſame Height as before. The Water is poured out, when the Inſtrument is carried, which one may do conveniently by means of the wooden Frame, which is ſet upright, by means of three Screws, ſuch as *s*, and a Line and Plummet *p* P. Fig. 117. At the back Part of the wooden Frame, from the Piece at Top K, Fig. 119. hangs the Plummet P, over a Braſs Point at N: M *m* are Brackets to make the upright Board K N continue at Right Angles with the horizontal One at N. Fig. 120. does likewiſe repreſent Fig. 120. the wooden Frame and Screws. Fig. 118. repreſents the Fig 118. Machine ſeen in Front, ſuppoſing the Forepart of the Tin Veſſel tranſparent. And here the Braſs Socket of the recurve Tube, into which the Ball is ſcrewed, has two Wings at II, fixed to the Bottom, that the Ball may not break the Tube by its Endeavour to emerge, ~~when the Water is poured in as high as *g b*.~~

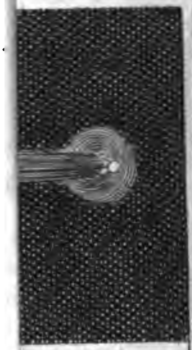
After I had contrived this Machine, I conſidered, that as the Tube is of a very ſmall Bore, if the Liquor ſhould riſe into the Ball A, in carrying the Inſtrument from one Place to another, ſome of it would ſtick to the Sides of the Ball A, and that upon its Deſcent in making the Experiment, ſo much might be left behind, that the Liquor would not be high enough at D, to ſhew the Difference of Level; therefore, to prevent that Inconveniency, I have contrived a blank Screw to ſhut up the Hole at A as ſoon as one Experiment is made, that in carrying the Engine, the Air in A may balance that in C, ſo that the Liquor ſhall not run up and down the Tube, whatever Heat and Cold may act upon the Inſtrument, in going from one Place to another.

Now, becauſe one Experiment being made in the Morning, the Water may be ſo cold, that when a ſecond Experiment is made at Noon, the Water cannot be brought to the ſame Degree of Cold that it had in the Morning; therefore in making the firſt Experiment, warm Water muſt be mixed with the cold; and when the Water has ſtood ſome time, before it comes to be as cold, as it is likely to be at the warmeſt Part of that Day; obſerve, and ſet down the Degree of the Thermometer, at which the Spirit ſtands; and likewiſe the Degree of the Water in the Barometer at D; then ſcrew on the Cap at A, pour out the Water, and carry the Inſtrument to the Place whoſe Level you would know; there pour in your Water, and when the Thermometer is come to the ſame Degree as before, open the Screw at Top, and obſerve the Liquor in the Barometer.

My Scale, for the Barometer, is ten Inches long, and divided into Tenths, ſo that ſuch an Inſtrument will ſerve for any Heights not exceeding ten Feet, each Tenth of an Inch anſwering to a Foot of Height. *N. B.* I have not made any Allowance for the Decreaſe of Denſity in the Air, becauſe I do not propoſe this Machine for meaſuring Mountains (though with proper Allowance for the decreaſing Denſity of the Air, it will do very well) but for Heights to be known in Gardens, Plantations, and the Conduct of Water, where an Experiment, that anſwers to two or three Foot in a Diſtance of twenty Miles, will render this a very uſeful Inſtrument.

CHAP.

e. 274



Evening

Fig. 110.

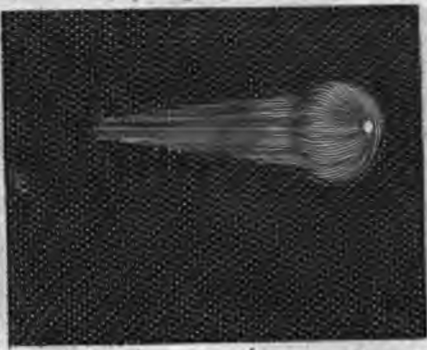


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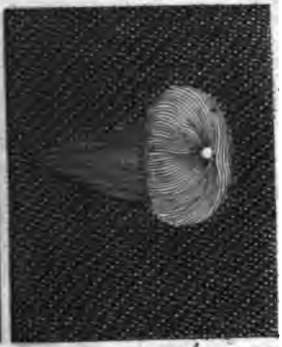
Fig. 111.



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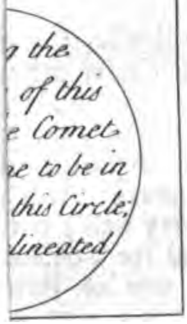

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Fig. 112.

Fig. 113.

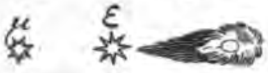


Fig. 115.

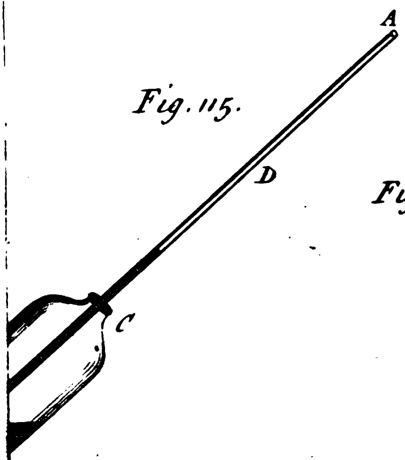


Fig. 116.

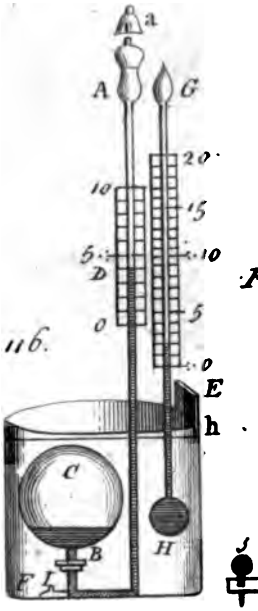
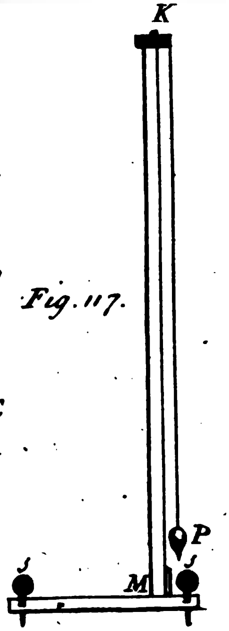


Fig. 117.



CHAP. V.

MECHANICS.

NON ab re fore exiſtimo ſi objectionem inſeruero à magni nominis viro [a] D. *Leibnitio* Germano factam, & in Actis Eruditorum *Lipſiæ* hoc anno publicatis inſertam contra *Carteſium* & Carteſianos, eandem ſemper in Mundo eſſe Motûs Quantitatem aſſerentes, una cum egregii quo laborat Paralogiſmi detectione manifeſta. Objectio Leibnitii ita ſe habet.

E. Prælectionibus de Motu, a Davide Gregorio *in Acad.* Edinburgena, Matheſeos Profeſſore, dictatis, Ann. Dom. 1686.

Vult grave E ex altitudine B A cadere, item grave F prioris quadruplum cadere ex altitudine D C, quæ altitudinis B A ſit tantum pars quarta. Ab ipſis Carteſianis & omnibus conceditur impetus ab hiſce gravibus cadendo acquiſitos, tales eſſe ut quiſque ſuum grave ad altitudinem unde ceciderat elevare potis eſſet, ſi directio motûs foret ſurſum verſus, *&c.* Ex ipſis porro principiis Carteſianis conſtat tantâ opus eſſe vi ad elevandum corpus F, quatuor librarum ad altitudinem C D unius ulnæ, quantâ opus eſt ad elevandum corpus E, unius libræ ad altitudinem A B quatuor ulnarum. Adeoque gravia E & F, ex prædictis altitudinibus cadendo æquales acquiſiviſſe vires, ſive motûs quantitates. Quod ſi aliâ ratione ineatur calculus, deprehenditur gravia inæquales obtinere motûs quantitates: etenim grave E cadendo ex altitudine B A quatuor ulnarum, acquiret duplam celeritatem ejus quam idem vel aliud grave cadendo ex altitudine DC unius ulnæ (per P. [illegible] hujus.) Ut vero habeatur quantitas motûs, ſive momentum gravis F, multiplicetur ipſius moles ut 1, in celeritatem ut 2; fietque momentum ſive quantitas motûs 2; corporis vero moles, ut 4, multiplicata in celeritatem ut 1, producet ejuſdem momentum ſive motûs quantitatem ut 4; *h. e.* Calculus hoc Modo inſtitutus facit quantitatem motûs in corpore F, duplam quantitatis motûs in corpore E, cum priori modo inventæ ſint æquales. Unde infert auctor magnum eſſe diſcrimen inter vim motricem & motûs quantitatem, neque alteram ab alterâ menſurandam, cumque & iſta eadem & æqualis in mundo perſiſtat, hæc pro re natâ continuo mutabitur.

Fig. 121.

Abſoluto hoc contra Carteſianos argumento, quo nullum certius, nullum ſimplicius eſſe aſſerit, miratur *Carteſium* ipſum, ejuſque ſequaces illud non percepiſſe. *Carteſium* appellat præſidentiorem, Carteſianos vero ipſos Peripateticos nimium imitari veretur, magiſtri libros rectæ rationis loco conſulendo. Poſteaque, hinc ortos eſſe quamplurimos in mechanicis errores, queritur. Novum condit theorema, neſcio quid de altitudinibus, celeritates productricibus continens, ex quarum ratione & ratione ipſorum corporum compoſitam rationem eam eſſe vult virium ſive momentorum.

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[a] *The Reader is deſired to take Notice, that the preceding Paper, Written about the Year* 1686, *by Mr.* David Gregory, *late Savilian Profeſſor of Aſtronomy at* Oxford, *is not in the Philoſophical Tranſactions, and was never printed till now.*

Verum pace tanti viri, alioqui de philoſophiâ bene meriti, dictum ſit, ſatis mirari non poſſe Carteſianos, Leibnitium talem tantumque paralogiſmum ſub adeo amplis titulis venditare. Fallacia hic latet: quod vult tantâ opus eſſe vi ad elevandum pondus E unius libræ ad altitudinem B A, quatuor ulnarum, quantâ ad elevandum pondus F quatuor librarum ad altitudinem C D unius ulnæ, quod utique verum eſt, & ab ipſo Carteſio diſerte agnoſcitur, modo uterque motus ſive elevatio perficiatur eodem vel æquali tempore, aliter vero omnino falſum erit; neque enim major, ſed prorſus eadem requiritur vis ad elevandum pondus quodvis ad altitudinem centum pedum, quæ ad altitudinem pedis unius, ſi ad priorem (effectum) efficiendum centies majus concedatur tempus quam ad poſteriorem. Cum jam in caſu propoſito majus conceditur tempus ad elevandum pondus E unius libræ ad altitudinem A B quatuor ulnarum, quam ad elevandum pondus F quatuor librarum ad altitudinem C D unius ulnæ (quoniam, æquali Tempore, Impetus caſu acquiſitus ſuum grave elevabit ad altitudinem unde ceciderat, quo cadendo impenderat; & grave E cadendo ex B in A, duplum tempus impendit ejus quod grave F impendit cadendo ex D in C) Patet dimidiam tantum vim requiri ad elevandum grave E unius libræ (in conditionibus ab auctore poſitis) in altitudinem A B quatuor librarum, quam ad elevandum grave F quatuor librarum ad altitudinem C D unius ulnæ; adeoque grave E, acquirere, cadendo ex B in A, dimidium tantum momentum ſive motûs quantitatem ejus quam acquirat F cadendo ex D & C, & non æqualem ut ex falſa ſuppoſitione invenit Auctor. Et ex poſteriore calculo idem invenitur, unde nulla contradictio; nec ex hoc argumento concluditur vim motricem non poſſe repreſentari per quantitatem motûs, neque ideo æqualem motûs quantitatem in mundo ſemper a deo haud conſervari. Plurimis autem aliis argumentis Carteſiana hæc propoſitio evertitur, ſicut Geometriæ hujus de Motu parte primâ, cum de motûs legibus ageretur, oſtenſum eſt. Verum miſſâ hac digreſſione phyſica in ordinem redeamus, *&c.*

II.

Concerning an Experiment, relating to the force of Bodies in Motion. By Henry Pemberton, *M.D.* F. *R.S.* N° 371. *p.* 57.

In *Polenus*'s Tract *de Caſtellis*, I found ſeveral curious Experiments, among which I reckon that of letting Globes of equal Magnitude, but of different Weights, fall upon a yielding Subſtance, as Tallow, Wax, Clay or the like, from heights reciprocally proportional to the Weights of the Globes. But cannot by any means admit of the Deduction that is drawn from thence, that becauſe the

Globes make in this Experiment equal Impreſſions in the yielding Subſtance, therefore they ſtrike upon it with equal Force: Whereby it is attempted to prove the Aſſertion of Mr. *Leibnitz*, that the force of the ſame Body in deſcending is proportional to the Height from whence it falls; or, in all Motion, proportional to the Square of the Velocity, and not proportional to the Velocity it ſelf, as is commonly thought. On the contrary, I think this very Experiment proves the great unreaſonableneſs of Mr. *Leibnitz*'s Notion.

Certainly this Experiment of *Polenus* is much more fit to inform us of the Law, by which theſe yielding Subſtances reſiſt the Motion of Bodies ſtriking upon them, than to ſhew the Forces, with which Bodies ſtrike; for whatever thoſe Forces be, the Effects muſt be very different, according to the Difference there may be in the Rule obſerved by ſuch Reſiſtance.

Now this Experiment ſhews, that if two Globes in Motion bear againſt equal Portions of the yielding Subſtance, the Oppoſition, that Subſtance makes to the Motion of the Globes, will be the ſame in both, however different the Velocities be, with which they move. This I demonſtrate as follows.

Let A and B be two Globes, equal in Magnitude, but of different Weights, which are equally immerſed into a yielding Subſtance. Suppoſe the Velocities, with which they move in their preſent Situation, to be reciprocally in the ſubduplicate *Ratio* of the Weights of the Globes; that is, let the *Ratio* of the Weight of the Globe A to the Weight of the Globe B, be duplicate of the *Ratio* of the Velocity of the Globe B, to the Velocity of the Globe A. Since therefore the *Ratio* of the Quantity of Motion in the Globe A, or of the Force with which it moves, to the Quantity of Motion in the Globe B, or to the Force with which that Globe moves, is compounded of the *Ratio* of the Weight of the Globe A, to the Weight of the Globe B, and of the *Ratio* of the Velocity of the Globe A, to the Velocity of the other Globe B, the Force, with which the Globe A moves, is to the Force, with which the Globe B moves, as the Velocity of this Globe B, to the Velocity of the other Globe A. But if the ſame Oppoſition be made to the Motion of the Globes, when they bear upon equal Portions of the yielding Subſtance, the Effect of that Oppoſition, while the Globes enter farther into the Subſtance by equal Spaces, will be proportional to the Time, in which the Globes are moving thoſe Spaces, or in which the Oppoſition is made, if we conſider thoſe Spaces while naſcent or in their firſt Origine; the Effect therefore of this Oppoſition will be reciprocally proportional to the Velocity of each Globe; namely, the momentaneous Loſs of Force in the Globe A will be to the momentaneous Loſs of Force in the Globe B, as the Velocity of the Globe B, to the Velocity of the Globe A; and the whole Force of the Globe A has been found to bear the ſame *Ratio* to the whole Force of the Globe B; conſequently theſe Globes, while they penetrate equal Spaces into the Subſtance, loſe part of their Force, which bear the ſame Proportion to the whole: And therefore, if their Velocities be at any time reciprocally in the ſubduplicate *Ratio* of their Weights, ſo that the Forces or Degrees of Motion, with which they move, be reciprocally proportional to their Velocities, the Forces, with which they preſs into the yielding Subſtance, at equal Indentures made in the Subſtance, will continue in the ſame Proportion; and therefore upon the Theory of Reſiſtance here ſuppoſed, when the whole Force and Motion of both theſe Globes is entirely loſt, they will be plunged into the Subſtance at equal Depths.

Now whereas in the Experiment of *Polenus*, the Globes, falling from Heights reciprocally proportional to their Weights, ſtrike upon the yielding Subſtance with Velocities reciprocally in the ſubduplicate Proportion of their Weights, and the Effect is in all Caſes found to be, what is here deduced from this Theory of Reſiſtance; it is a ſufficient Confirmation of the Truth of this Theory.

Only here, I have ſuppoſed the Globes to be ſtopt by the whole Reſiſtance of the Subſtance, they move againſt; although in ſtrictneſs they are ſtopt only by the Exceſs of that Reſiſtance above the Action of Gravity upon them. But I have neglected the Conſideration of the Action of Gravity, that being but ſmall in Proportion to the Reſiſtance, as will appear from the Globes being much more ſpeedily ſtopt by this Reſiſtance, than they would be by the Action of Gravity, if its force were applied upwards; for by that Force alone, the Globes would not be ſtopt, till they had meaſured Spaces equal to the Heights above the reſiſting Subſtance, from whence they fell; which Heights bear a great Proportion to the Depths, in the yielding Subſtance, into which the Globes in this Experiment are immerſed, as I have found upon Trial.

As I have aſſerted that the very Experiment of *Polenus* is not only reconcileable to the common Doctrine of Motion, as I have now demonſtrated; but even that it does it ſelf make manifeſt the great unreaſonableneſs, if not the abſolute Abſurdity, of Mr. *Leibnitz*'s Opinion; it remains that I briefly make proof of this.

If two Globes A and B, of equal Magnitude but of different Weights, ſtriking on a yielding Subſtance with equal Force, in every Caſe loſe all their Motion at equal Depths, it is neceſſary that at all times, during their Motion, they loſe equal Degrees of Force, when they bear upon equal Portions of the Subſtance, in entring equal Spaces into the Subſtance. This will be eaſily ſeen from what has before been ſaid. Now whereas Mr. *Leibnitz* ſuppoſes the power of Gravity to give to the ſame falling Body Degrees of Force proportional to the Height from whence it falls; according to his Opinion, by the Power of Gravity, equal Degrees of Force are added in the deſcent of the ſame Body through equal Spaces; and in different Bodies deſcending through equal Spaces, the Degrees of force added will be as the quantity of Matter, or as the weight of each Body. Therefore while the Globes A and B penetrate equal naſcent Spaces into the yielding Subſtance, by the Action of Gravity, were not that Action overcome by the Reſiſtance of that Subſtance, additional Degrees of Force would be communicated in ſuch Proportion, that the force added to the Globe A, would be to the force added to the Globe B, as the weight of the Globe A, to the weight of the Globe B, or in the duplicate *Ratio* of the Velocity of the Globe B, to the Velocity of the Globe A. But ſince the Globes loſe the ſame Degrees of force in entring equal Naſcent Spaces into the yielding Subſtance, the Effect of the Oppoſition made by this Subſtance to the Motion of the Globes, during the time of their paſſing through ſuch Naſcent Spaces, will be both the taking from them that ſame Degree of force, and moreover the additional Force, which would otherwiſe have been given them by their own Gravity. But farther, the Oppoſition made to the Motion of the Globe A, to the Oppoſition made to the Motion of the Globe B, will be in the *ratio* compounded of the *Ratio* of the Effect of the Oppoſition, which the Subſtance makes to the Motion of the Globe A, to the Effect of the Oppoſition, which the Subſtance makes to the Motion of the Globe B, and of the *ratio* of the Time, in which the Oppoſition is made againſt the latter Globe, to the Time in which it is made againſt the former; which latter *ratio* is the ſame with the *ratio* of the Velocity of the Globe A, to the Velocity of the Globe B. But ſince it is ſhewn, that the Effect of the Oppoſition made by the yielding Subſtance to theſe Globes is two-fold, and that one part of the Effect of the Oppoſition made to the Motion of the Globe A, is equal to one part of the Effect of the Oppoſition made to the Motion of the Globe B; and that another part of the Effect of the Oppoſition made to the Motion of the Globe A, to another part of the Effect of the Oppoſition made to the Motion of the Globe B, is in the Duplicate *ratio* of the Velocity of the Globe B, to the Velocity of the Globe A: One part of the Oppoſition it ſelf made to the Motion of the Globe A, will be to one part of the Oppoſition againſt the Motion of the Globe B, as the Velocity of the Globe A, to the Velocity of the Globe B; and another part of the Oppoſition to the Motion of the Globe A, to another part of the Oppoſition to the Motion of the Globe B, will be as the Velocity of the Globe B, to the Velocity of the Globe A. So that when the Globes bear upon equal Portions of the yielding Subſtance, the Oppoſition to their Motion will be in part as the Velocity of the Globes, and in part reciprocally as their Velocity. Hence, becauſe the reſiſting Subſtance is of an uniform Texture, the Oppoſition to the Motion of either of the Globes in its preſent Situation, and when moving with its preſent Velocity, will be to the Oppoſition it would meet with in the ſame Situation, if it moved with any other Velocity, partly as the preſent Velocity to that other Velocity, and partly as that other Velocity to the preſent. But by that part of the Oppoſition made againſt the Motion of the Globe, which is directly as the Velocity, the Globe can never be wholly ſtopt; for upon the ſtopping of the Globe, that part of the Oppoſition to its Motion will likewiſe totally ceaſe, and conſequently the Globe's Weight will carry it further down, unleſs the other part of the Oppoſition againſt its Motion prevent it. But I ſay again, neither can this latter part of the Oppoſition made to its Motion be ever great enough to ſtop the Globe; for the Degree of this Oppoſition being reciprocally as the Velocity of the Globe, when the Motion of the Globe is wholly taken away, it will become infinitely greater, than at any time, while the Globe is in Motion; ſo that when the Globe ſhould be ſtopt by this part of the Oppoſition made to its Motion, the Oppoſition to the Globe's Motion will become infinitely great; inſomuch, that no Degree of force whatever could be able to impel the Globe further into the Subſtance, but this can never come to paſs. Beſides, it is not neceſſary to apply any ſuch refined Argument againſt this part of the Reſiſtance; it would be alone ſufficient to conſider, how unreaſonable a Suppoſition it is, that a Reſiſtance ſhould increaſe, when the Velocity of the reſiſted Body decreaſes.

Thus may this Experiment be made uſe of to invalidate that very Opinion it is brought to ſupport. But another uſe may likewiſe be made thereof: For it will ſerve to illuſtrate what Sir *Iſaac Newton* has more than once hinted, that the Reſiſtance of Fluids, which ariſes from the Tenacity of their Parts, decreaſes in a leſs Proportion than the Velocity of the reſiſted Body decreaſes *; for as this Reſiſtance bears a great Analogy to the Reſiſtance of the yielding Subſtances we have been here treating of, ſo we have found, that the Reſiſtance of theſe Subſtances does not much depend upon the Velocity of the Body, againſt which the Reſiſtance is applied.

An Argument in confirmation of the foregoing Paper, by —— No 371. *p.* 67.

2. Suppoſe pieces of fine Silk, or the like thin Subſtance, extended in Parallel Planes, and fixed at ſmall Diſtances from each other. Suppoſe then a Globe to ſtrike perpendicularly againſt the middle of the outermoſt of the Silks, and by breaking through them to loſe part of its Motion. If the Pieces of Silk be of equal Strength, the ſame Degree of force will be required to break each of them; but the Time, in which each piece of Silk reſiſts, will be ſo much ſhorter as the Globe is ſwifter; and the loſs of Motion in the Globe

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* *Vid.* Philoſ. Nat. Princip. Math. Prop. 52. lib. 2. in Schol. Opticks. Qu. 28. p. 339, 340.

conſequent upon its breaking through each Silk, and ſurmounting the Reſiſtance thereof, will be proportional to the Time. in which the Silk oppoſes itſelf to the Globe's Motion; inſomuch that the Globe by the Reſiſtance of any one piece of Silk will loſe ſo much leſs of its Motion as it is ſwifter. But on the other Hand, by how much ſwifter the Globe moves, ſo many more of the Silks it will break through in a given Space of Time; whence the number of the Silks, which oppoſe themſelves to the Motion of the Globe in a given Time, being reciprocally proportional to the Effect of each Silk upon the Globe, the Reſiſtance made to the Globe by theſe Silks, or the loſs of Motion, the Globe undergoes by them in a given Time, will be always the ſame.

Now if the Tenacity of the Parts of Fluids obſerves the ſame Rule as the Coheſion of the Parts of theſe Silks; namely, That a certain Degree of force is required to ſeparate and diſunite the adhering Particles, the Reſiſtance ariſing from the Tenacity of Fluids muſt obſerve the ſame Rule as the Reſiſtance of the Silks, and therefore in a given Time the loſs of Motion, which a Body undergoes in a Fluid by the Tenacity of its Parts, will in all Degrees of Velocity be the ſame; or in fewer Words, that part of the Reſiſtance of Fluids, which ariſes from the Coheſion of their Parts, will be Uniform.

An Account of ſome Experiments to prove that the Force of Moving Bodies is proportionable to their Velocities. By the Rev. J. T. Deſaguliers, *LL. D. F.R.S.* N° 375. *p.* 269.

3. If a Man with a certain Force can move a Weight of fifty Pounds, through a Space of four Feet, in a determinate Time; it is certain he muſt employ twice that Force to move one hundred Pounds Weight, through the ſame Space in the ſame Time. But if he uſes but the ſame Force, he will move the one hundred Pounds Weight but two Feet in the ſame time. For as the one hundred Pounds Weight contains two fifty Pound Weights, if each of them has two Degrees of Velocity given to it, it will exactly require the ſame Force that would give one of them four Degrees of Velocity; hence it appears, that the Force is proportionable to the Maſs multiplied into the Velocity.

Experiment I. Fig. 122.

Let the Balance A B, whoſe Center of Motion, is at C, be ſo divided, that the *Brachium* A C be but the fourth Part of the *Brachium* C B; it is known to all Mechanicians, that a Weight of one hundred Pounds at A, will keep in *Æquilibrio* a Weight of twenty five Pounds hanging at B, where it will have a Velocity four times greater than that of the Weight at A. For, not only when the Balance is horizontal, there will be an *Æquilibrium*, but when the Balance is put in Motion, it will return to an *Æquilibrium* in a horizontal Poſition; the equal and contrary Forces applied at each, deſtroying one another. Whereas, if the Forces were as the Maſs multiplied into the Square of the Velocity, the twenty five Pound Weight ſhould have been ſuſpended at D, only twice as far from C, as the Weight at A; and in general, let the make of the Engine be what it will, let the mechanical Powers be combined in any manner, when two heavy Bodies, by means of a Machine, act upon one another in different Directions, if their Velocities are reciprocally as their Maſſes, they will deſtroy each others Forces and come to Reſt.

Experiment II.
Fig. 123.

Let the Weight P of one Pound, be placed in the Scale ſuſpended at the end A, of the Balance A B, which bears upon the *Gnomon*, or Iron Supporter, *k h i*. Then if the Weight C be let fall from D, or one Foot, it will by its Stroke on the end of the Beam B, raiſe up the oppoſite end A with the Weight P, ſo high, that the Spring *g h* will fly from the Button *i*, which kept it ſtreight and upright before the Shock. If the Weight P be of two Pounds, it cannot be raiſed by the fall of C from any height leſs than F or four Foot; whereas, if the force of the Shock was proportionable to the Space, without any regard to the Time, P ought to be raiſed, when C falls only from E, or two Foot, which never happens; or, if the Stroke was proportionable to the Maſs multiplied into the ſquare of the Velocity, when C falls from F, then P might weigh four Pound, whereas the Experiment will never ſucceed under thoſe Circumſtances.

Experiment III.

If (in order to avoid Friction) inſtead of a Blow, ſtruck upon the end B, by the falling Body, the ſaid Body C be faſtned to a pretty long String tied to the Bottom *m*, as at *c*, and firſt lifted up one Foot, and then let fall; ſo that in falling one Foot, it may pull down B, and lift up A with the Weight P of one Pound; whenever P is two Pounds, C muſt fall from a height greater than *f* or four Foot, otherwiſe it will not raiſe the *Brachium* A, eſpecially if it be let fall between *e* and *f*.

Experiment IV.

I took the Weight C of ſeventeen Ounces Troy, which was a round Ball of Lead with a hole through the middle of it, and having paſſed the String N-X through it, before it was faſtened to the Hook X, I placed the whole Machine in ſuch manner, that the String being ſtretched by the Weight N, went through the hole of the Weight C, and likewiſe through the hole of the *Brachium* B, upon which C lay, without touching the ſides of the hole either in the Weight or Balance; then having put ſuch a Weight P in the oppoſite Scale, as C falling from the height of one Inch, was able to raiſe high enough, to let looſe the Spring *g h* from the Button *i*: I added to P another Weight equal to it, and then letting fall C along the String that guided it, from a heighth of two Inches, then of three, and then exactly of four, it would not raiſe the double Weight P to the former height, but falling from five Inches, or a little higher, it raiſed it up.

Experiment V.

Leaving every thing as it was before, I changed the Weight C for another leaden Ball of twice the Weight, which falling from one Inch, raiſed the double Weight P to the uſual Height; then changing the Weight P in any Proportion, whatever Height was required for the heavieſt Ball C (or C 2) to fall from, in order to raiſe the Weight at P; more than four times the Height was required for the firſt Ball C, to raiſe the ſame Weight ſo high, as to let looſe the Spring.

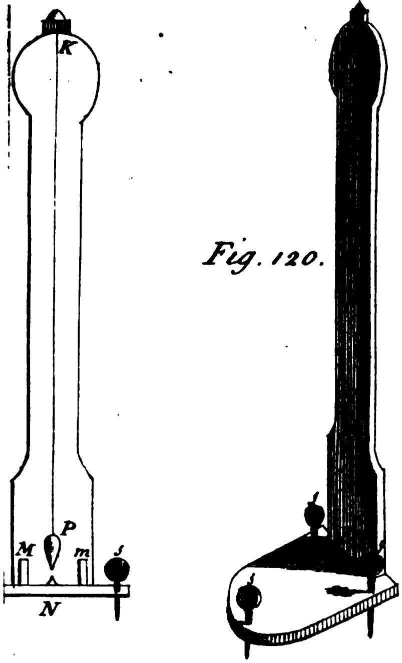


Fig. 120.

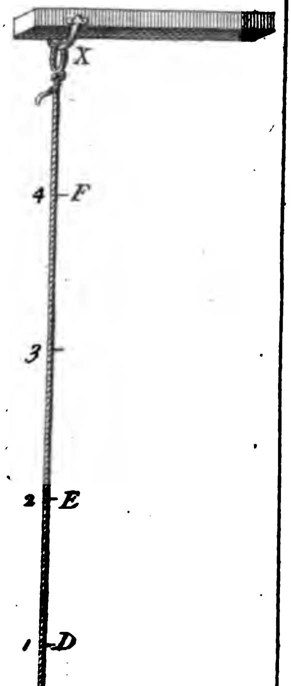
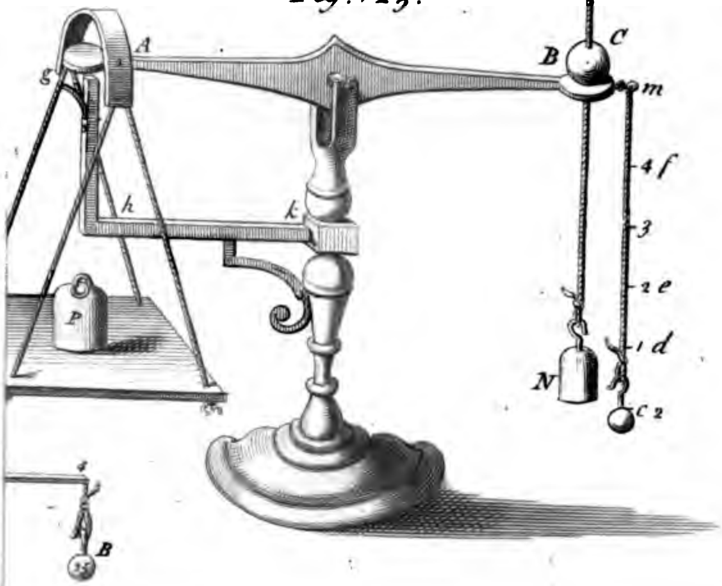


Fig. 123.



Experiment VI.

I tried the Experiment with the Weight C hanging at the String *m c* (as in Experiment III.) and a Fall from a height of near five Inches, was required to raiſe double the Weight in the oppoſite Scale, that a fall from one Inch would raiſe; only here the heighth above four Inches was not ſo great as in the former Experiment, the Friction being ſomething leſs. Then I ſuſpended the great Ball C (or C 2) by the String *m c*, and when by falling one Inch it raiſed the Weight P, the little Weight C could not produce the ſame Effect, without falling from a greater height than four Inches.

It is here to be obſerved, that which way ſoever theſe Experiments are tried, the Objections riſing from the Friction do no way ſerve to confirm the new Opinion, becauſe they ſhew that (upon account of the Friction) the Heights muſt be ſomething more than in a duplicate Proportion of the Velocities, but never leſs, to give a Blow with the ſame Body in Proportion to the Velocity.

That the *Momentum* of Bodies is in Proportion to the Maſs multiplied into the Velocity, is alſo moſt evidently ſhewn from the Congreſs of elaſtick Bodies, as has been demonſtrated by Sir *Iſaac Newton* in his *Principia*, in the Corollaries to his Laws of Motion. I had often tried the Experiments there mentioned with Balls of Ivory, Glaſs, and Steel, of two Ounces each, and found every thing anſwer, allowing for the want of perfect Elaſticity in the Bodies. But now upon this Occaſion, as the Objections to the received Opinion were renewed, I was willing to repeat the Experiments with the utmoſt Accuracy; and therefore, as Ivory Balls are not equally denſe in all their Parts, and Glaſs Balls break after two or three ſtrokes; I cauſed Balls to be nicely turned of Steel, and made as hard (as the Workmen call it) that is, as elaſtick as poſſible, and the Weights of them were preciſely as follows: Two Balls of twelve Ounces Troy each, one of ſix Ounces, one of three, one of two, and one of Eight-penny-weight. Then making Pendulums of theſe Balls, and hanging them upon the Machine contrived by *Mariotte* for the Congreſs of Bodies, and lately improved by Dr. *Graveſande*[a], I meaſured $57\frac{1}{4}$ Inches between the Center of Suſpenſion and the Center of Gravity of the Balls, and then every Degree of the Circle they deſcribed in their Oſcillation was one Inch, and the Degrees being marked upon a line of Chords on a Braſs Ruler above the Balls, by their Strings ſucceſſively covering the croſs Lines of Diviſion, the Degrees that the Balls fell from, and thoſe to which they roſe, were very diſcernable to an Eye placed at a convenient Diſtance.

Experiment VII.

I took the two Balls 12, and removing each from the loweſt Point of their equal and reſpective Circles, up to 4 Inches, or 4 Degrees, I let them fall ſo that they met at Bottom, and were both reflected again to 4, the Place from whence they fell.

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[a] Introd. N° 170. Vol. 1.

Experiment VIII. Every thing elſe being as before, inſtead of one of the Balls 12, I took the Ball 6, then letting 6 go from 8 Degrees, and 12 from 4, after Reflection 12 was driven up again to 4, as before.

Experiment IX. The Ball 3 falling from ſixteen Degrees met the Ball 12 that fell ſtill from 4, and after Reflection 12 went up again to 4.

Experiment X. The Ball 2 falling from 6° and 12 from 1°, 12 was reflected to 1, and when 2 fell from 12 Degrees, and the Ball 12 from 2, the 12 was reflected to 2.

Experiment XI. The Ball of eight Penny Weight (which weighed but $\frac{1}{30}$ of the Ball 12) falling from fifteen Inches or Degrees, raiſed up 12 (that fell from half a Degree) to the ſame Place again.

In all theſe Experiments the Error, or want of perfect Reflection, was greater in the little Balls than in the great ones, on account of their going through a greater *Arc* of a Circle, whereby they deviated more from a Cycloid than the great ones; as likewiſe on account of a Reſiſtance of the Air, which muſt be greater becauſe of the little Balls going through a greater *Arc*, moving with more Velocity, being ſuſpended by a String as thick as that of the great ones, and having more Surface in Proportion to their Weight. But all the Errors do not bring the *Phænomena* any thing near what they ought to be, if the Force of the Bodies was as the Square of their Velocities multiply'd into their Maſſes, for then the Ball 12 would have been driven to Heights very different from what it roſe up to.

In the eighth Experiment, the Ball 12 ſhould have riſen to near five Inches and three quarters, for the Ball 6 falling with the Velocity 8, muſt have had its Force $= 8 \times 8 \times 6 = 384$; and then, that the Ball 12 might have the ſame Force or Quantity of Motion, it muſt riſe near to 5, 7 becauſe $5{,}7 \times 5{,}7 \times 12 = 389{,}88$.

In the ninth, 12 ſhould have riſen to 8; for the Ball 3 muſt have had its Force $= 16 \times 16 \times 3 = 768$, and if 12 received its whole Force it muſt have riſen to 8 becauſe $8 \times 8 \times 12 = 768$.

In the ſecond Part of the tenth Experiment, 12 ſhould have riſen to near 5, becauſe $12 \times 12 \times 2 = 288$, and $5 \times 5 \times 12$ is but 300.

In the eleventh, the Ball 12 (thirty times heavier than the little one) muſt have gone to 2 ¾ Inches, becauſe the *Momentum* of the little Ball being $= 15 \times 15 \times 1 = 225$, that of the Ball 12 muſt be $= 2{,}75 \times 2{,}75 \times 12 = 226$, &c.

It may be here alledged, that one ought to ſubſtract the *Momentum*, with which the great Ball comes upon the little one; but that won't mend the Matter much, though indeed the Difference will be leſs. For,

In the eighth Experiment, if we ſubtract $4 \times 4 \times 12 = 192$ from 389,88 there will remain 197,88, and the Ball 12 will go but to 4; but then in Experiment 9, if we ſubtract the ſame N° 192 from 768, we ſhall have 576, which would carry 12 to near ſeven Degrees, becauſe $7 \times 7 \times 12 = 588$.

In the tenth Experiment, there is only 48 to be ſubtracted; and in the eleventh only 15, and therefore the Velocity of 12 will very much fall ſhort of what is agreeable to the new Opinion.

After the Experiments made, and what has been ſaid, till theſe Conſequences are overthrown, no notice ought to be taken of Objections, or new Experiments. But to give the Objectors all poſſible Satisfaction, I ſhall, in the following Paper, endeavour to ſhew the Fallacies of the Arguments, and ſolve the *Phænomena* of the Experiments made; ſhewing, both by Reaſon and Experiment, that the Facts ought to be as they are, in conſequence of the received Opinion and Laws of Reſiſtance.

Animadverſions upon ſome Experiments relating to the Force of moving Bodies; with two new Experiments on the ſame Subject. By the ſame. Nº 376. p. 285.

4. *Polenus*'s Miſtake lies in this; that he eſtimates the Force of the Stroke of the falling Balls, by the Depth of the Impreſſion made in the Tallow, Clay, Wax, or any yielding Subſtance. But we muſt conſider, that when two Bodies move with equal Forces, but different Velocities, that, which moves the ſwifteſt, muſt make the deepeſt Impreſſion, whilſt the ſloweſt Body communicates its Motion to the Clay round about, and therefore does not ſtrike in ſo deep as the ſwifter Body, which puts in Motion few Parts of the Clay, beſides thoſe that are before it, and which Parts have ſo much leſs Time to oppoſe this Body's Motion, as its Velocity is greater. To make this plainer, Let us ſuppoſe a Door half open, and moving very freely on its Hinges; if a Piſtol be fired againſt it, the Ball will go through the Door without moving it out of its Place; but if we take a large Weight of Lead, and throw it againſt the Door, with the ſame Force as the Piſtol Bullet moved, the Door will be carried out of the Place on its Hinges by the Stroke; becauſe in the firſt Caſe, the Motion of the Ball is communicated but to a few Parts of the Door, and in the laſt it is diffuſed all over it. Nay, the Door will be moved by the Stroke, even though there ſhould be a prominent Part in the Lead, no bigger than a Piſtol-Bullet, in order to ſtrike the Door upon no more of its Surface, than the Bullet had done.

For illuſtrating this farther I contrived the following Experiment.

I cauſed a Machine to be made, conſiſting of a Baſe of Wood A B, which could be ſet horizontal by means of three Screws, ſuch as SS: Upon this Board, or Baſe, there ſtood upright two parallel Boards, about four Inches wide, and four Inches aſunder, with the Elbow-piece E F ſliding behind one of them, ſo as to raiſe its upper End F to any Height deſired. Between theſe Boards, ſquare Frames of Wood C C *&c.* with Paper extended upon them, could ſlide in, to the Number of Six, in an horizontal Poſition. Theſe Paper Diaphragms being thus placed, I ſuſpended an Ivory Ball of about one Inch and a half Diameter, weighing ſomething more than an Ounce and an half, by a ſhort Thread, under F, ſo that its Center of Gravity hung four foot over the firſt Diaphragm; then cutting the Thread, the Ball fell upon the Paper, and by its perpendicular Stroke broke through that Diaphragm, and the three next under it. Then putting ſo much Lead into the Ball above-mentioned, (which was made hollow for that Purpoſe) as to make it weigh twice as much as it did before; and bringing down F, to let it fall but from one foot; it broke through only two Diaphragms by its Fall. Making the Experiment ſeveral times with different Heights, but ſtill keeping the Proportion in Height of four to one, when the Balls were as one and two, the heavy and ſloweſt Ball broke through but half the Number of Papers. It happened indeed ſometimes that there was ſome little difference, when the Papers were not equally ſtrong, or equally ſtretched, but the ſwifteſt Ball always broke through more Papers than the ſlow one.

Now though this Experiment does at firſt ſeem to confirm *Polenus*'s Theory; yet, when duly weigh'd, it proves no ſuch thing. For the lighter Ball does not break through more Papers, becauſe it has more Force, or a greater Quantity of Motion, but becauſe each Diaphragm has but half the time to reſiſt the Ball, that falls with a double Velocity, and therefore their Reſiſtance being as the time, as many more of them muſt be broken by the ſwift Ball, as by the ſlow one.

To all the Objectors, that allow the Force of moving Bodies, and their Quantity of Motion to be the ſame, what has been ſaid in this and my former Paper, ſeems to be a full Anſwer; but as there are now ſome Philoſophers, who diſtinguiſh that Force from the Quantity of Motion, I am obliged to ſay ſomething more for the clearing up of that Point.

If I underſtand them right, they call *vis viva*, a Force, whoſe Effect is ſenſible, as the Force of Gravity, when it accelerates Bodies in their Fall; and *vis mortua* a Force, which being deſtroyed, produces no ſenſible Effect, as the force of Gravity acting upon a Weight in one Scale of a Balance, when the Weight cannot deſcend by reaſon of a Counterpoiſe in the other Scale. But certainly no Man, that conſiders the thing attentively, would make that Diſtinction. However, ſince *Polenus* allows, that the Quantity of Motion in Bodies is as the Maſs multiplied into the Velocity (or M V); but ſays, that the Force, with which they act, which he diſtinguiſhes by the Names of *vis viva*, is as the Maſs multiplied into the Square of the Velocity, or M V V: I have made the following Experiment to ſhew his Notion to be inconſiſtent; though all the *Phænomena* of unequal Weights applied to a *Statera*, ſo as to make an *Æquilibrium*, might ſerve for that Purpoſe, if it had not been objected, that the particular Conſtruction of the Machine hindered it from agreeing with the ſuppoſed Theorem, that the Force is as the Matter multiplied into the Square of the Velocity.

Experiment. Fig. 125.

Let two Balls, A and B, be joyned by a String, which going through the ſmooth Hole C of an even Table, and under the Pulley P, ſuſpends a Weight W. It is plain, that upon letting go the Balls A and B, from the places A, B, they will move towards C with the ſame Force, becauſe each of them will be drawn towards C by half the Force of the Weight W, whether the Balls be equal, or unequal.

1. The Balls being of two Ounces each (of Ivory), were, at the ſame inſtant of Time, let looſe from A and B, each diſtant twelve Inches from C, and both came to C at the ſame Time. Here the equal Forces will agree with the Product of the Maſſes into the Velocities, or into the Squares of the Velocities; becauſe $A \times 12 = B \times 12$, as well as $A \times 144$ is equal to $B \times 144$.

2. If A be four Ounces Weight, and let go from D, or ſix Inches, whilſt B, equal to two Ounces moves from 12 Inches; both Bodies will again meet at C: therefore here the equal Forces muſt be expreſſed by the Maſſes into the Velocities, and not into their Squares; for though $A \times 6$ be equal to $B \times 12$ ($4 \times 6 = 2 \times 12$), $A \times 6 \times 6$, or 144 is but half of $B \times 12 \times 12$, or 288. Whereas if the Forces had been as *Polenus* affirms, B ſhould have been let looſe only from 8, 4 Inches.

3. When A is ſix Ounces, it is let looſe only from E, or 4 Inches, to meet at C with B let looſe from 12; for then $A \times 4 = B \times 12$, whilſt $A \times 4 \times 4$, or 96, is three times leſs than $B \times 12 \times 12$, or 288. So that according to *Polenus*, B muſt have been let looſe from 7; but in that Caſe it comes ſooner to C than A.

N. B. The Weight W, muſt be greater than the Weight of both Balls, leſt the Friction of the Table ſhould ſpoil the Experiment.

A Remark upon the New Opinion relating to the Forces of moving Bodies, in the Caſe of the Colliſion of Non-Elaſtic Bodies. *By* Mr. John Eames, *F.R.S.* N° 396. *p.* 184.

5. A Variety of Experiments have been made, and reaſoning uſed in *England* and *France*, to prove the Truth of the common Opinion; but they do not entirely ſatisfie all the Gentlemen on the other ſide of the Queſtion. The preſent ingenious Profeſſor of Mathematicks and Philoſophy at *Utrecht* tells us in the Preface to his *Epitome Elementorum Phyſico-Mathematicorum*, publiſhed this Year, *Anno* 1726, *In corporum motum viribus ſupputandis amplexus ſum ſententiam Cl.* Leibnitſii, Hugenii, Poleni, S. Graveſandii, *& antiquæ valedixi, quam hactenus foveram, & docueram: Neque me retinuerunt argumenta doctiſſimorum virorum in* Galliis *&* Britannia *eandem defendentium. Et quando experimenta à* Poleno *&* S. Graveſandio *deſcripta examinantur & inſpiciuntur, tam manifeſto evincunt vires corporum percutientium eſſe in ratione compoſita ex quadrata velocitatum. & ſimplici maſſarum, ut illis ſubſcribere teneamur, niſi apertiſſimis contradicere ſtudeamus.* I beg leave to examine the truth of the new Opinion in the Caſe here pro-

poſed, *viz. Vires corporum percutientium*; and I ſhall endeavour to ſhew ~~from their~~ own Principles, that it cannot be true in all the Caſes of Non-Elaſtic Bodies.

It is allowed, that the common Rules of finding the Velocities of Non-Elaſtic Bodies after the Stroke are true: For thus the ingenious Mr. *S'Graveſand* tells us in Paragraph 251, of his *Supplementum Phyſicum*; *Ex hoc principio* (i. e. *multiplicando maſſam per velocitatem*) *deduxêre Philoſophi ipſas illas regulas* n. 234. *ſ.* 237. *ſ. quas nos variis modis ex principiis noſtris deduximus*; *mirum hîc quid contigit, error erroris fuit deſtructio, & duplex error ad veritatem conduxit*; *falſum de menſura virium ſecuti ſunt principium, & quod veritati etiam minimè congruum eſt, nullam vim intro premendo partes, & harum ſuperando cohæſionem corpora amittere poſuere.* Now the Rule for finding the common Velocity of Non-Elaſtic Bodies moving the ſame way after the Colliſion, is, to divide the Sum of the Quantities of Motion in the two Bodies, by the Sum of the Quantity of Matter.

It is alſo granted, *Motu duobus corporibus communi corpora hæc in ſe mutuo agere non poſſe.* Sect. 215. *Pendet ergo ictus à velocitate reſpectiva, qua manente intenſitas impactionis eadem erit, quomodocunque celeritates abſolutæ varient. Ab intenſitate hac pendet partium introceſſio, quæ ergo ſemper eadem erit, ſi duo corpora eadem velocitate reſpectiva in ſe mutuo incurrunt, quibuſcunque velocitatibus moveantur.*

Theſe Principles furniſh us with an Argument againſt the new Opinion. For if it be true, then equal Cauſes may have unequal Effects, and that in their own Senſe of an Effect: The Proof ſhall be taken from Inſtances of the Effects of the Colliſion of Non-Elaſtic Bodies, whoſe reſpective Velocities ſhall be always equal.

Let (A) and (B) ſtand for two Non-Elaſtic Bodies of equal Quantities of Matter; and let (B) be at Reſt, while (A) moves towards it with 8 Degrees of Velocity.

Here the common Velocity after the Stroke will be half the Velocity of (A) before the Stroke, *i. e.* 4 Degrees. Conſequently the Force in (B) thus communicated by the Stroke will be as its Square, or 16.

Let (B) move forward with two Degrees of Velocity, and (A) follow it with 10 Degrees; the reſpective Velocity will be 8 as before; conſequently by their own Principles quoted above, the Strokes in both Caſes are equal. The Velocity in (B) after the Stroke will be half the Sum of the Velocities before the Stroke, or 6 Degrees, by the common Rule.

According to the new Opinion, the Forces being as the Squares of the Velocities, the Force of (B) before the Stroke will be to its Force after the Stroke, as the Square of 2 is to the Square of 6; *i. e.* as 4 is to 36. Subduct the Force in (B) before the Stroke, from the Force it has after the Stroke, and you have the Degrees of Force communicated by the Stroke: Which, if this Opinion were true, would be 32, *i. e.* juſt double the Number of Degrees communicated by the ſame Force in the former Inſtance, which was but as 16. Thus equal Strokes produce unequal Effects in our ſenſe of Effects.

The following Table gives ſeveral other Inſtances. In the three firſt Columns you have the Velocities of the two Bodies both before, and after the Stroke; in the two next, you have the Forces in (B) both before, and after the Stroke; and in the ſixth, the Difference of thoſe Forces, or the different Degrees of Force effected by the ſame Stroke; and in the laſt Column, the Proportion of thoſe Forces, or Effects of the Cauſe or Stroke.

The Velocity in			The Forces in B.		Force communicated by the Stroke.	Proportion.
A	B	B				
8	0	4	0	16	16	1
10	2	6	4	36	32	2
14	6	10	36	100	64	4
18	10	14	100	196	96	6
22	14	18	196	324	128	8
26	18	22	324	484	160	10
Before		After	Before	After		
the Stroke.			the Stroke.			

If it be ſaid, that I have not conſidered the other Part of the entire Effect of the Stroke, the Intropreſſion of the Parts; I reply, this will make but a ſmall Alteration in the Matter; ſince the Intropreſſions in all theſe Caſes are equal, the relative Velocities being by Suppoſition the ſame: So that notwithſtanding, upon the whole, one and the ſame, or equal Cauſes, will produce unequal Effects.

6. The Demonſtration runs thus: “ Concipio corpus C *Fig.* 126.
“ moveri obliquè in elaſtrum L, velocitate C L ut 2, angulo
“ inclinationis C L P exiſtente 30 gr. cujus nempe ſinus C P eſt ſe-
“ miſſis radii C L. Suppono autem eam eſſe reſiſtentiam in elaſtro,
“ ut ad illud tendendum requiratur præcisè unus velocitatis gradus
“ in illo corpore, ſi perpendiculariter impingeret. Quid ergo jam
“ fiet poſt incurſionem obliquam corporis C in elaſtrum L? Quo-
“ niam motus per C L componitur, ut notum eſt, ex duobus col-
“ lateralibus per C P & P L, & cum C P, ſecundum quam cor-

Remarks upon a ſuppoſed Demonſtration, that the moving Forces of the ſame Body are not as the Velocities, but as the Squares of the Velocities. By the ſame. N° 396. *p.* 188.

Fig. 126. “pus directè impingit in elaſtrum L, exprimit dimidiam celeritatem corporis per C L, conſumetur hic motus per C P, tenſo elaſtro (perinde enim eſſet, ac ſi corpus C celeritate C P perpendiculariter incurreret in elaſtrum, quod per Hypotheſin, eam celeritatem deſtruere poſſet) remanente corporis celeritate, & directione P L. Producta igitur P L in M, ita ut L M ſit $= PL = \sqrt{3}$ (ponitur enim $CL = 2$) & applicato in M alio ſimili elaſtro faciente cum L M angulum L M Q, cujus ſinus $LQ = CP = 1$, per eandem rationem manifeſtum eſt corpus C, poſt tenſionem elaſtri L, tenſurum eſſe elaſtrum M, amiſſo motu per L Q, & ſervato motu per Q M. Prolongata itaque Q M ad N, ut fiat $MN = QM = \sqrt{2}$ ibique ſubſtituto elaſtro ſimili tertio conſtituente cum M N angulum M N R ſemirectum, quo ſcilicet M R iterum ſit $= CP = 1$; patet ſimiliter motum per M R totum impendi in tenſionem elaſtri N, corpore interim moveri pergente directione & celeritate $RN = 1$. Denique ſi hac celeritate reſidua impingat perpendiculariter in elaſtrum O, huic tendendo totam ſuam vim reliquam dabit; ipſum itaque corpus ad quietem redigetur. Hiſce ita præmiſſis, patet nunc potentiam corporis C tantam fuiſſe, ut per ſe ſolum tendere poſſit præcisè quatuor elaſtra talia, ad quæ ſingula ſeorſim tendenda requiritur dimidia velocitas corporis æqualis ipſi C, adeoque cum effectus illius quadruplo major ſit quàm effectus hujus, evidens eſt quoque vim corporis velocitate 2 grad. quadruplam eſſe vis corporis ejuſdem, vel æqualis, velocitate 1 grad.

“Haud abſimili modo demonſtrarem corpus C velocitate 3 grad. tendere poſſe 9 elaſtra, ad quorum unum tendendum unus velocitatis gradus in eo corpore requiritur, & tandem in genere numerum elaſtrorum tenſorum ſemper eſſe quadratum numeri graduum velocitatis. Unde igitur ſequetur, vires corporum æqualium eſſe in duplicata ratione celeritatum. Q E D.

1. This Argument is founded entirely on the commonly received Doctrine of the Compoſition and Reſolution of Forces, and not upon any deciſive Experiments, that have been actually made upon this Occaſion.

2. All that is proved from this Doctrine, is, that a Body moving with two Degrees of Velocity, may be made to bend 4; with 3 Degrees of Velocity it may be made to bend 9 ſimilar Springs, each deſtroying one Degree of Velocity in a perpendicular Direction, before its Force is entirely ſpent, provided you take care to alter the Directions of the Motion in every Stroke but the laſt, after a certain manner: That had the ſame Body moved but with one Degree of Velocity in one Direction, and that in a perpendicular one, it would have loſt all its Force at once, and bent but one of thoſe Springs: Which is far from proving the thing in Queſtion.

3. To make the Reaſoning on this Head concluſive, the two Bodies ſhould not only be equal in Quantity of Matter, but alike in that material Circumſtance the Direction of their Motions; ſo that if one of the Bodies move in a perpendicular Direction, the other ſhould do ſo too; or if the one ſtrikes in an oblique Direction, the other ſhould do the ſame, and that in the ſame Degree of Obliquity; and laſtly, if one moves in ſeveral Directions, the other ſhould do the ſame. But in the Caſe before us one is ſuppoſed to move but in one Direction perpendicularly, and the other to move in three oblique Directions, and but one perpendicular.

4. Let therefore the ſame Body move always in the ſame Directions, and with a ſmall Alteration, the Argument uſed in this Demonſtration will be ſo far from proving that ſide only of the Queſtion for which it was brought, that it will equally ſerve to prove the truth of the other, namely, that the Forces of the ſame Body moving with different Velocities are as thoſe Velocities.

Let therefore the ſame Body, inſtead of moving with two Degrees of Velocity, move but with one, and in the ſame Directions as above; only let the Springs be capable of deſtroying but half a Degree of Velocity in a perpendicular Direction; then by the ſame ſteps of reaſoning it will follow, that this Body will now alſo bend 4 ſimilar Springs, before its Force is ſpent; ſo that the ſame Body moving with half the Velocities, and in the ſame Directions as before, bends the ſame Number of Springs; only now the Springs make but half the Reſiſtance, that the Springs in the former Caſe made; therefore the Effect in this Caſe, according to our way of eſtimating an Effect, is but half the former Effect; conſequently the Forces producing theſe Effects are as 2 to 1: But in this *Ratio* are the Velocities, with which the Body moved in the two Caſes; therefore the Forces are as the Velocities.

Let the Body move with 3 Degrees of Velocity, and it will bend 9 ſimilar Springs, each deſtroying one Degree of Velocity in a perpendicular Direction, before the whole Force is conſumed. So alſo by the ſame way of arguing, it is as certain, that if the ſame Body move with one Degree of Velocity, it will bend 9 ſimilar Springs; each deſtroying a third Part of one Degree of Velocity in a perpendicular Direction, before its Force is extinguiſhed: So that ſtill the Effects, or Reſiſtances overcome in the ſame Directions, are, according to our way of computing, as 3 to one; and ſo alſo their Forces muſt be but in the ſame *Ratio* of 3 to 1, as were the Velocities; conſequently the Forces are as the Velocities.

5. Since therefore this Proof drawn from the Doctrine of Compoſition and Reſolution of Forces equally proves both ſides of the Queſtion, it proves too much, or in reality nothing at all; and is therefore far from deſerving the Name of a Demonſtration.

Remarks upon ſome Experiments in Hydraulics, *which ſeem to prove, that the Forces of equal moving Bodies are as the Squares of their Velocities. By the ſame.* N° 400. *p.* 343.

7. The Reſult of theſe Experiments is, That the Velocities of any Fluid, iſſuing out at equal Orifices made in the Sides of Veſſels filled up to different Heights, and kept full at thoſe Heights, above the Orifices, are found to be as the ſquare Roots of thoſe Heights reſpectively. Thus, when the different Heights above the Orifices are as the Numbers 1, 4, 9, 16, *&c.* the Velocities of the Particles of Water, iſſuing out, are found to be as the Numbers 1, 2, 3, 4, *&c.*

The Argument drawn from theſe Experiments, in Favour of the Opinion, that the Forces of equal Maſſes, or moving Bodies, are proportional to the Squares of their Velocities, runs thus. All the Particles of Water, being of the ſame Nature, and uniform, every ſingle Particle iſſuing out with two Degrees of Velocity, muſt move with 4 Times the Force of any other ſingle Particle, that moves but with one Degree of Velocity; becauſe the Force with which it moves, is the Effect of a Cauſe 4 Times greater; namely, the Preſſure of a Column of Water, whoſe Height is 4 Times greater.

Thus, again, a Particle of Water running out, with 3 Degrees of Velocity, muſt move with 9 Times the Force of a Particle moving with but 1 Degree of Velocity; becauſe that Force is the Effect of a Cauſe 9 Times greater, *viz.* the Preſſure of a Column 9 Times higher: Since no leſs than a Column 9 Times higher, is found, by Experience, neceſſary to make the ſeveral Particles of Water iſſue out with 3 Degrees of Velocity. So that, in theſe two Inſtances, it ſeems to be certain, that the Forces communicated, are as the Squares of the Velocities. And that it is ſo univerſally, is argued thus: The Preſſures are as the Altitudes, and the Altitudes as the Squares of the Velocities of every ſingle Particle; therefore the Preſſures are as the Squares of the Velocities; but the Preſſures are the Cauſes of the Forces, with which the ſeveral Particles of Water iſſue out, or move; and therefore ſince Effects are proportional to their Cauſes, the Forces with which the ſeveral Particles iſſue out, and move, are as the Squares of the Velocities.

Remark I. The Fault committed in this Reaſoning, and which quite runs through it, is the miſtaking a Part of the Effect for the Whole. The entire Effect of any of theſe Preſſures is, not barely a certain Number of Degrees of Velocity, in any ſingle Particle, but certain Degrees of Velocity in a certain Number of Particles; and that certain Number of Particles, in a given Time, is, confeſſedly, as the Degrees of Velocity.

Remark II. The entire Effect of theſe Preſſures being taken into Conſideration, ſeems to overturn this new Rule in Mechanicks for computing the Forces of moving Bodies, which is, *That the Forces are as the Quantities of Matter multiplied by the Squares of the Velocities.* And this I ſhall endeavour to make out thus: The Gentlemen who advance this new Rule, at the ſame Time that they aſſert the Velocities, in the Caſes of the Experiment above-mentioned, to be as the Square Roots of the Altitudes, do alſo confeſs, that the Quantities of the Fluid, preſſed out in equal Times, are as thoſe Velocities. For thus an ingenious Profeſſor tells us in his *Epitome Element. Phyſ. Math.* Part. 2da. Cap. iv. p. 665. " *Quantitates fluidorum ex utroque vaſe exeuntium in eodem tempore ſunt inter ſe velut celeritates, adeoque in ſubduplicata ratione altitudinum fluidorum ſupra foramina.*" Now if this be true, that the Quantities of Water flowing out in equal Times, are as the Velocities, then the Forces cannot be as the Quantities of Matter multiplied by the Squares of the Velocities: For then the Effects, inſtead of being proportional, would be more than in Proportion to their Cauſes. Thus, the Effect of a Preſſure of a Column of any Fluid, as Water, 9 Inches high, inſtead of being but 9 times greater than that of 1 Inch above the Orifice, will be no leſs than 27 Times greater. For the Velocity being at this Height triple, the Quantity of Matter in a given Time, will alſo be triple; which laſt, multiplied by the Square of the Velocity, gives 27 for the Force communicated by a Preſſure of 9 Inches in Altitude, while the Force communicated by the Preſſure of 1 Inch, is but as 1. So that the moving Forces produced will be as 27 to 1, while the Cauſes producing theſe Forces, are but as 9 to 1, *i. e.* three times too little for ſuch a Purpoſe.

Thus again, if the Velocities be as 1 and 4, the Quantities of Water iſſuing out will be as 1 and 4; but the Effects, or Forces produced, according to the new Rule, will be as 1 and 64; though the Preſſures, which communicate them, are but as the Altitudes, which are as 1 and 16. Whereas, to produce ſuch Effects, the Altitudes of the latter Column ought to have been as 64; *i. e.* 4 Times greater than by Experience it is found to be.

Remark III.

I cannot but obſerve, in the laſt Place, that the common Rule of eſtimating the Forces of moving Bodies by the Quantities of Matter multiplied by their Velocities, is rather confirmed by theſe very Experiments. For then, according to the old Maxim, Effects are proportional to their Cauſes, the Forces communicated will be as the Forces communicating, or Preſſures. Thus let the Altitude, and conſequent Preſſure of any Column of Water be 9 Times greater than the Altitude of another; then the Velocity of every ſingle Particle of Water preſſed out will be triple, and the Number of Particles iſſuing out in a given Time will likewiſe be triple; therefore the Force reſulting from theſe two multiplied together, according to the common Rule, will be 9, proportional to the Preſſure, as it ought to be. So again, if the Altitude be 16 Times greater, the Velocity will be quadruple, and the Number of the Particles quadruple, and the Force produced the Product of theſe two; *i. e.* 16, ſtill proportional to the Altitude, or Preſſure.

And univerſally, the Forces communicated, according to the old Rule, are in a Ratio compounded of two others, One of the Quantities of Matter, and the other of the Velocities: The Ratio of the Velocities, by the Experiments, is the ſubduplicate Ratio of the Heights, and the Ratio of the Quantities of Matter is, by Confeſſion, likewiſe the Subduplicate of the Heights: Therefore the Compound of theſe 2 is the *Ratio Integra*, or ſimple Ratio of the Heights; in which Ratio are the Preſſures themſelves, which produce theſe moving Forces: So that, according to the common Rule, the Effects are always proportional to their Cauſes.

After the ſame manner *S. Graveſande* reaſons in Paragraph 355 of *Phyſices Elem. Math. Edit.* 1.

Of the Proportion of Velocity and Forces in Bodies in Motion, by the Rev. Dr. Sam. Clarke. Nº 401. *p.* 382.

8. Every Effect muſt neceſſarily be proportionate to the Cauſe of that Effect; that is, to the Action of the Cauſe, or the Power exerted at the Time when the Effect is produced. To ſuppoſe any Effect proportional to the Square or Cube of its Cauſe, is to ſuppoſe that an Effect ariſes partly from its Cauſe, and partly from Nothing.

In a Body in Motion, the Force ariſing from the Quantity of the Matter as its Cauſe, muſt neceſſarily be proportional to the Quantity of the Matter: And the Force ariſing from the Velocity of the Motion as its Cauſe, muſt neceſſarily be proportional to the Velocity of the Motion. The whole Force therefore ariſing from theſe two Cauſes, muſt neceſſarily be proportional to theſe two Cauſes taken together. And therefore in Bodies of equal Bigneſs and Denſity, or in one and the ſame Body, the Quantity of Matter continuing always the ſame, the Force muſt neceſſarily be always proportional to the Velocity of the Motion. If the Force were as the Square of the Velocity, all that part of the Force, which was above the Proportion of the Velocity, would ariſe either out of Nothing, or (according to Mr. *Leibnitz*'s Philoſophy) out of ſome living Soul eſſentially belonging to every Particle of Matter.

Whenever any Effect is in a duplicate Proportion, or as the Square of any Cauſe; it is always either becauſe there are two Cauſes acting at the ſame Time, or that one and the ſame Cauſe continues to act for a double Quantity of Time.

The Reſiſtance made to a Body moving in any fluid Medium, is in a duplicate Proportion to the Velocity of its Motion; becauſe, in Proportion to its Velocity, it is reſiſted by a greater Number of Particles in the ſame Time; and again, in Proportion to its Velocity, it is reſiſted by the ſame Particles ſingly with a greater Force, as being to be moved out of their Places with greater Velocity.

Light decreaſes in a duplicate Proportion of its Diſtance from the Sun; becauſe the Rays divaricate according to two Dimenſions; according to the Dimenſion upwards or downwards, and according to the Dimenſion ſide-ways. But according to the third Dimenſion forwards from the Sun, a Ray of Light undergoes no Alteration; becauſe the Particles, of which it conſiſts, being all emitted with an equal Velocity, continue every where at equal Diſtance from each other.

One and the ſame Cauſe, acting in a double Quantity of Time, produces the ſame Effect, as two equal Cauſes acting in a ſingle Quantity of Time. One and the ſame Force, in two Parts of Time, will cauſe a Body in Motion to deſcribe the ſame Space, as double the Force would do in one Part of Time. The Space deſcribed therefore by a Body in Motion, is not as the Force; but as the Force and the Time taken together. A Body, with any the leaſt aſſignable Force, will move through infinite Space, if it meets with no Reſiſtence, in an infinite Time. And in Spaces where there is an uniform Reſiſtence to Motion, the Space deſcribed before the Motion ceaſes, muſt needs be as the Force and as the Time together: Becauſe a double Force will carry a Body twice as far in the ſame Time, and will alſo cauſe the Motion to be twice as long Time in deſtroying by an uniform Reſiſtence. The Space deſcribed therefore before the Motion ceaſes, is in this Caſe demonſtrably as the Square of the Force. A Body thrown upwards with double Force, will be carried four times as high, before its Motion be ſtopped by the uniform Reſiſtence of Gravity; becauſe the double Force will carry it twice as high in the ſame Time, and moreover require twice the Time for the uniform Reſiſtence to deſtroy the Motion. The Caſe is the ſame in accelerated Motion; in Bodies accelerated by a Succeſſion of elaſtick Impreſſions, or falling with a Motion accelerated by the uniform Power of Gravity, or by any other uniform Power whatſoever. The Space deſcribed muſt needs be as the Force, and as the Time wherein the Force operates.

What I have thus demonſtrated concerning any Force, conſidered as the Cauſe producing an Effect; and concerning the Time, during which the Force operates; is on all Hands acknowledged to be true concerning Velocity. And therefore Velocity and Force, in this Caſe, are one and the ſame Thing. So that to affirm Force to be as the Square of the Velocity, is to affirm that the Force is equal to the Square of itſelf.

Now from hence appears very clearly the Ground of the Error theſe Gentlemen have fallen into, and of their Miſapplication of the Experiments they build upon.

The Effect of a Force impreſs'd on a moveable Body, is the Motion of that Body from one Place to another. Now foraſmuch as the Effect cannot but be proportional to its Cauſe, hence Mr. *Leibnitz* (whom the other Gentlemen have followed) contends that the Space deſcribed by a Body in falling, is proportional to the Force by which it is impelled during its Fall. Which Space being agreed to be as the Square of the Velocity (as being proportional to the Velocity and to the Time taken together) hence they infer that the Force likewiſe is as the Square of the Velocity.

But from what has been ſaid, it is plain, that the Space deſcribed in theſe and all other the like Caſes, is not as the Force only, but as the Force and as the Time wherein the Force acts; that is to ſay, as the Square of the Force. For the Cauſe of the Quantity of the Space deſcribed, is not barely the Quantity of the Force, but alſo the Continuance of the Time wherein the Force acts. The Force therefore and the Time taken together, being neceſſarily as the Space deſcribed; as the Velocity and the Time taken together, are on all Hands acknowledged to be; it follows that the Velocity and the Force are equal, and not the Force as the Square of the Velocity.

When two unequal Bodies faſtened at the Ends of the Arms of a Balance of unequal Length, counterpoiſe each other, and vibrate in equal Times; as they muſt neceſſarily do, being faſtened to the Arms of the ſame Balance: Which is an Obſervation Mr. *Leibnitz* lays great Streſs upon: In that Caſe indeed the Forces will be as the Spaces deſcribed. But not therefore as the Square of the Velocities. For in that Caſe, the Velocities themſelves are as the Spaces deſcribed, becauſe the Times are equal.

When a Body projected with a double Velocity, enters deeper into Snow or ſoft Clay, or into a Heap of ſpringy or elaſtick Parts, than in Proportion to its Velocity; it is not becauſe the Force is more than proportional to the Velocity; but becauſe the Depth it penetrates into a ſoft Medium, ariſes partly from the Degree of the Force or Velocity, and partly from the Time wherein the Force operates before it be ſpent.

In the Colliſion of hard Bodies, it is (I think) agreed on all Hands, that it is demonſtrated by Reaſon, and confirmed by Experience; that when a perfectly hard Ball, moved with whatever Degrees of Velocity, ſtrikes full upon another hard Ball, equal in Bigneſs and Weight, and without any Motion in it; if the Balls be unelaſtick, they will both go on together the ſame Way, dividing the Motion equally between them, with half the Velocity the firſt Ball had originally: But if they be perfectly elaſtick, the moving Ball will communicate its whole Motion and Velocity to the quieſcent Ball, and it ſelf lie ſtill in the other's Place. Were it true now, that the Force of the moving Ball was as the Square of its Velocity; theſe Experiments would then ſhew (which is infinitely abſurd) that the Force or *vis inertiæ* in the quieſcent Ball, the dead Force, was always proportional to the Square of the Velocity (which theſe Gentlemen affect to call the living Force) of the moving Ball, whatever its Velocity were. Or the Force in both might juſt as reaſonably be ſuppoſed to be as the Cube, or the quadrato-quadrate, or any other Power of the Velocity of the moving Ball. Which is turning the Nature of Things into Ridicule.

A Contrivance to avoid the Irregularities in a Clock's Motion, occaſioned by the Action of Heat and Cold upon the Rod of the Pendulum, by Mr. G. Graham, *Watch-Maker, F.R.S.* Nº 392. *p.* 40.

III. Whereas ſeveral, who have been curious in meaſuring of Time, have taken Notice, that the Vibrations of a Pendulum are ſlower in Summer than in Winter; and have very juſtly ſuppoſed this Alteration has proceeded from a Change of Length in the Pendulum itſelf, by the Influences of Heat and Cold upon it, in the different Seaſons of the Year; with a View therefore of correcting, in ſome Degree, this Defect of the Pendulum, I made ſeveral Trials, about the Year 1715, to diſcover whether there was any conſiderable Difference of Expanſion between Braſs, Steel, Iron, Copper, Silver, *&c.* when expoſed to the ſame Degrees of Heat, as nearly as I could determine; conceiving it would not be very difficult, by making uſe of two Sorts of Metals, differing conſiderably in their Degrees of Expanſion and Contraction, to remedy, in great Meaſure, the Irregularities to which common Pendulums are ſubject. But although it is eaſily diſcoverable, that all theſe Metals ſuffer a ſenſible Alteration of their Dimenſions by Heat and Cold; yet I found their Differences, in Quantity from one another, were ſo ſmall, as gave me no Hopes of ſucceeding this Way, and made me leave off proſecuting this Affair any farther at that Time. In the Beginning of *Dec.* 1721, having Occaſion for an exact Level, beſides other Materials I made Trial of, Quickſilver was one; which, although I found it was by no Means proper for a Level, yet the extraordinary Degree of Expanſion, that I obſerved in it, when placed near the Fire, beyond what I had conceived to be in ſo denſe a Fluid, immediately ſuggeſted to me the Uſe that might be made of it, by applying it to a Pendulum. In a few Days after, I made the Experiment, but with much too long a Column of Quickſilver, the Clock going ſlower with an Increaſe of Cold, contrary to the common Pendulum; however, it was a greater Confirmation of the Advantage to be expected from it, ſince it was eaſy to ſhorten the Column in any Degree required. The only doubt I entertained, was, leſt there ſhould not be a proportional Expanſion and Contraction between the Quickſilver, and the Rod of the Pendulum, through the various Degrees of Heat and Cold, from the one Extreme to the other. To make this Experiment the more convincing, I placed the Clock in a Part of the Houſe, the moſt expoſed of any to the Changes of Heat and Cold, the Room having no Fire in it in the Winter, and expoſed to a South Sun, with Leads above it, which, in the Summer, made it extremely hot. I hung a Thermometer by it, and had likewiſe another Clock at no greater Diſtance from it, than was neceſſary to keep the Caſes from touching one another. This Clock had been made ſome Years before, with extraordinary Care, having a Pendulum about 60 Pounds in Weight, and not vibrating above one Degree and a half from the Perpendicular; and which, in a more temperate Situation, had not altered above 12 or 14″ in 24 Hours, between Winter and Summer; but in this Place it altered 30″ a Day, between the hotteſt and coldeſt Weather, in the Year 1722, a Year no way remarkable for either Extreme. But this great Alteration was owing to the Situation I mentioned above, and which I made Choice of for the ſake of making the Experiment the more ſenſible. The two Clocks being firmly ſcrewed to a Party-Wall, I began to make the firſt Trial of this Kind of Pendulum, *Dec.* 18. 1721, and by *Jan.* 3. perceiving the Pillar of Quickſilver conſiderably too long, I procured a ſhorter Glaſs, which I got ready by the eighth, and made uſe of, until the Beginning of *June* following: By which Time I was well ſatisfied of the Advantage of the Contrivance, notwithſtanding both theſe Pendulums were but rudely executed, and this laſt had the Pillar of Quickſilver too ſhort, but much nearer the true Length than the firſt. This encouraged me to provide another Glaſs, a little longer than the laſt, and to beſtow more Care upon all the Parts of the Pendulum that required Exactneſs. This being finiſhed, by the 9th of *June*, I began then to obſerve the Motion of the Clock, by the Tranſits of the fixed Stars, as often as the Weather permitted, making uſe of a Teleſcope which moved in the Plane of the Meridian; with this Inſtrument I could be ſure of not erring above two Seconds in Time. The Clock was kept conſtantly going, without having either the Hands or Pendulum altered, from the 9th of *June*, 1722, to the 14th of *Oct.* 1725, being three Years and four Months.

For the firſt Year, I wrote down every Day, the Difference between the two Clocks, with the Height of the Thermometer, not omitting the Tranſits of the Stars, as often as it was clear. The Reſult of all the Obſervations was this, That the Irregularity of the Clock, with the Quickſilver Pendulum, compared with the Tranſits of the Stars, exceeded not, when greateſt, a ſixth Part of that of the other Clock with the common Pendulum; but for the greateſt Part of the Year, not above an eighth or ninth Part; and even this Quantity would have been leſſened, had the Pillar of Mercury been a little ſhorter; for it differed a little the contrary Way from the other Clock, going faſter with Heat, and ſlower with Cold; but I made no Alteration in Length, to avoid an Interruption of the Obſervations. To confirm this Experiment the more, about the Beginning of *July*, 1723, I took off the heavy Pendulum from the other Clock, and made another with Quickſilver, but with this Difference, that inſtead of a Glaſs Tube, I made uſe of Braſs, and varniſhed the inſide, to ſecure it from being injured by the Mercury. This Pendulum I have made uſe of ever ſince, and find it about the ſame Degree of Exactneſs as the other. The Reaſon, why this kind of Pendulum is more exact than the common Sort, will be evident to any one, who conſiders, that as Heat lengthens the Rod of the Pendulum, at the ſame Time it increaſes the Length of the Pillar of Quickſilver, and its Center of Gravity is moved upwards: And when, by Cold, the Rod of the Pendulum is ſhortened, the Pillar of Quickſilver is likewiſe ſhortened, and its Center of Gravity carried downwards; by this Means, if the Column of Quickſilver be of a proper Length, the Diſtance, between the Point of Suſpenſion and the Center of Oſcillation of the Pendulum, will be always nearly the ſame, upon which the exact Motion of a Clock principally depends. Were the Pendulum of a Clock to remain invariably of the ſame Length, yet ſome little Inequalities would appear in its Motion, from the Difference of Friction ariſing from the Imperfections of the Materials, as well as different Degrees of Foulneſs; upon which Account, the Force communicated to the Pendulum, would not be conſtantly equal, which would cauſe ſome ſmall Alteration. But when the Pendulum is very heavy, and vibrates in a ſmall Arch, and the Workmanſhip of all the Parts is well performed, there will be very little Inequality in the Motion, beſides what proceeds from Heat and Cold.

In making uſe of Quickſilver for a Pendulum, by varying the Diameter of the Veſſel that contains it, or the Thickneſs of the Rod of the Pendulum, whether it be of Braſs or Steel, they may be reduced nearly to an Equality as to the receiving, or retaining the Impreſſions of Heat or Cold, upon which the greater Regularity of the Motion depends; and particular Care ought to be uſed to free the Mercury from all Blebs of Air, otherwiſe their great and ſudden Expanſion, or Contraction, may cauſe a conſiderable Diſorder; but the Air may as eaſily be excluded in this Way, as in a Barometer, and the great ſpecifick Gravity of Quickſilver, renders it a proper Material for the Weight of a Pendulum.

Of the parabolic Motion of Projectils, written in 1710, by B. Taylor, F. R. S. N° 367. p. 151. Fig. 127.

IV. Prop. I. *Vi gravitatis, ejuſque directione datis, motus corporis projecti, in medio non reſiſtente, fit in Parabolâ.*

Dem. Projiciatur corpus de loco A in directione lineæ A B, ſitque ejus trajectoria curva A C D. Ad trajectoriæ punctum quodlibet C, duc rectam C B in directione vis Gravitatis, rectæ A B occurrentem in B; atq; reſolvetur motus projectilis per A C in partes A B, B C, quarum A B oritur a motu projectionis uniformi, atque B C a vi gravitatis accelerante. Eſt ergo recta deſcripta A B tempori proportionalis ab initio motûs in A, atq; eſt B C in duplicatâ ratione ejuſdem temporis, ſicut olim demonſtravit *Galilæus*; adeoque in duplicatâ ratione rectæ A B. Cum ergo ſit B C in duplicatâ ratione rectæ A B, conſtat curvam A C D eſſe Parabolam. Q. E. D.

Prop. II. *Velocitas corporis projecti in quolibet puncto trajectoriæ, ea eſt, quam corpus acquirere poteſt cadendo per altitudinem æqualem quartæ parti parametri Parabolæ ad punctum illud.*

Dem.

Fig. 128. *Dem.* Sit Trajectoria A C D. Ad punctum quodlibet A ducantur tangens A B, & diameter A E. In tangente A B fiat A B æqualis dimidio parametri ad verticem A, & diametro A E parallela ducatur B C, trajectoriæ occurrens in C, & ad punctum C duci intelligatur tangens C G, tangenti A B occurens in F, atq; diametro A E in G. Tum ex naturâ parabolæ erunt A G, C B æquales, adeoq; & A F, F B; & quoniam eſt A B æqualis dimidio parametri ad punctum A, erit B C quarta pars ejuſdem parametri, & proinde æqualis ipſi B F. Ipſi B C proximam & parallelam duc b c, parabolæ occurrentem in c, & duc lineæ B b, parallelam C β, ipſi b c occurrentem in β. Tum quoniam ſpatium C c, adeoque & ſpatium β c, finguntur perexigua, velocitates quibus deſcribuntur erunt æquabiles quamproximè; adeoq; ſpatia B b, ſeu C β, C c, cum eodem tempore deſcribantur, erunt ut velocitates quibus deſcribuntur, & viciſſim velocitates erunt ut ſpatia. Coincidant puncta C c, atq; erunt hæ rationes accuratæ. Sed in iſto caſu propter ſimilia triangula C β c, F B C, fit C β ad β c, ſicut F B ad B C; ideoq; velocitates quibus deſcribuntur B b, β c ſunt ut F B, B C, hoc eſt, ſunt æquales. Velocitas autem, quâ deſcribitur B b, ea eſt, qua movetur projectile in puncto A, & velocitas altera qua deſcribitur β c, ea eſt quam corpus acquirit cadendo per altitudinem B C quartæ partis parametri ad punctum A. Eſt ergo velocitas projectilis in quolibet puncto A æqualis velocitati, quam corpus acquirere poteſt cadendo per altitudinem quartæ partis parametri ad punctum illud. Q. E. D.

Prop. III. *Datis velocitate & directione projectionis, invenire Trajectoriam corporis projecti.*

Fig. 129. 1. Projiciatur corpus de loco in directione rectæ A B. Duc A C in directione vis gravitatis, (hoc eſt Horizonti perpendicularem,) ejus longitudinis, ut ſit C punctum, unde corpus cadendo acquirere poteſt velocitatem in A, quâ fit projectio. Duc A F æqualem A C, angulum F A B conſtituentem cum lineâ directionis A B, æqualem angulo C A B. Duc C D perpendicularem ad A C (hoc eſt horizonti parallelam,) eiq; occurrentem F D, ipſi A C parallelam. Biſeca F D in E; atq; erit E F axis, atq; E vertex principalis Parabolæ, per quam movetur projectile. Unde deſcribetur Trajectoria per notas proprietates Parabolæ. Q. E. F.

Dem. Eſt enim A C quarta pars parametri ad verticem A. Unde conſtant cætera ex conicis.

2. Ad punctum Trajectoriæ quodvis G, duc G H ipſi A C parallelam, & ipſi C D occurrentem in H; atque erit H G altitudo, per quam corpus cadendo acquirere poteſt velocitatem, quâ movetur projectile in puncto G. Q. E. F.

Hoc item conſtat ex Prop. 2. & ex conicis.

Fig. 128. *Schol.* Si ad puncta A, & C ducantur tangentes A B, C G occurrentes rectis horizonti perpendicularibus C B, A G, in B & G;

velocitates in A & C erunt inter ſe ut tangentium partes interceptæ, A B, C G.

Prop. IV. *Unico facto experimento invenire velocitatem projectionis.*

Projiciatur corpus de loco A in directione qualibet A B, atq; obſervetur punctum percuſſum C. In directione vis gravitatis ducatur C B, ipſi A B occurrens in B, atque ipſis C B, A B, fiat tertia proportionalis L. Erit quarta pars longitudinis L. altitudo, per quam corpus cadendo acquirere poteſt velocitatem projectilis in A. Q. E. I. Fig. 128.

Dem. Eſt enim L parameter Trajectoriæ ad punctum A ; unde conſtat ſolutio per propoſitionem ſecundam.

Schol. Commodiſſimè inſtituitur experimentum, erectâ ad horizontem perpendiculari A G, & directionem ſumendo A B, quæ biſecet angulum C A G; rectâ etiam A C exiſtente horizonti parallelâ. Nam in iſto caſu altitudo quæſita æqualis eſt dimidio diſtantiæ A C.

Prop. V. *Datis directione & velocitate projectionis; invenire occurſum Trajectoriæ cum rectâ tranſeunte per punctum unde ſit projectio.*

Projiciatur corpus de loco A in directione rectæ A B. In directione gravitati contrariâ, fiat A C æqualis altitudini, per quam corpus cadendo acquirere poteſt velocitatem, quâ fit projectio, atq; ducatur C E ipſi A C perpendicularis. Fiat F A æqualis ipſi C A, atq; angulum conſtituens F A B æqualem angulo C A B. Sit A K recta, cujus occurſus cum Trajectoriâ quæritur. Duc F I ipſi A K perpendicularem, atq; ipſi C E occurrentem in D. In C E ſume E D æqualem C D, atq; ducatur ipſi C E perpendicularis E K, ipſi A K occurrens in K. Erit K punctum quæſitum. Fig. 130.

Dem. In F I productâ fiat f I æqualis F I, atq; ducantur f A, f E, F E, F K. Quoniam eſt angulus F I A rectus, atq; f I æqualis F I, eſt etiam f A æqualis F A. Sed per conſtructionem eſt F A æqualis C A, atque angulus D C A rectus. Sunt ergo puncta C, F, f ad circulum centro A deſcriptum, quem tangit recta D C in C. Sunt ergo F D, C D, f D, continuè proportionales. Sed eſt E D æqualis C D (per conſtructionem). Sunt ergo F D, E D, f D continuè proportionales; adeoque ob angulum communem ad D, triangula F E D, E f D ſunt ſimilia, atque angulus D E F æqualis angulo E f F. Puncta itaq; tria F, E, f ſunt ad Circulum, quem tangit recta E D in E. Sed quoniam eſt f I æqualis F I, atq; angulus F I K rectus, centrum iſtius circuli eſt in rectâ I K; item quoniam eſt angulus D E K rectus, centrum illud eſt etiam in rectâ E K. Eſt ergo K centrum iſtius circuli, adeoque F K æqualis eſt ipſi E K. Jam (*per Prop.* 3.) ſunt F focus Trajectoriæ, atq; C A quarta pars parametri ad punctum A. Unde cum ſit C E ad A C & E K perpendicularis, atq; F K æqualis E K, erit punctum K ad Trajectoriam (per conica). Q. E. D.

Prop. VI. *Iiſdem datis, invenire occurſum Trajectoriæ cum rectâ, quâlibet poſitione datâ.*

Fig. 131. Projiciatur corpus de loco A in directione A B, ſitq; G H recta cujus occurſus cum Trajectoriâ quæritur. Duc A C in directione gravitati contrariâ, atq; æqualem altitudini, per quam corpus cadendo acquirere poteſt velocitatem, quâ ſit projectio ; & duc A F æqualem ipſi A C, ita ut ſit angulus F A B æqualis angulo C A B ; & ducatur C E perpendicularis ipſi C A. Ducatur F I ipſi G H occurrens ad angulos rectos in I, atq; ipſi C E in D ; & in F I fiat f I æqualis F I. In C E fiat E D media proportionalis inter F D & f D ; & ipſi C E ducatur perpendicularis E K, ipſi G H occurens in K. Erit K punctum quæſitum. Q. E. I.

Dem. Conjungendo f E demonſtratur ad modum propoſitionis præcedentis.

Schol. Quoniam punctum E ſumi poteſt ad utramlibet partem puncti D, duo ſunt puncta K, k, ubi recta G H occurrit Trajectoriæ.

Prop. VII. *Datâ velocitate projectionis, invenire directionem, quæ faciat, ut Trajectoria tranſeat per punctum datum.*

Fig. 130. Projiciatur corpus de loco A, & ſit K punctum, per quod tranſire debet Trajectoria quæſita. Fiat A C, in directione gravitati contrariâ, æqualis altitudini, per quam corpus cadendo acquirere poteſt velocitatem projectionis. Ducatur C E ipſi A C perpendicularis, & ad eam duc K E perpendicularem. Centris A & K, & radiis C A, E K deſcribantur duo circuli ſibi mutuo occurrentes in F. Duc F A, & biſeca angulum C A F rectâ A B. Erit A B directio quæſita, in quâ fieri debet projectio, ut tranſeat Trajectoria per punctum K. Q. E. F.

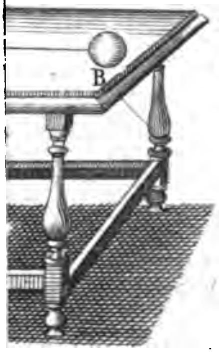
Dem. Eſt C A æqualis quartæ parti parametri ad punctum A (per Prop. 2.) Et per conſtructionem ſunt F A, C A æquales, item F K, E K. Eſt ergo F focus Parabolæ per puncta A, K, deſcriptæ. Sed illam tangit recta A B in A, propter angulos F A B, CAB æquales. Corpore itaq; projecto de puncto A, in directione A B, eâ cum velocitate, quam corpus acquirere poteſt cadendo per altitudinem C A, tranſibit Trajectoria per punctum K. Q. E. D.

N. B. Cum circulorum centris A, K, & radiis C A, E K, deſcriptorum duo ſint concurſus, F f, biſectis angulis F A C, f A C, duo etiam erunt directiones, quæ faciant, ut Trajectoria tranſeat per punctum datum K.

Prop. VIII. *Datâ directione projectionis, invenire velocitatem, quæ faciat ut Trajectoria tranſeat per punctum datum.*

Fig. 132. Projiciatur corpus de loco A in directione rectæ A B, & faciendum ſit ut tranſeat Trajectoria per punctum K. Duc A K, eamque biſeca in C, & in directione gravitatis duc C B, ipſi A B occurrentem in B ; & junge B K. Duc A D, K E, ipſi C B parallelas, & ducantur A F, K F ſibi mutuo occurrentes in F, ita ut ſint anguli F A B, D A B æquales, item F K B, E K B. Erit F A æqualis altitudini

ig. 125.



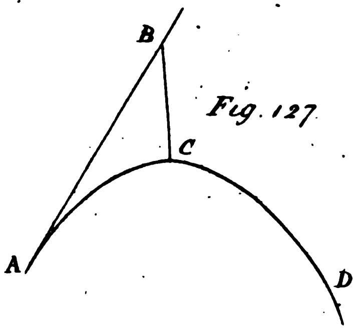
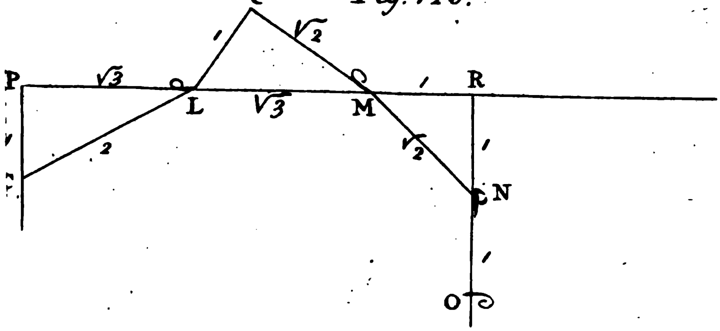
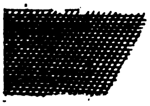


Fig. 127.

Fig. 126.





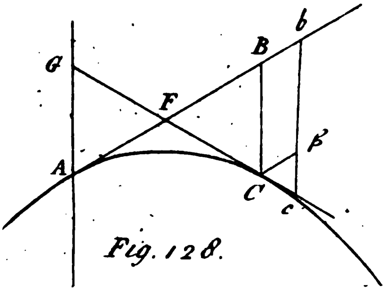


Fig. 128.

ipſâ E K, erit etiam p K minor ipſâ F K. Sed eſt p K major ipſâ f K, atque eſt f K major ipſâ F K, (quoniam eſt f A minor ipſâ [illegible] F, ita ut ſint anguli FAB, DAB æquales, item FKB, EKB. Erit FA æqualis altitudini, per quam corpus cadendo acquirere poteſt velocitatem quæſitam, quâ projectio fieri debet in directione A B, ut tranſeat Trajectoria per K. Q. E. F.

Dem. Quoniam C B eſt in directione gravitatis, eſt diameter Parabolæ; & quoniam C A æqualis eſt C K, eſt C B diameter ad ordinatam A K. Unde cum ſit A B tangens ad parabolam in A, erit etiam K B tangens ad punctum K. Hinc quoniam A D eſt in directione diametrorum, atque angulus F A B æqualis angulo D A B, tranſit A F per focum parabolæ. Eodem argumento tranſit etiam K F per focum. Eſt ergo F focus parabolæ, adeoque F A quarta pars parametri ad punctum A, quæ proinde æqualis eſt altitudini, per quam corpus cadendo acquirere poteſt velocitatem ad hoc neceſſariam, ut projecto corpore de A, in directione A B, tranſeat Trajectoria per punctum K.

Prop. IX. *Invenire velocitatem minimam & directionem ei congruam, quâ fieri poteſt, ut tranſeat Trajectoria per punctum datum.*

Projiciendum ſit corpus de loco A cum velocitate omnium mi- Fig. 133.
nimâ & directione ei congruâ, quâ fieri poteſt ut perveniat in locum K, hoc eſt ut tranſeat Trajectoria per punctum K. Ductis A C, K E in directione gravitati contrariâ, & ductâ A K, biſeca angulos C A K, EKA rectis A B, K B, ſibi mutuo occurrentibus in B. Duc B C ipſi A C occurrentem ad angulos rectos in C, atque erit C A altitudo, per quam corpus cadendo acquirere poteſt velocitatem quæſitam; eritque A B directio quæſita. Q. E. F.

Dem. Ducatur B F ipſi A K occurrens ad angulos rectos in F, atque occurrat C B ipſi K E in E. Propter angulos C A B, B A F, item angulos E K B, B K F, æquales, atque angulos rectos in C, E, & F, erunt æquales C A, F A, item E K, F K. Hinc conſtat puncta A, K eſſe ad parabolam, quam tangit recta A B in A, cujuſque parameter ad punctum A eſt quadruplum altitudinis C A, foco exiſtente F. Corpore itaque projecto de A in directione A B, eâ cum velocitate, quam corpus acquirere poteſt cadendo per altitudinem C A, Trajectoria erit dicta Parabola (per Prop. 2.) Dico autem illam eſſe velocitatem minimam, ſeu eſſe C A partem quartam parametri omnium minimæ, quâ Parabola deſcribi poteſt, quæ tranſeat per puncta, A, K.

Si fieri poteſt, in C A capiatur altitudo c A minor, quæ ſit quarta pars parametri ad punctum A. Duc ipſi C A perpendicularem c e, ipſi K E occurrentem in e, & centro A & radio A c, deſcribatur circulus ipſi A K occurrens in f. Quoniam c A dicitur quarta pars parametri ad punctum A, focus Parabolæ erit punctum aliquod p, in circumferentia circuli c p f, centro A & radio A c deſcripti. Si ergo ſit punctum K ad parabolam illam, erit p K æqualis e K. Eſt vero F K æqualis E K. Unde cum ſit e K minor ipſâ E K, erit etiam p K minor ipſâ F K. Sed eſt p K major ipſâ f K, atque eſt f K major ipſâ F K, (quoniam eſt f A minor ipſâ

F A per hyp.) unde ſit p K major ipſâ F K. Sed jam dicebatur p K minor ipſâ F K; quæ repugnant. Nequit ergo Parabola deſcribi, quæ tranſeat per puncta A, K, minori parametro quam in ſolutione definitum eſt. Q. E. D.

Prop. X. *Datâ velocitate projectionis, invenire directionem, quæ faciat, ut corpus projiciatur ad diſtantiam omnium maximam in plano dato; atque diſtantiam illam definire.*

Fig. 134. Sit planum datum A K atque invenienda ſit diſtantia maxima A K, ad quam corpus projici poteſt in plano illo.

Duc A C in directione gravitati contrariâ, æqualem quartæ parti parametri ad punctum A. Tum biſecto angulo C A K rectâ A B, erit A B directio projectionis quæſita. Duc C B ipſi C A perpendicularem, rectæ A B occurrentem in B, atque in C B productâ fiat B E æqualis ipſi B C. Tum ductâ E K, ipſi C A parallelâ, quæ occurrat plano A K in K, erit A K diſtantia maxima quæſita.

Dem. Centro A & radio A C deſcribe circulum, ipſi A K occurrentem in F, & ducantur B F, B K. Quoniam anguli CAB, BAF ſunt æquales (per conſtructionem) atque A F æqualis C A, erit B F æqualis C B, æqualis B E (per conſtructionem) atque anguli ad F recti. Unde etiam ſit F K æqualis E K. Sunt ergo puncta A, K ad parabolam foco F deſcriptam, quam tangit A B in A (propter angulos CAB, FAB æquales) quartâ parte parametri ad punctum A exiſtente C A. Corpore igitur projecto de loco A, in directione A B, eâ cum velocitate, quam corpus acquirere poteſt cadendo per altitudinem C A, Trajectoria tranſibit per punctum K (per Prop. 2.) Q. E. D.

Dico autem, quod ſit K A diſtantia omnium maxima, ad quam corpus projici poteſt de loco A eâdem cum velocitate.

Si fieri poteſt, eâdem parametro, ad A deſcribatur parabola, quæ tranſeat per punctum diſtantius k, hoc eſt, projiciatur corpus ad diſtantiam majorem k A. Duc B k, atque ipſi KE parallelam k e, ipſi C E occurrentem in e. Quoniam F B, E B, item F K, E K, ſunt æquales, ſunt etiam anguli F B K, E B K æquales. Angulus ergo F B k major eſt angulo k B e; unde ſit k F major ipſâ k e. Sed quoniam eſt A C quarta pars parametri ad punctum A, focus parabolæ erit alicubi in circumferentiâ circuli centro A, & radio C A deſcripti. Sit focus ille p, & ducatur p k. Tum quoniam p k major eſt ipſâ F k, erit etiam p k major ipſâ k e. Sed ut parabola tranſeat per punctum k, debet eſſe p k æqualis k e. Nequit ergo parabola duci in circumſtantiis propoſitis, quæ tranſeat per punctum k diſtantius puncto K; adeoque nec corpus projici ad diſtantiam majorem ipſâ K A. Q. E. D.

Prop. XI. *Iiſdem poſitis, invenire locum puncti* K, *ſeu Curvam deſcribere, quæ tangat omnes parabolas eodem vertice* A *&* *eâdem parametro deſcriptas.*

Fig. 135:

Sit A vertex datus, atque in directione gravitati contrariâ ducatur A C æqualis quartæ parti parametri datæ. Tum deſcriptâ parabolâ, cujus vertex principalis ſit C, atque focus A; erit ea curva quæſita.

Dem. Duc quamlibet A K, atque in eâ ſume F A æqualem C A, & ducatur C B ad C A perpendicularis, ſitque K punctum in propoſitione præcedente inventum. In A C productâ, factâ C c æquali C A, ducatur c e parallela ipſi C E; ducatur etiam K E parallela ipſi A C, ipſis C E, c e occurrens in E & e. Per propoſitionem præcedentem eſt K E æqualis ipſi F K; unde cum ſit etiam F A æqualis ipſi AC, æqualis ipſis C c, E e (per conſtructionem) eſt ergo K e æqualis K A; unde eſt punctum K ad parabolam foco A & vertice principali C deſcriptam. Q. E. D.

Biſecto autem angulo A K E à rectâ K B, tanget hæc utramque parabolam, tam foco F per A & K, quam foco A per K deſcriptam. Unde ſe mutuo tangunt parabolæ. Q. E. D.

Experiments relating to the Reſiſtance of Fluids, March 30*th*, 1721. *By the Rev.* J. T. Deſaguliers, LL. D. *F. R. S.* Nº 367. *p.* 142.

V. I took a Ball of Gold of an Inch in Diameter, that had a little Stem of the ſame Metal, with a place on it to faſten a String to; and having ſuſpended it by a ſilken Thread too ſtrong to lengthen by ſtretching, I made the Diſtance between the Center of the Ball, and the Point of Suſpenſion equal to 12,5 Inches, then cauſing the Ball to vibrate in a Trough full of Water, (which had an upright Piece of Wood in the middle of one ſide with Pins or Keys from which the Ball hung, that the Center of Suſpenſion might always be in the ſame Place) I obſerved by looking from a Pin on one ſide of the Trough to a Mark made oppoſite to it on the other ſide, whereabouts the String of the *Pendulum* (juſt above the Surface of the Water, in which the Ball was quite immerſed) went after 14 Vibrations, and by another Pin and oppoſite Mark, alſo obſerved where it went to, after 28 Vibrations. Taking out the Water, I filled the Trough with Mercury, the length of the *Pendulum*, Point of Suſpenſion and all other things remaining as before: Then letting go the Ball in the Mercury from the ſame Place whence it was let down when the Trough was full of Water; (which was marked by a String ſtretched a-croſs to prevent Miſtakes) after one whole Vibration, it came very little ſhort of the ſame Mark as it had come to in Water after fourteen Vibrations; and when it vibrated twice in Mercury, it came to the ſame Place it had done after between 26 and 28 Vibrations in Water; and this it did exactly ſeveral times.

Afterwards filling an upright Copper Pipe of four Inches Diameter with Mercury to the Height of 3 Foot 10 Inches, and ſuſpending the golden Ball in it by a ſhort String about an Inch long, ſo as to have the Ball juſt immerſed under the middle of the Surface of the Mercury; I cauſed it to be let down ſuddenly, and obſerving how long it was falling down to the Bottom of the Tube, I found that

the Experiment was diſturbed by the Ball's ſtriking againſt the Sides of the Tube, which retarded the Fall of the Ball, and the more ſo the oftner the Ball ſtruck. When the Ball was leaſt retarded, it was only 2 ½ Seconds in falling, which muſt be taken as the true Time of the Fall of the Ball in a Height of Quickſilver equal to 3 Foot 10 Inches; becauſe when I tryed the Experiment again at home the firſt Day of *April* following, the Ball fell in the Mercury once or twice without ſtriking the ſides of the Tube at all, but not in leſs time than 2 ½ Seconds.

I alſo repeated the other Experiments at Home, making the Golden *Pendulum* 39,2 Inches long, ſo as to make it vibrate but once in a Second, and then I found that it would vibrate 5 or 6 times in the Mercury before the Vibrations became ſo ſmall as not to be obſerved; and then the firſt Vibration in the Mercury ended very near where the 14th in Water had done; the ſecond in Mercury ended where the 27th in Water had done, and obſerving the third Vibration in Mercury, it ended exactly at the Mark where the 40th in Water ended; and this was obſerved by ſeveral Perſons as well as myſelf.

Then I weighed 14 Penny-weight of the Mercury (in which I made the Experiments) firſt in the Air, then in Water, where it loſt only one Penny-weight and one Grain of its Weight; that is, it weighed in Air 336 Grains, and in Water 311, ſo that its ſpecifick Gravity was to that of Water as 13,44 to 1.

As to the golden Ball which had Varniſh and Cement upon it to keep the Mercury from ſinking into it, I found it to weigh as follows,

	Ounces	dwt.	gr.	
It weighed in Mercury	1	00	18	or 498 gr.
in Water	5	01	00	or 2424 gr.
in Air	5	07	09	or 2577 gr.

I took the Wire and *Pendulum* of a long *Pendulum* Clock, and having faſtened the golden Ball at the end of the Wire under the pendulous Weight that ſerved for the Clock, in order to make the Vibrations of the golden Ball in the Mercury continue longer, I did not find it to keep on the Motion above one Swing or two the longer for that Help; neither did a round Ball of Lead placed upon the ſaid Wire, juſt above the Surface of the Mercury, help any more; and as I found ſome Inconveniences in theſe two laſt ways of making the Experiment, I rather chuſe to rely upon thoſe made with the golden Ball hanging by a ſilken Thread of 39,2 Inches long, meaſuring from the Point of Suſpenſion to the Center of the Ball.

A Propoſition on the Balance, *not taken Notice of by Mechanical Writers, explained and confirmed by an Experiment. By the ſame.* N°407.*p.*128. Fig. 136.

Theor. VI. A B is a Balance, on which is ſuppoſed to hang at one End B the Scale E with a Man in it, who is counterpoiſed by the Weight W hanging at A, the other End of the Balance. I ſay, that if ſuch a Man, with a Cane or any rigid ſtreight Body, puſhes upwards againſt the Beam any where between the Points C and B (provided he does not puſh directly againſt B) he will thereby make himſelf heavier, or over-poiſe the Weight W, though the Stop G G hinders the Scale E from being thruſt outwards from C towards G G. I ſay likewiſe, that if the Scale and Man ſhould hang from D, the Man by puſhing upwards againſt B, or any where between B and D (provided he does not puſh directly againſt D) will make himſelf lighter, or be over-poiſed by the Weight W, which did before only counterpoiſe the Weight of his Body and the Scale.

If the common Center of Gravity of the Scale E, and the Man ſuppoſed to ſtand in it be at k, and the Man by thruſting againſt any Part of the Beam, cauſe the Scale to move outwards ſo as to carry the ſaid common Center of Gravity to k x, then inſtead of B E, L l will become the Line of Direction of the compound Weight, whoſe Action will be encreaſed in the Ratio of L C to B C. This is what has been explained by ſeveral Writers of Mechanicks; but no one, that I know of, has conſidered the Caſe when the Scale is kept from flying out, as here by the Poſt G G, which keeps it in its Place, as if the Strings of the Scale were become inflexible. Now to explain this Caſe, let us ſuppoſe the Length B D or half of the Brachium B C to be equal to 3 Feet, the Line B E to 4 Feet, the Line E D of 5 Feet to be the Direction in which the Man puſhes, D F and F E to be reſpectively equal and parallel to B E and B D, and the whole or abſolute Force with which the Man puſhes, equal to (or able to raiſe) 10 Stone. Let the oblique Force E D (= 10 Stone) be reſolved into the two E F and E B, (or its Equal F D) whoſe Directions are at right Angles to each other, and whoſe reſpective Quantities (or Intenſities) are as 6 and 8, becauſe E F and B E are in that Proportion to each other, and to E D. Now ſince E F is parallel to B D C A, the Beam, it does no way affect the Beam to move it upwards; and therefore there is only the Force repreſented by F D, or 8 Stone to puſh the Beam upwards at D. For the ſame Reaſon, and becauſe Action and Reaction are equal, the Scale will be puſhed down at E with the Force of 8 Stone alſo. Now ſince the Force at E pulls the Beam perpendicularly downwards from the Point B, diſtant from C the whole Length of the Brachium B C, its Action downwards will not be diminiſhed, but may be expreſſed by $8 \times \overline{BC}$: Whereas the Action upwards againſt D will be half loſt, by reaſon of the diminiſhed Diſtance from the Center, and is only to be expreſſed by $8 \times \frac{\overline{BC}}{2}$; and when the Action upwards to raiſe the Beam is ſubſtracted from the

Action downwards to depreſs it, there will ſtill remain 4 Stone to puſh down the Scale; becauſe $\overline{8 \times BC} - 8 \times \frac{\overline{BC}}{2} = 4\,BC$. Conſequently a Weight of 4 Stone muſt be added at the End A to reſtore the Æquilibrium. *Therefore a Man*, &c. *puſhing upwards under the Beam between* B *and* D, *becomes heavier.* Q. E. D. On the contrary, if the Scale ſhould hang at F from the Point D, only 3 Feet from the Center of Motion C, and a Poſt g g hinders the Scale from being puſhed inwards towards C, then if a Man in this Scale F puſhes obliquely againſt B with the oblique Force above-mentioned; the whole Force, for the Reaſons before given (in reſolving the oblique Force into two others acting in Lines perpendicular to each other) will be reduced to 8 Stone, which puſhes the Beam directly upwards at B, while the ſame Force of 8 Stone draws it directly down at D towards F. But as CD is only equal to half of CB, the Force at D compared with that at B, loſes half its Action, and therefore can only take off the Force of 4 Stone from the Puſh upwards at B; and conſequently the Weight W at A will preponderate, unleſs an additional Weight of 4 Stone be hanged at B. *Th[illegible]fore a Man*, &c. *puſhing upwards under the Beam between* B *and* D *becomes lighter.*

Schol. I. Hence knowing the abſolute Force of the Man that puſhes upwards, (that is, the whole oblique Force) the Place of the Point of Truſion D, and the Angle made by the Direction of the Force with a Perpendicular to the Beam at the ſame Point, we may have a general Rule to know what Force is added to the End of the Beam B in any Inclination of the Direction of the Force or Place of the Point D.

RULE *for the firſt Caſe.* Firſt find the perpendicular Force by the following Analogy, whoſe Demonſtration is known to all that underſtand the Application of oblique Forces.

As the Radius:
To the right Sine of the Angle of Inclination::
So is the oblique Force:
To the perpendicular Force.

Then the perpendicular Force multiplied into the Length of the Brachium BC, *minus* the ſaid Force multiplied into the Diſtance DC, will give the Value of the additional Force at B, or of the Weight required to reſtore the Æquilibrium at A.

Or to expreſs it in the Algebraical Way. Let o f expreſs the oblique Force, *p f* the perpendicular Force, and x the Force required, or Value of the additional Weight at A to reſtore the Æquilibrium.

DE:

$$DE : DF :: of : pf$$

$$\overline{pf \times BC} - \overline{pf \times DC} = x$$

The ſame Rule will ſerve for the ſecond Caſe, if the Quantity found be made Negative, and the additional Weight ſuſpended at B. Or having found the Value of the Perpendicular Force, the Æquation will ſtand thus $-\overline{pf \times BC} + \overline{pf \times DC} = -x$, and conſequently the additional Weight muſt be hanged at B; becauſe $-x$ at A is the ſame as $+x$ at B.

Schol. II. Hence it follows alſo, that if, in the firſt Caſe, the Point of Truſion be taken at C, the Force at B, (or Force whoſe Value is required) will be the whole Perpendicular Force; becauſe C D is equal to nothing: And if the Point D be taken beyond C towards A: The Perpendicular Force puſhing upwards at that Point, multiplied into D C, muſt be added to the ſame Force multiplied into B C, that is $\overline{pf \times BC} + \overline{pf \times DC} = x$.

The Machine I made uſe of to prove this Experimentally, was as follows. Fig. 137.

The Braſs Balance A B is 12 Inches long, moveable upon the Center C, with a Perpendicular Piece B b hanging at the End B, and moveable about a Pin at B, and ſtopped at its lower End b (by the upright Plate G G) from being thruſt out of the Perpendicular by the puſhing Pipe F E, whoſe lower Point being put into a little Hole at H, the upper Wire or Point (when put into another little Hole under the Beam at D) is by Means of the Worm-ſpring E F preſſing againſt the Plug E to drive forwards the ſaid Wire h D, made to puſh the ſaid Beam upwards with the Force of the Spring. T S S is a Stand, to which is fixed the Pillar T C that ſuſtains the Balance; and it has alſo a Slit S S to receive a Shank of the moveable Plate G G, to be fixed in any Part of the Slit by a Screw underneath.

Experiment. Hang on B b, as in the Figure. Then let E F be ſo applied to the Hole H, that its upper Wire h D k may go through a little Loop at D ſo as not to thruſt the Beam upwards, but be in the ſame Poſition as if it did, that by hanging on the Weight W, the Brachium B C with B b and F E may be counterpoiſed; and then the Action againſt D and H may be eſtimated without the Weight of the puſhing Pipe.

Then drawing down the End of the Wire k, thruſt it into the little Hole under D, and B will be ſo pulled downwards as to require the additional Weight of 4 Ounces to be hung on at A to reſtore the Æquilibrium, when B H is 4 Inches, B D 3 Inches, and the whole Force of the Spring equal to 10 Ounces.

I need not here ſay, that for explaining the ſecond Caſe, B b is to be ſuſpended at D, with the Plate G G fixed to ſtop it at the Place M to keep it from being puſhed towards T, and that the upper End of G F E D k muſt puſh into an Hole made under B, in which Caſe the Weight B muſt be hanged at B to reſtore the Æquilibrium.

P. S. To ſhew experimentally that the Force which the Spring exerts in this oblique Truſion is equal to 10 Ounces: Take the Beam A B, which weighs 4 Ounces, from its Pedeſtal C T, and having ſuſpended at each End, A and B 3 Ounces, ſupport it under its Center of Gravity by the puſhing Pipe E F ſet upright under it, and you will find that the Beam with the two Weights will thruſt in the Wire k h as far as h, the Place to which the oblique Truſion drives it.

An Experiment explaining a mechanical Paradox, that two Bodies of equal Weight ſuſpended on a certain ſort of Balance do not loſe their Æquilibrium, by being removed one farther from, the other nearer to the Center. By the ſame. N° 419. *p.* 125. Fig. 138.

Prop. VII. If the two Weights P, W, hang at the Ends of the Balance A B, whoſe Center of Motion is C; thoſe Weights will act againſt each other (becauſe their Directions are contrary) with Forces made up of the Quantity of Matter in each multiplied by its Velocity; that is, by the Velocity which the Motion of the Balance turning about C will give to the Body ſuſpended. Now the Velocity of a heavy Body is its perpendicular Aſcent or Deſcent, as will appear by moving the Balance into the Poſition a b; which ſhews the Velocity of P to be the perpendicular Line e a, and the Velocity of B will be the perpendicular Line b g: For if the Weights P and W are equal, and alſo the Lines e a and b g, their *Momenta* made up of e a multiplied into W, and b g multiplied into P, will be equal, as will appear by their deſtroying one another in making an Æquilibrium. But if the Body W was removed to M, and ſuſpended at the Point D, then its Velocity being only f d, it would be over-balanced by the Body P; becauſe f d multipled ininto M, would produce a leſs *Momentum* than P multiplied into b g.

As the Arcs A a, B b, and D d deſcribed by the Ends of the Balance or Points of Suſpenſion are proportionable to their Sines e a, g b, and d f, as alſo the *Radii* or Diſtances C A, C B, and C D; in the Caſe of this common ſort of Balance, the Arcs deſcribed by the Weights, or their Points of Suſpenſion, or the Diſtances from the Center may be taken for Velocities of the Weights hanging at A, B, or D; and therefore the acting Force of the Weights will be reciprocally as their Diſtances from the Center.

Scholium. The Diſtances from the Center are taken here for the Velocities of the Bodies, only becauſe they are proportionable to the Lines e a, b g, and f d, which are the true Velocities. For there are a great many Caſes wherein the Velocities are neither proportionable to the Diſtances from the Center of Motion of a Machine, nor to the Arcs deſcribed by the Weights or their Points of Suſpenſion. Therefore it is not a general Rule, that Weights act in Proportion to their Diſtances from the Center of Motion; but a Corollary of the general Rule, that Weights act in Proportion

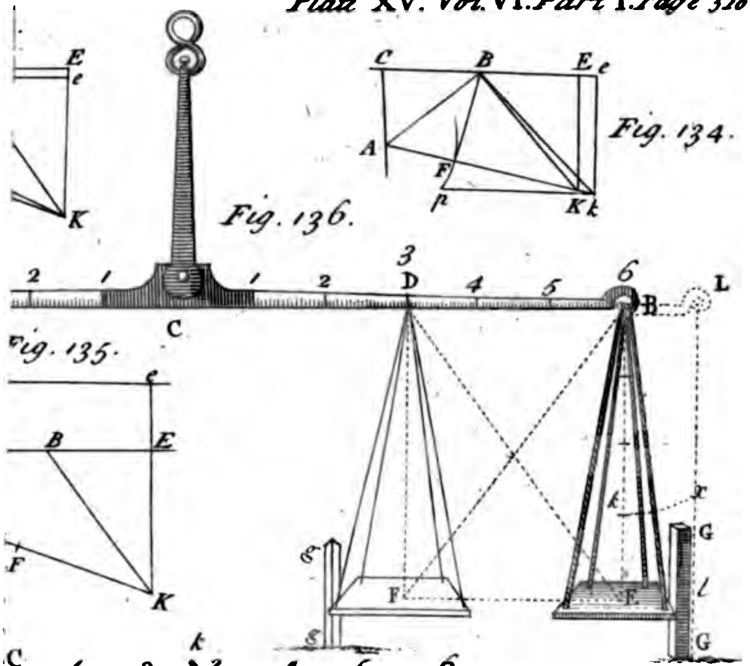
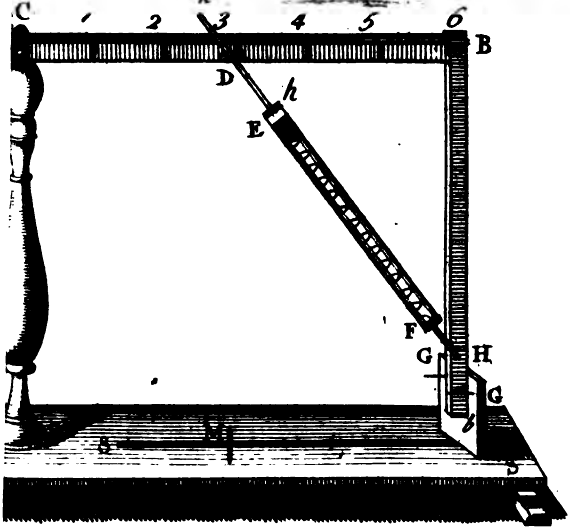


Fig. 134.

Fig. 136.

Fig. 135.



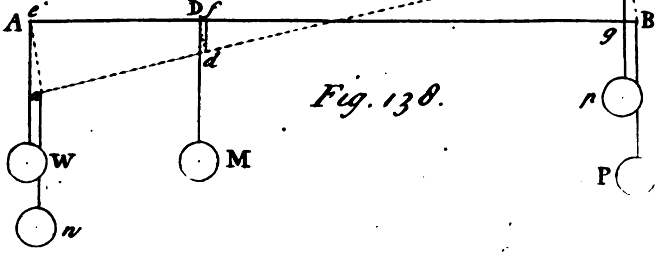


Fig. 138.

tion to their true Velocities, which is only true in ſome Caſes. Therefore we muſt not take this Caſe as a Principle, which moſt Workmen do, and all thoſe People who make Attempts to find the perpetual Motion, as I have more amply ſhewn in the *Philoſophical Tranſactions*, N° 369.

But to make this evident even in the Balance, we need only take Notice of the following Experiment. ACBEKD is a Balance in the Form of a Parallelogram paſſing through a Slit in the upright Piece NO ſtanding on the Pedeſtal M, ſo as to be moveable upon the Center Pins C and K. To the upright Pieces AD and BE of this Balance are fixed at right Angles, the horizontal Pieces FG and HI. That the equal Weights P, W, muſt keep each other in *Æquilibrio*, is evident; but it does not at firſt appear ſo plainly, that if W be removed to V, being ſuſpended at 6, yet it ſhall ſtill keep P in *Æquilibrio*; though the Experiment ſhews it. Nay, if W be ſucceſſively moved to any of the Points 1, 2, 3, E, 4, 5, or 6, the *Æqulibrium* will be continued; or if, W hanging at any of thoſe Points, P be ſucceſſively moved to D, or any of the Points of Suſpenſion on the croſs Piece FG, P will at any of thoſe Places make an *Æquilibrium* with W. Now when the Weights are at P and V, if the leaſt Weight that is capable to overcome the Friction at the Points of Suſpenſion, C and K be added to V, as u, the Weight V will overpower, and that as much at V as if it was at W.

From what we have ſaid above, the Reaſon of this Experiment will be very plain.

As the Lines AC and KD, CB and KE always continue of the ſame Length in any Poſition of the Machine, the Pieces AD and BE will always continue parallel to one another, and perpendicular to the Horizon: However, the whole Machine turns upon the Points C and K; as appears by bringing the Balance to any other Poſition, as abed: And therefore as the Weights applied to any Part of the Pieces FG and HI can only bring down the Pieces AD and BE perpendicularly, in the ſame Manner as if they were applied to the Hooks D and E, or to X and Y, the Centers of Gravity of AD and BE; the Force of the Weights (if their Quantity of Matter is equal) will be equal; becauſe their Velocities will be their perpendicular Aſcent or Deſcent, which will always be as the equal Lines 4 l and 4 L, whatever Part of the Pieces FG and HI the Weights are applied to. But if to the Weight at V be added the little Weight u, thoſe two Weights will overpower, becauſe in this Caſe the *Momentum* is made up of the Sum of V and u multiplied by the common Velocity 4 L.

Hence follows, that it is not the Diſtance c 6 multiplied into the Weight V, which makes its *Momentum*; but its perpendicular Velocity L 4 multiplied into its Maſs. Q. E. D.

This is ſtill further evident, by taking out the Pin at K; for then the Weight P will over-balance the other Weight at V, becauſe then their perpendicular Aſcent and Deſcent will not be equal.

Obſervations on the Crane, with Improvements, by the ſame. N° 411. *p.* 194. Fig. 140.

VIII. When great Weights are to be raiſed from a great Depth, and laid on Carriages very near the Precipice, as at the Edge of a Stone Quarry, the Crane muſt be a fixed one, and only the Gibbet moveable, from which the Weight hangs. Here, in the common Way, the Rope R r r, or Chain, which runs over the Gibbet, goes between two Pullies P, Q, fixed within the upper horizontal Beam of the Crane A Q T X, above the Axis of the Gibbet B G V, ſo as to be carried eaſily to the Right or Left Hand, from W to w, when the Gibbet turns upon its Axis to bring the Burthen over the Carriage deſign'd to receive it. For this Purpoſe a ſmall Rope, called the Guide-Rope, is faſtened to the Weight, or to the upper Part of the Gibbet near its Extremity, g, which a Man is to pull to bring the Weight over the Place, to which it muſt be lowered. Now in performing this, the main Rope or Chain not continuing parallel to the Arm of the Gibbet, gives the Weight a Tendency towards that Side to which it deviates, and that ſometimes ſo ſuddenly, that without Care, and much Force applied, if the Weight be very great, the Burthen will ſwing to or from the Carriage, ſo as to break every thing in its Way. Sometimes a horizontal Piece, like a Handſpike, is fixed in the upright Shaft of the Gibbet a little above B, to turn it by; but in that Caſe too the Force is unequal, as the Weight is carried round; ſo that the Lives of the Men that are loading, often depend upon the Care of the Man who guides the Weight, by either of the Means above-mentioned.

N. B. No Situation of the Pullies can prevent this; and we find Accidents to happen every Day, as will appear by the Examination of *Fig.* 142.

But if upon the Axis of the Gibbet there be fixed an Iron Wheel, y, with many Teeth, to be carried round by a Pinion, u, of a few Leaves, upon the End of whoſe Axis is faſtened a Wheel, x, with Arms (that Axis going through the perpendicular Piece T Z behind the Shaft of the Gibbet) a Man ſtanding at that Wheel is out of Harm's Way, and has ſuch an Advantage of Power as to hold the Weight ſteady in any Place required, notwithſtanding its Tendency to ſwing, as mentioned above, which is not felt at the Ends of the Arms of this laſt Wheel. The firſt who made Uſe of this Contrivance is Mr. *Ralph Allen*, Poſt-maſter of *Bath*, at his Stone-Quarry, where the Weight raiſed is 4 or 5, and ſometimes 6 or 7 Ton.

I need not ſay that the Power to bring up the Weight works here by Means of a Capſtane, or upright Shaft, R O, drawn round by Horſes, that the Weight may come up more expeditiouſly, though in the Figure the Handſpikes, f, e, b, going in at ſuch a Hole as d, ſhew that Men may work it upon Occaſion.

The ſame Gentleman having laid his Stone on Waggons of a peculiar Make, cauſes it to run down Hill about a Mile and a half, on a wooden Waggon-way to the River-ſide, where he has a Wharf, and there by another Crane exactly ſuited to the Work, he takes the Stone from the Carriages, and with great Expedition lets it down into the Barges or Veſſels that come to fetch it.

This Crane is of the Sort which is commonly called a Rat's-Tail Fig. 146. Crane, moving round a ſtrong Poſt like a Wind-mill, ſo that it may turn quite round with all its Load. The Axel B b, on which the Rope winds, is here horizontal like a Winch; but to gain Strength, inſtead of the walking Wheel C A, it is carried round by a ſtrong Fig. 144, 145. Wheel and Pinion, or is in Effect a double Axis in Peritrochio. Now in the common Cranes of this Kind, there is only a Catch (as E K A,) Fig. 144. to hold the Burthen at the Height it is brought up to, whilſt the Crane is turned round in order to have the Weight lowered into the Veſſels, which is done by lifting up the Catch, and being ready to let it down again as Need requires. Sometimes a half Circumference of Wood (D I I B,) is held hard againſt a wooden Wheel W w, on the Axel, to regulate and govern the Deſcent of the Weight. But as in either of theſe Caſes, if the Man at the Crane is careleſs, very bad Accidents happen. Mr. *Padmore*, Mr. *Allen*'s chief Work-man, has made ſuch a Contrivance, that the Pall or Lever whereby the Axel is preſſed to direct the deſcending Motion, does ſo communicate with the Catch, that in Caſe the Man that ought to manage it, ſhould careleſly let it go, the Catch always takes, and thereby all Accidents are prevented; as will be ſhewn in the Explanation of the 144th and 145th Figures.

Where Goods are to be raiſed high, as in unloading Veſſels, and alſo to be let down deep, as in loading them; (that is, where both the former Operations are to be performed) if the Weights do not exceed two or three Ton, and many Hands are not to be had, then an endleſs Screw turned by an Handle at each End (in an oppoſite Situation, or with one Handle and a Balance to it) leading an Axis in Peritrochio, or as it is commonly called, a Worm and Wheel applied to a Crane, with a Gibbet, is moſt uſeful: For the Teeth of the Wheel are pulled by the Weight ſo directly againſt the Thread of the Worm in its Endeavour to deſcend, that one may leave the Handle in any Poſition where it will ſtop, without any Catch, or the leaſt Danger of the Weight falling back again.

But then, if you would have the Weight to be let down, to deſcend pretty quick, which cannot be performed by applying the Hand to the Handle, which goes through a great Space in Compariſon to the Space deſcribed by the Weight (without which ſufficient Force would be wanting) only give the Handle a Swing, and if

the

the Worm be well oiled, the Handle and its Counterpoiſe, or the two Handles, will perform the Office of a Fly in the common Jack, turning very faſt round, and regulating the Motion of the Weight, which from that Impulſe will deſcend continually, and not too faſt, like the Weight of a Jack.

The Way to ſtop this Motion at any Time, is to graſp the Axis of the Screw hard, betwixt the Screw and the Handle in its round Part. The Hand is ſufficient to do it, and will ſtop it in two or three Turns.

The worſt Cranes are thoſe where Men walk in a large Wheel, by reaſon of Accidents that happen daily on account of the ſhort Space between a Man's two Feet. This may be prevented by uſing four-footed Animals, the Length of whoſe Bodies makes a Baſe of ſufficient Length to keep the Wheel from running back. Fig. 146.

An Explanation of the Figures.

Fig. 140. Repreſenting a fixed Crane with a Gibbet moving on an upright Shaft or Axis.

A a Q, The Roof of the Crane to preſerve the Rope R T r from the Weather, when the Arm of the Gibbet V G g being turned towards Y is brought under it.

A T, The upper Piece of the Crane, in a horizontal Situation, called the Plate of the Crane.

X, Y, Z, The three Crane Poſts braced at Top and Bottom.

D S, M N, I E, Three Cills within the Stone Work, braced with Wood, and made faſt with an upright Plate of Iron pinned to the Wood on each Side.

N. B. *When the Crane is not in Stone Work, the three Cills muſt be all in one Piece, reaching from* D to E.

H I, h E, Are the Braces of the main Poſt of the Crane, which come up above the Level of the Wharf L w B, which are longer and ſtronger than the others. Here a croſs Piece whoſe Section is (&) keeps the main Poſt from twiſting.

R O, The Capſtane, or Shaft of the Crane to receive the Rope or Chain; which Shaft is turned here by Bars or Handſpikes, ſuch as b d, f d, or e d, the lower Part being ſtrengthened with Iron Hoops above and below the Holes at d, with a Pivot or Iron Axis turning in a Hole in a Piece whoſe Section is F.

p p, Are two Pins, which hold on a Collar in which the upper Part of the Shaft turns.

C B, The Shaft or Axel of the Gibbet with Pivots and Iron Hoops at Top and Bottom, and a Wheel of Iron, y, having Teeth perpendicular to its Plane. This Wheel is led by a Pinion, u, which is on the Axis of the Wheel x, by whoſe Arms a Man ſtanding at H may bring about the End of the Gibbet g with the

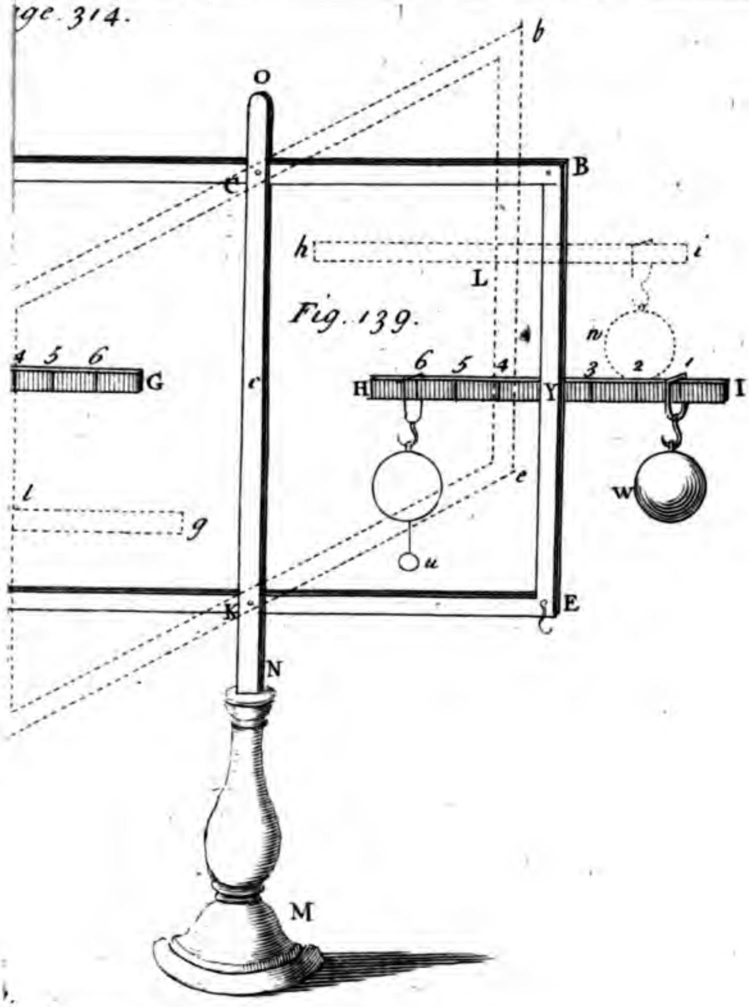
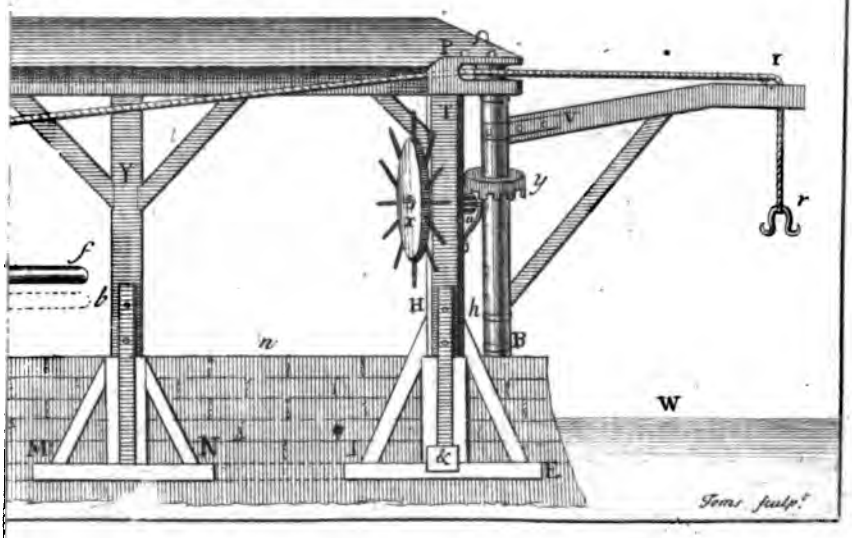


Fig. 139.



Toms sculp.

Ram-head r, and the Weight hanging at it, either to the Right or Left, and eaſily hold the Gibbet in any Poſition.

C T P Q, A ſtrong Piece or Block having three Pullies, one vertical, and the other two horizontal, that the Rope may run over the Firſt of them, and between the two others.

Fig. 141. Repreſents a horizontal Section of the Crane in its upper Part, or rather a View of it from the Plane of the Roof, ſuppoſing the Roof taken off. Fig. 141.

N. B. *The ſame Letters mark the Parts which have been deſcribed in* Figure 140.

Fig. 142. Shews the Inconveniencies in the Motion of the Gibbet. Fig. 142.

L B E D, Repreſents Part of the Wharf next the Water, or Precipice of a Quarry.

T P Q, The Block-Piece which holds the three Pullies, expreſſed by the ſame Letters in *Fig.* 140 and 141.

s g r G, The Arm of the Gibbet repreſented by V g, *Fig.* 140.

T, The vertical Pulley.

P, Q, The horizontal Pullies, repreſented in another Situation by p, q, when their Centers from m, y, are brought to n and t.

C, Is a Point directly over the Pivot of the Shaft, or Axel of the Gibbet.

C 1, C 2, C 3. C 4, C 5, Repreſents a Line over the Arm of the Gibbet, or rather a Plane going through the Middle of it, in ſeveral of its Situations, when turned towards the right Hand, from its direct Poſition C r.

C 6, C 7, C 8, C c, Repreſent the ſeveral Situations of the Gibbet towards the Left, the laſt Pulley r, at the End of the Gibbet, immediately over the Weight traverſing in the Circle, 5, 4, 3, 2, 1, 6, 7, 8.

When the Gibbet is in the Poſition c g, the Rope runs directly over the Middle of its Arm, therefore the leaſt Force applied to r or *r*, can keep in its Place the greateſt Weight that can be drawn up by the Crane, when ſuſpended to the Ram-head. If the Pullies are at p and q, the Gibbet loaded will alſo be without Labour retained in the Poſition C 2 on the Right, and C 6 on the Left, and with no great Trouble in the Poſition C 1.

But if the Gibbet be brought over the Wharf at 4 on the Right, or at 8 on the Left, the Rope will no longer run over the Middle of the Gibbet, but deviate from it, ſo as to make with it the Angle q 4 t, or o 8 n, and raiſe the Weight by the Motion of the Gibbet in Proportion as the Line q 4, or o 8, is longer than t 4, or n 8; and therefore the Weight will tend to run back towards g in Proportion to the Difference of thoſe Lines, which muſt give a Twitch to the Perſon who draws from r, or *r* by a guide Rope.

If to prevent this Inconveniency the Pulley at q be removed back to Q, then indeed the Rope will run over the Line C 4, or t 4,

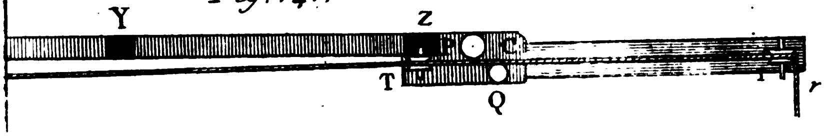
and

“ laſt Rope is that which raiſes the Weight. At the other End of the “ Axel there is a great wooden Wheel like a Pully H H, about “ which is wound a long Rope N.

“ To work this Engine, one muſt pull the long Rope N, which “ cauſing the great Wheel to turn, does alſo carry round the Axel “ or Barrel, which is made faſt to it. This Axel, as it turns round, “ cauſes the Ropes E E to wind about it, and thereby the Axel “ and the Wheel riſe, whilſt the Rope E, to which the Weight is “ faſtened, does alſo wind itſelf up upon the Axel the contrary “ Way; and this double winding up of the Ropes makes both the “ Burthen and the Axel and Wheel to riſe at the ſame Time. “ Now it is evident, that all this Riſe is performed without the “ Friction of any Part, and conſequently, the whole Power which “ draws the Rope N, is employed without any Hindrance; which “ cannot be in other Engines.

“ It may be objected that the Power which acts at N, muſt, “ beſides the Weight, raiſe alſo the Axel and great Wheel, and “ that their Weight is one of thoſe Obſtacles which *Ariſtotle* ſays all “ Engines are liable to; and that this Obſtacle is equivalent to the “ Friction which is in other Organs. But it may be anſwered, “ That Friction is an Obſtacle wholly unavoidable in all other Organs; but that it is eaſy to remedy the Obſtacles of this, which “ is done by Means of the heavy Body M, taken equal in Weight “ to the great Wheel and Axel, which it ſuſtains by Means of “ the Rope I I, which running over the Pullies L L, is fixed to the “ Ring or Collar K, that goes round the Axel F. For the Axel “ and the Wheel being counterpoiſed by this Weight, the Power “ which acts by drawing the long Rope N, acts for raiſing the “ Weight only. The Experiment which was made with this Engine has confirmed the Truth of this Problem, by comparing its “ Effects with thoſe of a Crane, in which the Proportion of the “ Bigneſs of the Axel to the Circumference of the Wheel, was “ the ſame as in my Machine: For it happened that in the Crane, “ a Weight of one hanging at a Rope going about the Wheel, “ drew up a Weight of Seven, when it had one half added to it to “ make it preponderate, or give Motion to the Power: And when “ the Weight to be raiſed, and the Weight which ſerved as a “ Power, were proportionably encreaſed, there was alſo a Neceſſity to encreaſe the additional Weight, which made the Power “ preponderate, in the ſame Proportion: So that it was required to “ add one half to the Power when the Weight was Seven; the “ Addition to the Power became one for a Fourteen Pound Weight, “ Two for a Twenty-eight Pound, Four for a Fifty-ſix Pound, “ and ſo on; becauſe the Reſiſtance from Friction encreaſes nearly “ in the ſame Proportion that the Weights are decreaſed. But this “ did not happen to my Engine, in which one Quarter was always

Fig. 141.



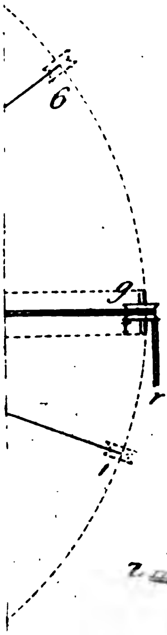


Fig. 143.

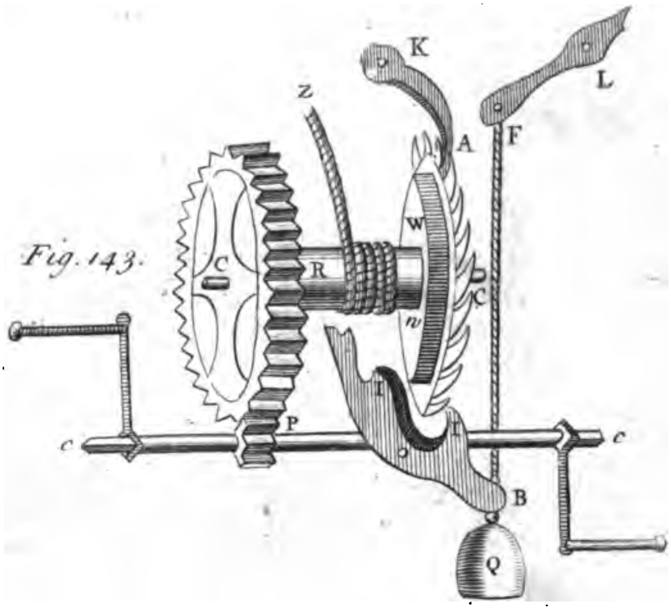
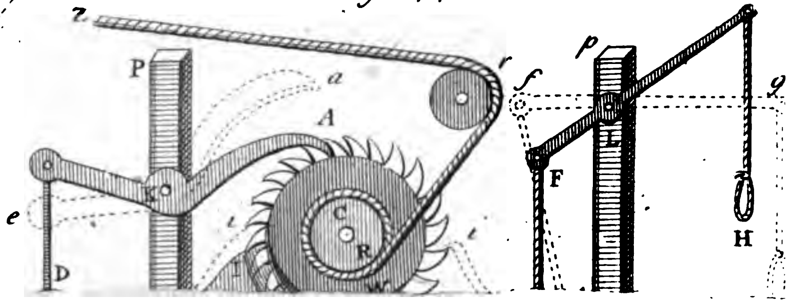


Fig. 144.



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“ ſufficient for the Draught (or to make the Power preponderate) “ not only when the Weight was Seven, but alſo when it was Four- “ teen Pound, Twenty-eight Pound, Fifty-ſix Pound, *&c.* which “ evidently ſhews, that this Engine acts without Friction.”

Thus far Monſieur *Perault*. But however plauſible this Deſcription may appear, a little Attention will ſhew, that if this new Engine had no Friction, yet it is more inconvenient than an *Axis in Peritrochio* with the ſame Proportions; and likewiſe that it has more Friction than the ſame Machine in the common Uſe. A C E, is a common *Axis in Peritrochio*, which has the Wheel A E five times bigger in Diameter than the Axel; ſo that A C, the Radius of the Wheel (which is the Diſtance of the Power) is to C B the Radius of the Axel (the Diſtance of the Weight) as 5 to 1: Conſequently One (for Example one Ounce, as in our Experiment) will keep five in Æquilibrio. Now, though the Friction of the Gudgeon at C is unavoidable, yet it may be diminiſhed by diminiſhing the Diameter of the Gudgeon, provided it remains ſtrong enough to ſuſtain the Machine and its Burthen. Here one Penny-weight, or $\frac{1}{20}$ of the Power added to it, makes it preponderate, and give the Machine Motion with a due Velocity. Fig. 148.

Now this very Engine made uſe of in Monſieur *Perault*'s Way, does ſo alter the Diſtances of the Weight and Power, that inſtead of one for our Power, we muſt have two and a half to keep the very ſame Weight Five, in Æquilibrio, as may appear by *Fig.* 149. Fig. 149. where, ſince in the Action of the Machine, when we pull the Rope P A, we make the Axel D B to wind itſelf up upon the Rope H D, it is evident that D is now become the Center of Motion, D B (the whole Thickneſs of the Axis) the Diſtance of the Weight = 2; and the Diſtance of the Power is reduced to A D = 4. So that if two Men, having been employed in the common Way to raiſe Weights equal to the Strength of ten Men, an Engineer ſhould alter the Manner of working, and fit up the *Axis in Peritrochio* in Monſieur *Perault*'s Way, inſtead of gaining an Advantage, he muſt call in three more Men to perform the Work. If it be anſwered, that what is loſt in Strength, will be gained in Time, it may not only be ſaid, that one cannot always call in more Help on the ſudden, but that even then, though we ſhould not call this an Inconveniency, yet there will be ſtill more Friction in this than in the common Method; for the Roller or Axel will find a Difficulty to wind on the Ropes, becauſe they are not perfectly pliable, and the leſs ſo, the greater the Weight is that ſtretches them. This, together with the Friction of the Collar of the Rope of the Counterpoiſe to the Engine, makes the Hindrance greater than in the common Way. For it appears by Experiment, that when the Power is become equal to 2 $\frac{1}{2}$ to keep the Weight 5 in Æquilibrio, there muſt be added $\frac{1}{5}$ (here 4 Penny-weight) to put the Power in Motion.

And to ſhew that this Friction of the Ropes is not always the ſame as Monſieur *Perault* ſuppoſes it; when P (or the Power) is made only one Ounce, and W (or the Weight) two Ounces, then to make the Power preponderate, only 2 Penny-weight and 18 Grains was ſufficient.

N. B. When P is $= 2\frac{1}{2}$; and W $= 5$, the additional Weight marked $\frac{1}{5}$, was 4 Penny-weight and 2 Grains.

A farther Examination of the Machine ſaid to be without Friction. By the ſame. N° 412. *p.* 228. Fig. 150,

2. In every Inclination of the Plane, if the Sine of the Angle of Inclination be taken in Parts of the Radius of the Axel, or Roller, the Power will be to the Weight : : as the Radius of the Roller + the Sine of Inclination, to the Radius of the Wheel — the ſaid Sine of Inclination; that is, P (= 1) : W (= 3) : : d k : a k.

In the preſent Experiment B E is an inclined Plane, on which the Roller C is to roll up, touching the ſaid Plane at the Point c; A M is the Wheel behind that Plane, another ſuch Plane, and equally inclined, being alſo ſuppoſed, behind the Wheel, to ſupport the other End of the Roller.

The Lines of Direction of the Power and Weight being a P and d W, through the Point of Contact, or Center of Motion, c draws A D parallel to the Horizon, and perpendicular to a P and d W; through the Center of the Engine, C draws a d parallel to A D. Suppoſe the Angle B c A of the Plane's Inclination to be 30°, the right Sine will then be equal to half the Radius; therefore dividing C 2 (the Radius of the Roller) into two equal Parts at k, if you draw k c and C c, the Angle k c C will be equal to B c A, and its Sine will be C k. Now ſince it is evidently the ſame thing to make uſe of a d for a Lever, whoſe Center of Motion is at k, as of A D equal and parallel to it with its Center of Motion at c; it follows that in this Inclination of the Plane, the Diſtance of the Weight d k is greater than d C (the Diſtance of the Weight in the common Uſe of this Engine) by the Addition of the Quantity C k, the Sine of the Angle of Inclination; and k a, the Diſtance of the Power is leſs than C a (the Diſtance of the Power in the common Way) by the Subtraction of the ſaid Quantity or Sine C k: Conſequently that on an inclined Plane, the Power is to the Weight : : as D c : to c A. Q. E. D.

Corollary I. Hence it follows, that the Radius of the Wheel, and the Radius of the Roller being given, the Loſs of Power may be found in any Inclination of the Plane. Thus, as here, the Power, which in the common Way would be but $\frac{1}{5}$ of the Weight, muſt be $\frac{1}{3}$ Part of it: So if the Angle of the Plane's Inclination was but $11^{\circ}\ 32'$ the Power would be $\frac{1}{4}$ of the Weight, *&c.*

Corollary II. Hence follows alſo, that if the Plane B E be Horizontal, no Force of the Power will be loſt, becauſe c g : c f : : C G : C F.

Scholium.

As the Friction of the winding of the Ropes, ſuch as B c in the new Way, is greater than the Friction of the Pivot in the old Way, (beſides the Friction of the Collars of the Counterpoiſe to the Engine) ſo that Friction diminiſhes, as the Ropes bear leſs Weight, according to the Diminution of the Angle of the Plane; and when the Plane is horizontal, and without a Counterpoiſe, even then the winding up of the Ropes, and Preſſure of the Roller againſt the Plane, is equal to the Friction in the common Way.

N. B. The Experiment is made here with Pivots twelve times leſs in Diameter than the Roller, and fine pliable Silk, inſtead of Ropes.

An Experiment to ſhew that the Friction of the ſeveral Parts in a Compound Engine, may be reduced to Calculation; by drawing Conſequences from ſome Experiments upon ſimple Machines, in various Circumſtances. By the ſame. Nº 423. p. 292.

X. The Machine conſiſts of three Pullies (two upper and one lower, or a Tackle of three) whoſe Diameters are exactly as follows, 2 Inches, 1 ½ Inch, 1 ¼ Inch; and all the Center Pins of ¼ Inch Diameter: The Rope being of $\frac{1}{10}$ Inch in Diameter.

The Weight is 18 Pounds *Averdupois*, and conſequently the Power to keep it in *Æquilibrio* muſt be = 6 ℔, and a very little more muſt make the Power raiſe the Weight, if there was no Friction; but here no leſs than 20 Ounces are required, though the Machine is as nicely made as it can poſſibly be.

I have ſhewn by Experiment, that when the Weight is unknown, ⅔ of the Power is the Friction of a Cylinder, whoſe Surface moves as faſt as the Power, and whoſe Gudgeons are equal in Diameter to the Cylinder. Now as the Diameter of the firſt Pulley is eight times bigger than its Pin, its Friction muſt be $\frac{4\text{ ℔}}{8}$ or 8 Ounces.

The ſecond Pulley, whoſe Surface moves as ſlow again as the Power, and whoſe Pin is ſix times leſs in Diameter, muſt of Conſequence have its Friction of only 5 ½ Ounces; becauſe $\cfrac{\cfrac{64\text{ ℥}}{2}}{6} = 5\tfrac{1}{3}\text{ ℥}.$

The third Pulley moving with ⅓ of the Velocity of the Power, on a Pin of ¼ of its Diameter, has for its Friction 4 ⅓ —— ℥; becauſe $\cfrac{\cfrac{64\text{ ℥}}{3}}{5} = 4\tfrac{1}{3}\text{—℥}.$

Now the Sum of all theſe Frictions being 17,6 ℥ which is the 5,4 Part of the Power 6 ℔, this Addition does ſo encreaſe the Friction

tion as to require a Super-addition of the 5,4 Part of that firſt Addition, and ſo on, in this Series, ℥ 17,62 + 3,2 + 0,59, &c. = 21,41 ℥.

Then the Sum of the Frictions upon account of bending the Ropes (too tedious to explain now, before I give a full Account in my intended Theory of Friction) deduced from the Experiment that a Rope of $\frac{1}{10}$ Inch in Diameter ſtretched by 6 ℔ requires 4,5 ℥ to bend it round a Cylinder of 1 Inch——, amounts to 1,8+1,15+1,124=4,424 ℥, which, with the other Friction, amounts to 25,834 ℥. But as I have formerly ſhewn in theſe *Tranſactions*. that when a Rope drawn by unequal Weight runs over a Pulley, the Preſſure on the Pin is diminiſhed; that diminiſhed Preſſure (found by Calculation to be near 6 ℥) being taken from the above Sum, the Friction remaining will be 19,834 ℥; and the Experiment is juſt 20 ℥.

N. B. Nothing was here allowed for the Weight added to bend the Ropes, which would ſtill bring the Experiment nearer the Theory.

Two Experiments of the Friction of Pullies. By the ſame. Nº 425. p. 394.

XI. The firſt Experiment was made with a Tackle of five Braſs Sheevers in Iron Frames or Blocks; that is, three Sheevers in the upper Block, and two in the lower.

Having made an Æquilibrium, by hanging one Hundred and a quarter at the lower Block, and a quarter of an Hundred at the running Rope; I added 17 Pounds and a half before the Power could go down and raiſe the Weight.

Experiment 2. Two Hundred and a half being balanced by half a Hundred, the Addition of 28 Pounds made the Power raiſe the Weight.

N. B. The Sheevers were five Inches Diameter, the Pins half an Inch, and the Rope three quarters.

In the firſt Experiment 17 Pounds and a half exceeds by 4 Pounds and a half the Sum of the Frictions deduced from the Theory. But in the ſecond Experiment 28 Pounds exceeds the Sum of the Frictions but one Pound.

The Reaſon of this appeared to be, that the Rope at firſt was too big for the Cheeks that held the Sheevers; but in the ſecond Experiment, where the Rope was more ſtretched, it was ſomewhat diminiſhed in Diameter, and ſo brought off from rubbing ſo hard againſt thoſe Cheeks.

From knowing the Quantity of Friction *à priori* in ſuch large Tackles, we may know what to expect in Practice: For if one Man, who for a ſmall time can exert the Force of one Hundred Pounds, thinks that he may draw up a Stone, or a Roll of Sheet-Lead, or any other ſuch Weight to the Top of a Houſe with a Tackle of Five (becauſe this would ſeem feaſible from mechanical Principles)

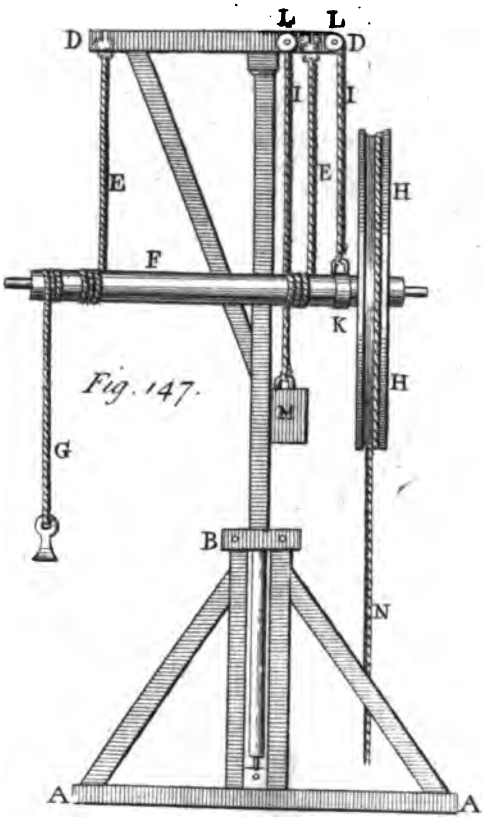


Fig. 147.

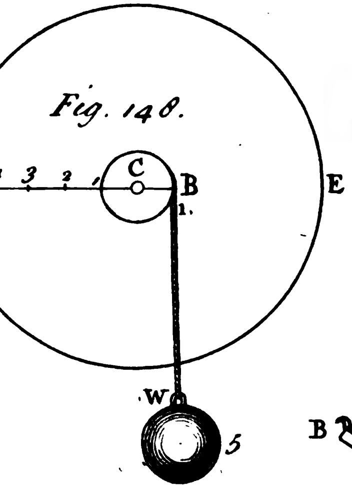


Fig. 148.

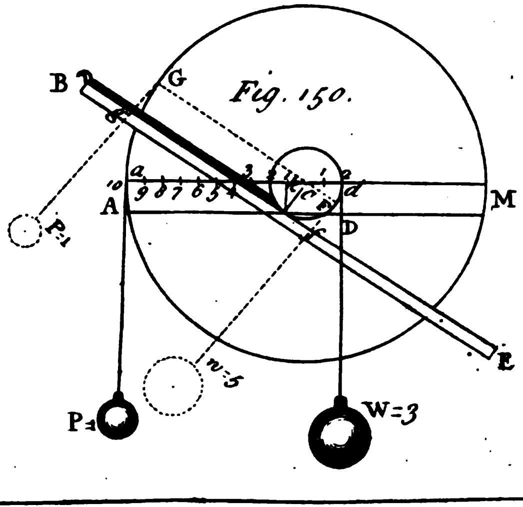


Fig. 150.

ples) will find himſelf miſtaken on account of the Friction, which will not be ſurmounted without an additional Force of fifty Pounds.

XII. Thoſe who endeavour after a perpetual Motion take it for granted, that if a Weight deſcending in a Wheel, at a determinate Diſtance from the Centre, does in its Aſcent approach nearer to it; ſuch a Weight in its Deſcent will always preponderate, and cauſe a Weight equal to it to riſe, provided it comes nearer the Centre in its Riſe; and accordingly as itſelf riſes, will be overbalanced by another Weight equal to it; and therefore they endeavour by various Contrivances to produce that Effect, as if the Conſequence of it would be a perpetual Motion. *Remarks on ſome Attempts made towards a perpetual Motion, by the ſame.* N° 367. *p.* 234.

But I ſhall ſhew, that they miſtake one particular Caſe of a general *Theorem*, or rather a *Corollary* of it, for the *Theorem* itſelf. The *Theorem* is as follows:

If one Weight in its Deſcent, does by Means of any Contrivance, cauſe another Weight to aſcend with a leſs *Momentum* or Quantity of Motion than itſelf, it will preponderate and raiſe the other Weight. *Theorem.*

Therefore if the Weights be equal, the deſcending Weight muſt have more Velocity than the aſcending Weight, becauſe the *Momentum* is made up of the Weight multiplied into the Quantity of Matter. *Corollary* I.

Therefore if a Lever or Balance, have equal Weights faſtened or hanging at its Ends, and the *Brachia* be ever ſo little unequal, that Weight will preponderate, which is fartheſt from the Centre. *Corollary* II.

This ſecond *Corollary* cauſes the Miſtake; becauſe thoſe, who think the Velocity of the Weight is the Line it deſcribes, expect that that Weight ſhall be overpoiſed, which deſcribes the ſhorteſt Line, and therefore contrive *Machines*, to cauſe the aſcending Weight to deſcribe a ſhorter Line than the deſcending Weight. As for Example, in the Circle A D B a, the Weights A and B being ſuppoſed equal, they imagine, that if (by any Contrivance whatever) whilſt the Weight A deſcribes the *Arc* A a, the Weight B is carried in any *Arc*, as B b, ſo as to come nearer the Centre in its riſing, than if it went up the *Arc* B D; the ſaid Weight ſhall be overpoiſed, and conſequently, by a Number of ſuch Weights, a perpetual Motion will be produced. *Scholium.* Fig. 151.

This is attempted by ſeveral Contrivances, which all depend upon this falſe Principle; but I ſhall only mention one, where a Wheel having two parallel Circumferences, has the Space between them divided into Cells, which being carved, will (when the Wheel goes round) cauſe Weights placed looſe in the ſaid Cells, to deſcend on the Side A A A, at the outer Circumference of the Wheel; and on the Side D to aſcend in the Line B b b b, which comes nearer the Centre, and touches the inner Circumference of the Wheel. In a *Machine* of this Kind, the Weights will indeed move in ſuch a Manner, Fig. 152.

Manner, if the Wheel be turned round, but will never be the Cauſe of the Wheel's going round.

The Velocity of any Weight is not the Line, which it deſcribes in General, but the Height that it riſes up to, or falls from, with reſpect to its Diſtance from the Centre of the Earth. So that when Fig. 151. the Weight deſcribes the *Arc* A a, its Velocity is the Line A C, which ſhews the perpendicular Deſcent (or meaſures how much it is come nearer to the Centre of the Earth) and likewiſe the Line B C denotes the Velocity of the Weight B, or the Height that it riſes to when it aſcends in any of the *Arcs* B b, inſtead of the *Arc* B D: So that in this Caſe, whether the Weight B in its Aſcent be brought nearer the Centre or not, it loſes no Velocity, which it ought to do, in order to be raiſed up by the Weight A. Nay, the Weight in riſing nearer the Centre of a Wheel, may not only not loſe of its Velocity, but be made to gain Velocity, in Proportion to the Velocity of its counterpoiſing Weights, that deſcend in the Circumference of the oppoſite Side of the Wheel; for if we conſider two *Radij* of the Wheel, one of which is Horizontal, and the other (faſtened to Fig. 153. and moving with it) inclined under the Horizon in an Angle of 60 degr. and by the Deſcent of the End B of the *Radius* B C, the *Radius* C D by its Motion cauſes the Weight at D, to raiſe up the Line p P, which is in a Plane that ſtops the ſaid Weight from riſing in the Curve D A, that Weight will gain Velocity, and in the Beginning of its Riſe, it will have twice the Velocity of the Weight at B; and conſequently, inſtead of being raiſed, will overpoiſe, if it be equal to the laſt mentioned Weight. And this Velocity will be ſo much the greater, in Proportion as the Angle A C D is greater, or as the Plane P p (along which the Weight D muſt riſe) is nearer to Fig. 151. the Centre. Indeed if the Weight at B, could by any Means be lifted up to β, and move in *Arc* β b, the End would be anſwered; becauſe then the Velocity would be diminiſhed, and become β C.

Experiment. Fig. 153. Take the Lever B C D, whoſe *Brachia* are equal in Length, bent in an Angle of 120 degr. at C, and moveable about that Point as its Centre: In this Caſe, a Weight of two Pounds hanging at the End B of the horizontal Part of the Lever, will keep in *Æquilibrio* a Weight of Four Pounds hanging at the End D. But if a Weight of one Pound be laid upon the End D of the Lever, ſo that in the Motion of D along the *Arc* p A, this Weight is made to riſe up againſt the Plane P p (which divides in half the Line A C equal to C B) the ſaid Weight will keep in *Æquilibrio* two Pounds at B, as having twice the Velocity of it, when the Lever begins to move. This will be evident, if you let the Weight 4 hang at D, whilſt the Weight 1 lies above it: For then if you move the Lever, the Weight 1 will riſe four times as faſt as the Weight 4.

Experiments concerning the Coheſion of Lead. By the ſame. No. 389. *p.* 345. Fig. 154.

XIII. I took the Leaden Balls A and B, the firſt weighing one Pound, and the other two Pounds; and having from each of them cut off a Segment of about $\frac{1}{4}$ Inch in Diameter, I preſs'd them together with my Hand, with a little Twiſt, to bring the flat Parts to touch as well as I could. The Balls ſtuck ſo faſt, that when the Hand H, by means of a String, ſuſtained the upper Ball A, the lower one B (by reaſon of its Contact at C) was ſuſtained, though loaded with the Scale S, and Weights E, which amounted to 16 Pounds. A little more Weight added ſeparated them, and, upon viewing the touching Surfaces, it appeared that they did not exceed a Circle of $\frac{1}{10}$ Inch Diameter; but this Surface can hardly be meaſured exactly, on account of its Irregularity. The Experiment was repeated ſeveral Times, and the Coheſion of the Balls was different every Time.

Fig. 155.

On the upper Pin or Bar of the wooden Frame D*d*I H, I ſuſpended the Steelyard E F, whoſe Hook held up a leaden Ball A of two Inches in Diameter, having a Hole through it, at A, to receive a String; the lower Ball B equal to, and prepared in the ſame Manner as the firſt, received the Pin O o, through its String, ſo that G, the Weight of the Steelyard, was made uſe of to ſeparate the Balls, which happened when it was applied at the Number 20, in the firſt Experiment; but, in the three following Experiments, the Balls were not ſeparated till the Weight was removed to the Numbers 25, 37, and 45, expreſſing Pounds on the Steelyard.

Laſtly, The Balls being applied together as before, ſtill cleaning the Surface of Contact with my Knife, and never making a Contact ſenſibly greater than what I mentioned before; the Weight G removed quite to the End F, where it weighed 47 Pounds, was not able to ſeparate the Balls.

CHAP.

CHAP. VI.

HYDROSTATICS, HYDRAULICS.

Deſcription and Uſe of a new Areometer. By D. G. Fahrenheit, *F. R. S.* Nº 384. *p.* 140. Fig. 156.

I. 1. GLOBULO, A. ſatis magno, (quo major Globulus eo melior) tubi ſibi oppoſiti C D & E F annectuntur, tubulo gracillimo E F receptaculum G additur, mediumque tubuli puncto *a* minutiſſimo, ſatis tamen viſibili, denotatur. Extremitas altera tubuli C D globulo B prædita eſt, qui receptaculi loco ponderi inferiori (quo nempe inſtrumentum aggravatur) inſervit. Diſtantia globuli B a centro globi A triplo major ſit, quam diſtantia receptaculi G ab eodem centro. Inſtrumento ita præparato, globulus B tantâ mercurii quantitate repletur, ut ſi Aræometron liquori leviſſimo, exempli gratia, ſpiritui vini bene dephlegmato, vel ſpiritui Terebinthinæ immergatur, illud in liquore fere uſque ad punctum *a* deſcendat; quo facto, tubulus prope E hermetice ſigillatur, & inſtrumentum bilance accuratiori ponderatur; eritque pondus inſtrumenti etiam ipſiſſimum liquoris ab inſtrumento deturbati pondus, utpote ſatis hydroſtatices peritis notum eſt. Si autem graviores inveſtigandi ſunt liquores, exempli gratia, aqua, lixivia, vel ſpiritus acidi, eorum gravitatis differentia invenitur, dum nempe inſtrumentum in receptaculo G tanto pondere oneratur, ut illud iterum ad punctum *a* ſubſidat. Hoc pondere gravitati inſtrumenti addito, illorum liquorum gravitates ſpecificæ (ſi pondera ſint minutiſſima) ſatis exacte habebuntur: & ſic de cæteris.

Dixi quod inſtrumentum in memoratis ſpiritibus fere ad punctum *a* ſubſidere debeat; melius enim erit, ut non perfecte liquor illud punctum attingat, & ut differentia parva ponderibus minutiſſimis adjuvetur: hoc enim modo, ſi forſan adhuc liquores leviores darentur, vel etiam ſi liquorum memoratorum gravitas a calore ſpecifice levior redderetur, adhuc inſtrumento explorari poterunt, quod alias non ſuccederet, ſi illud perfecte ad punctum *a* in nominatis ſpiritibus ſubſideret.

Dum autem experimenta fiunt, cavendum eſt, ne ſuperficies, tam inſtrumenti, quam liquorum aliquâ pinguedine, vel aliis particulis heterogeneis ſint imbutæ; aliter enim experimenta nunquam ſatis accurate peragentur.

A new Kind of Hydrometer made by Mr. Clarke, *and communicated to the Society, by* J. T. Deſaguliers, *L. L. D. F. R. S.* Nº 413. *p.* 277. Fig. 157.

2. The Hydrometer, by ſome called Areometer, is an Inſtrument commonly made of Glaſs, conſiſting of a Stem A B, graduated by ſmall Beads of Glaſs of different Colours, ſtuck on the Outſide, a larger

Fig. 153.

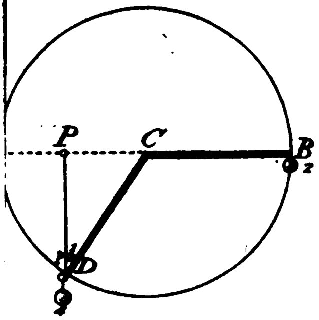


Fig. 154.

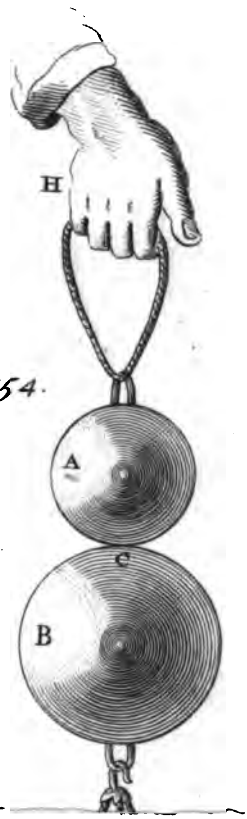
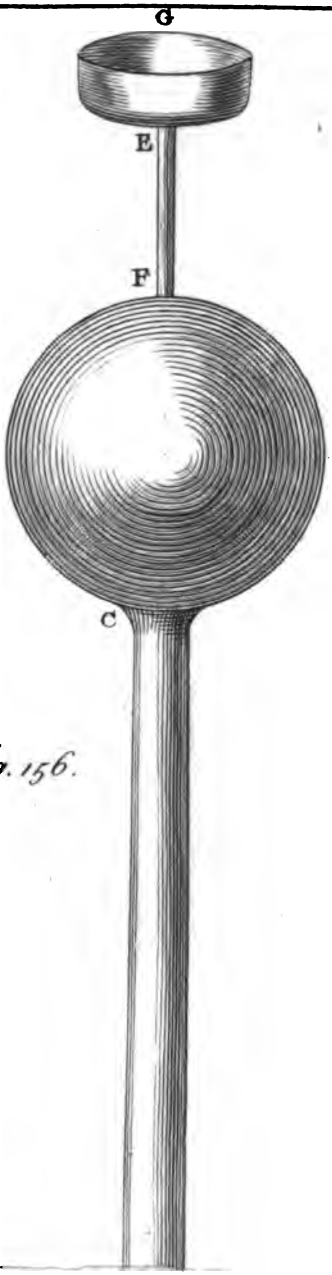


Fig. 156.



larger Ball, B, quite empty as well as the Stem, and a ſmall Ball, C, filled with Quickſilver before the End A was hermetically ſealed, in ſuch Manner as to make the Hydrometer ſink in Rain-Water as deep as *m*, the Middle of the Stem. Such an Inſtrument does indeed ſhew the different ſpecifick Gravity of all Waters or Wines, by ſinking deeper in the lighter, and emerging more out of the heavier Liquors; but as it is difficult to have the Stem exactly of the ſame Bigneſs all the Way, and if it could be had, the ſame Inſtrument would not ſerve for Water and Spirits, ſinking quite over Head in Spirits when made for Water, and emerging in Water with Part of the great Ball out, when made for Spirits. The Hydrometer has only been uſed to find whether any one Liquor is ſpecifically heavier than another; but not to tell how much, which cannot be done without a great deal of Trouble, even with a nice Inſtrument. The Hydroſtatical Balance has ſupplied the Place of the Hydrometer, and ſhews the different ſpecifick Gravity of Fluids to a very great Exactneſs. But as that Balance cannot well be carried in the Pocket, and much leſs managed and underſtood by Perſons not uſed to Experiments, Mr. *Clarke* was reſolved to perfect the Hydrometer for the Uſe of thoſe that deal in Brandies and Spirits, that by the Uſe of the Inſtrument they may, by Inſpection, and without Trouble, know whether a ſpirituous Liquor be Proof, above Proof, or under Proof, and exactly how much above or under: And this muſt be of great Uſe to the Officers of the Cuſtoms, who examine imported or exported Liquors.

After having made ſeveral fruitleſs Trials with Ivory, becauſe it imbibes ſpirituous Liquors, and thereby alters its Gravity, he at laſt made a Copper Hydrometer, having a Braſs Wire of about $\frac{1}{4}$ Inch thick going through, and ſoldered into the hollow Copper Ball, B *b*. The upper Ball of this Wire is filed flat on one Side, for the Stem of the Hydrometer, with a Mark at *m*, to which it ſinks exactly in Proof Spirits. There are two other Marks, A and B, at Top and Bottom of the Stem, to ſhew whether the Liquor be $\frac{1}{10}$ above Proof (as when it ſinks to A) or $\frac{1}{10}$ under Proof (as when it emerges to B) when a Braſs Weight, ſuch as C, has been ſcrewed on, to the Bottom at *c*. There are a great many ſuch Weights of different Sizes, and marked to be ſcrewed on, inſtead of C, for Liquors that differ more than $\frac{1}{10}$ from Proof, ſo as to ſerve for the ſpecifick Gravities in all ſuch Proportions as relate to the Mixture of ſpirituous Liquors, in all the Variety made Uſe of in Trade. There are alſo other Balls for ſhewing the ſpecifick Gravities quite to common Water, which makes the Inſtrument perfect in its Kind.

Fig. 158.

A Caution to be uſed in examining the Specifick Gravity of Solids, by weighing them in Water. By James Jurin, *M. D. R. S. Secr.* N° 369. *p.* 223.

II. 1. As it is oftentimes of good Uſe to know the Specifick Gravity of ſolid Bodies, a great Number of Experiments have been made upon this Subject by Members of the *Royal Society*, and other Curious Perſons; the Reſult of which has been publiſhed in ſeveral Tables in the *Philoſophical Tranſactions*, and elſewhere. But, as it is neceſſary that Experiments of this Nature ſhould be made with great Exactneſs, if we would ſo far depend upon them, as to draw any Inferences from them in Natural Philoſophy, it may not be amiſs to mention a Caution, which is oftentimes neceſſary in the making of them, and which I have Reaſon to think has been generally very little regarded. It is this; That when a dry, porous Body is to be weighed in Water, in order to diſcover its Specifick Gravity, it is neceſſary, by ſome means or other, to extricate the Air out of all the ſmall Pores and Cavities within it, that the Water may have free Liberty to enter and pervade them. Unleſs this Care be taken, it muſt needs happen, that the Air, which poſſeſſes thoſe ſmall Cavities, and keeps the Water out, will render the Solid of leſs Weight in the Water, and conſequently of leſs apparent Specifick Gravity than it really is. The beſt way of avoiding this Inconvenience, is, to ſet the Veſſel of Water, in which the ſolid Body is immerged, under the Receiver of an Air-Pump, and to extract the Air out of the Body by that Means; which will be more eaſily and exactly done, if the Water be firſt heated over the Fire. And where the Conveniency of an Air-Pump cannot be had, the ſame Thing may be done almoſt as well, by letting the ſolid Body continue ſome Time in boiling Water over the Fire.

But no ſolid Body muſt ever be put into hot Water, that will in any meaſure diſſolve, or give a Tincture to the Water.

One Inſtance of the Neglect of this Caution, may be ſeen in the Accounts we have of the ſpecifick Gravity of the Stones taken out of human Bladders, which have been commonly found to be but about one half, and ſome of them have been no more than a fourth Part heavier than an equal Bulk of Water. From this it has been too haſtily concluded, that theſe Stones are very improperly called by that Name, as not at all approaching to the ſpecifick Gravity of even the lighteſt real Stones, that we have any Account of.

Whereas it is much more reaſonable to ſuppoſe, that thoſe Stones, which have been found to be ſo light, were ſuch as had been a conſiderable Time taken out of the Bladder, and conſequently had loſt much of their Weight by the Evaporation of the Urine, with which they had at firſt been ſaturated, and that they had afterwards been tried without the Caution above-mentioned. I would therefore beg Leave to recommend it to thoſe, who ſhall examine the Specifick Gravity of the human *Calculus*, that they will either try the Experiment upon Stones freſh taken out of the Bladder, or elſe that they will be pleaſed to uſe the above-ſaid Method, to extricate the Air out of their Cavities. If they do this, I am confident they will meet with ſome *Calculi* (as I have done) exceeding the Weight of ſome ſorts of burnt earthen Ware and Alabaſter, and approaching very near to that of Brick, and the ſofter ſort of paving Stone. But it is not to be expected, that they ſhould entirely equal the Specifick Gravity of Stone, found in the Earth; becauſe the Mixture of ſome Portion of the Animal Oil and Volatile Salt, with the ſtony Subſtance of the human *Calculi*, muſt needs leſſen the Specifick Gravity of the whole Concrete.

I ſhall mention one other Obſervation, relating to this Subject; which, however trivial it may ſeem, yet to me was very ſurprizing, when I accidentally diſcovered it. It is, That the Subſtance of all Wood (as Oak, Fir, *&c.*) is ſpecifically heavier than Water. To prevent being miſunderſtood, I muſt obſerve, that in Wood, and other Vegetables, there are two Sorts of Veſſels; one of which convey the Sap, and the other contain only Air, for which Reaſon they are called Air-Veſſels. When Wood floats, or ſwims in Water, this Effect is not owing to the Lightneſs of the Subſtance of the Wood, but only to its being buoyed up by the Air contained in the Veſſels before-ſaid. For when the Air is extracted out of thoſe Veſſels, and inſtead thereof the Water has inſinuated itſelf into them, the Wood will ſink to the Bottom. As is very eaſily ſhewn in ſmall Chips, or Shavings of Wood, by means of the Air-Pump, or an Infuſion in boiling, or even in cold Water for a ſufficient Time. And the ſame is found to ſucceed in the Roots, Stalks, Leaves, and Seeds of as many other Vegetables as I have yet tryed; Cork only excepted; in which laſt I had no Reaſon to expect it, conſidering the particular Structure of that Subſtance, as deſcribed by Dr. *Hook*, in his *Micrographia.*

The Specific Gravities of ſome Bodies, by D. G. Fahrenheit, *F. R. S.* N°. 383. *p.* 114.

2. Aurum	19081
Mercurius	13575 *
Plumbum	11350
Argentum	10481
Cuprum Suecicum	8834
Idem Japonenſe	8799
Ferrum	7817
Stannum provinciæ Indiæ Orientalis vulgo dictæ Malacca	7364
Stannum Anglicanum	7313
Marcaſita alba	9850
Regulus Antimonii	6622
Aurichalcum	8412
Cryſtallus de rupe	2669
Pyrites homogeneus	2584
Cinis clavellatus ſordibus, ſaleque neutro quodam (quod fere ſemper magis vel minus in cinere illo reperitur) depurgatus	3112
Sal illud neutrum	2642
Sal maritimum	2125
Nitrum	2150

Alumen	1738
Saccharum albiſſimum	1606½
Oleum Vitrioli	1877½ *
Lixivium cineris clavellati ſale quantum fieri potuit impraegnatum	1563 *
Idem alio tempore præparatum	1571⅓ *
Aqua fortis melioris notæ	1409 *
Spiritus nitri	1293½ *
Aqua pluviatilis	1000 *
Oleum Raparum	913
Alcohol vini	826
Idem magis dephlegmatum	825

Experimenta variis ſunt facta modis. Corpora enim fixa, ut vulgo fieri ſolet, prius bilancis accuratioris ope in aere & deinde in aqua pluviatili ſunt ponderata. Salium pondus, prius in aere, & tunc in liquore idoneo quodam eſt exploratum, & deinde calculo ad gravitatem aquæ comparatum. Liquorum gravitates, interdum Aræometro ſupra deſcripto, aliquando autem vaſibus hic delineatis ſunt indagatæ.

Fig. 159. Globus Vitreus concavus A ad Lampadis flammam ſatis magnus conficitur, duobus tubulis vitreis ſibi oppoſitis B & B præditus. Tubulorum extremitates ſunt apertæ, attenuatæ, & aliquantiſper incurvatæ, ne liquor effluere poſſit. Globulus præterea in inferiori loco aliquantulum eſt applanatus, ut eo commodius bilanci imponi poſſit.

Fig. 160. Ampulla A, e tenuiſſimo vitro ad lampadis flammam paratur, collo ſatis largo prædita, cujus apertura operculo B, intus concavo tam accurate, quam fieri poteſt, clauditur.

Ope hujus ampullæ, etiam ſalium gravitates ſpecificæ explorari poſſunt, & quidem hoc modo. Ampulla prius liquore quodam idoneo (in quo nempe ſal, cujus gravitas exploranda eſt, non ſolvitur) impletur, & poſtquam liquoris innotuit pondus, liquor effunditur, atque vas probe exſiccatur. Hoc facto, ſale fere totum vas impletur, & ſalis pondus inquiritur; hoc noto, interſtitia ſalis liquore replentur, ponderiſque incrementum a liquore addito quæritur. Si hocce incrementum ponderis a pondere toto liquoris ſubtrahitur, reſiduum exprimet gravitatem liquoris a ſale deturbati.

Sal neutrum cineris clavellati in ſpiritu nitri nullam facit ebullitionem. Mercurium in ſpiritu nitri ſolutum albo colore præcipitat. Carbonibus ſuperimpoſitum, crepitu in minores partes diſrumpitur & diſpergitur.

Nitrum in tigillo ſupra ignem fuit liquefactum, ut eo modo ab omni humiditate depurgaretur, ſpatiaque nonnulla aere alias plena nitro ipſo replerentur.

Gravitates liquorum, quæ aſteriſco notatæ ſunt, ad gradum quadrageſimum octavum meorum thermometrorum calculo ſunt revocatæ, & nonnullorum jam in * experimentis de gradu ebullitionis liquorum quorundam facta eſt mentio.

Modus ſimpliciſſimus ad inveſtigandam gravitatis differentiam, quæ a diverſo temperamento fluidorum originem ſuam trahit, eſt, ut prius liquore minus calido (cujus tamen gradus ope Thermometri notus eſſe debet) vas aliquod repleatur, & ponderetur, deinde illud vas iterum calidiori liquore impleatur, & ut prius ponderetur. Si in hoc ſecundo experimento gradus caloris iterum eſt notatus, habebitur differentia gravitatis liquoris, a calore inter hos gradus effecta, quæ deinde ope calculi facile unicuique gradui attribui poteſt.

Experimenta in aere ſunt facta; addenda ergo erit unicuique numero gravitas aeris, ut habeatur materiarum gravitas in vacuo. Eſt autem aeris gravitas ſpecifica ad illam aquæ, fere ut 1 ad 1000.

Of the Figures of Fluids, turning round an Axis, By Mr. Peter Lewis de Maupertuis, *F. R. S.* Nº. 422. *p.* 240. Fig. 161.

III. *Prob.* I. Invenire Figuram Sphæroidis fluidi circa axem rotantis, poſito quod fluidi partes verſus centrum attrahantur ſecundum aliquam diſtantiæ a centro dignitatem.

Solut. Sit PQ axis revolutionis, & PAQB ſectio Sphæroidis per axem; jam cum partes fluidi inter ſe quieſcant, columnarum unaquæque CD idem habebit pondus verſus C; conſiderando ergo e columnis unam CD quæ efficit cum CP datum angulum cujus ſinus $= b$ pro radio $= 1$, & quæ ex infinitis cylindrulis G*g* componitur; cylindruli cujuſque pondus verſus C quæro.

Gravitas abſoluta in A cum ſit data & $= p$, pro habenda gravitate in G, erit $p : \overset{\prime}{p} :: CA^n . CG^n$; unde habebitur gravitas in G ſeu

$$\overset{\prime}{p} = \frac{p . CG_n}{CA^n}.$$

Sed cum propter revolutionis motum pars quævis fluidi repellitur vi centrifuga ſecundum GH; & cum in mobilibus quæ contemporaneas circulationes abſolvunt, vires centrifugæ ſint ut circulorum deſcriptorum radij; ſi vis centrifuga in A ſit data & $= f$, pro habenda vi centrifuga in G, erit $f . \overset{\prime}{f} :: CA . LG =$ (ob $LG . CG :: b . 1$) $b\,CG$; unde habebitur vis centrifuga in G ſeu $\overset{\prime}{f} = \frac{f\,b\,.\,CG}{CA}$: Sed vis hæc cum ſecundum GH agat decomponenda eſt in duas vires KH & GK, ex quibus una tantum GK partem aliquam vis ſecundum GC tollit. Habebitur ergo vis illa GK dicendo GH . GK vel 1 . b : : $\frac{f\,b\,.\,CG}{CA}\,.\,f = \frac{f\,b\,b\,.\,CG}{CA}$ = vi cylindrulum G g verſus D trahenti. Vis ergo cylindrulum G g verſus C trahens erit tantum $\frac{p\,.\,CG^n}{CA^n} - \frac{f\,b\,b\,.\,CG}{CA}$; & pondus cylindruli verſus C, erit $\left(\frac{p\,.\,CG^n}{CA^n} - \frac{f\,b\,b\,.\,CG}{CA}\right)$ G g. Jam columnæ C G ex cylindrulis iſtis conflatæ pondus erit $\left(\frac{p\,.\,CG^n}{CA^n} - \frac{f\,b\,b\,.\,CG}{CA}\right)$G g; quod cum G g ſit Elementum ipſius C G, dabit pro pondere columnæ C G, $\frac{p\,.\,CG^{n+1}}{n+1\,.\,CA^n} - \frac{f\,b\,b\,CG^2}{2\,CA}$; & pro pondere totius columnæ C D, $\frac{p\,.\,CD^{n+1}}{n+1\,.\,CA^n} - \frac{f\,b\,b\,.\,CD^2}{CA}$, quod efficere debet pondus conſtans A.

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* *Phil. Tranſ.* Nº 381.

Si ergo vocentur C A $= a$, C D $= r$, habebitur $\frac{p\,r^{n+1}}{n+1\,.\,a^n} - \frac{f\,b\,b\,r\,r}{2\,a} =$ A. Et cum æquatio hæc, quæcunque ſit b, ſemper obtineat, jam ſi b pro indeterminata ſumatur, æquatio præcedens relationem dabit inter radium quemvis C D & ſinum anguli quem cum axe P Q facit.

Nunc determinanda eſt quantitas conſtans A. Ut æquatio præcedens, ſit ad ſectionem ſphæroidis illius cujus ſemi-axis C A $= a$, oportet, quando angulus D C P eſt rectus, vel quando $b = 1$, ſit $r = a$; tunc ergo habetur $\frac{p\,a^{n+1}}{n+1\,.\,a^n} - \frac{f\,a\,a}{2\,a} =$ A, vel A $= \left(\frac{2\,p - n\,f - f}{2\,.\,n+1}\right)a$.

Et ſic æquatio correcta, eſt $\frac{p\,r^{n+1}}{n+1\,.\,a^n} - \frac{f\,b\,b\,r\,r}{2\,a} = \left(\frac{2p - nf - f}{2\,.\,n+1}\right) a$ vel $2\,p\,r^{n+1} - (n+1)\,f\,b\,b\,a^{n-1}\,r\,r = (2\,p - n\,f - f)\,a^{n+1}$.

Æquatio hæc, omnium ſphæroidum ſectiones determinat quæcunque ſit dignitas diſtantiæ, ſecundum quam ſit attractio; unâ tantùm excepta hypotheſi in qua attractio foret in ratione ſimplicis diſtantiæ a centro inverſa.

In hoc caſu recurrendum erit ad $\left(\frac{n\,.\,\mathrm{CG}^n}{\mathrm{CA}^n} - \frac{f\,b\,b\,.\,\mathrm{CG}}{\mathrm{CA}}\right)$ Gg quod tunc ſit $\left(\frac{p\,.\,\mathrm{CA}}{\mathrm{CG}} - \frac{f\,b\,b\,.\,\mathrm{CG}}{\mathrm{CA}}\right)$ Gg, cujus fluens non niſi per Logarithmos habetur, & prodit $p\,.\,\mathrm{CA}\ \log.\ \mathrm{CG} - \frac{f\,b\,b\,.\,\mathrm{CG}^2}{2\,\mathrm{CA}} = \mathrm{A}$; vel pro pondere totius columnæ $p\,a\ \log.\ r - \frac{f\,b\,b\,r\,r}{2\,a} = \mathrm{A}$.

Ut corrigatur hæc æquatio, oportet ut quando $b = 1$, ſit $r = a$; tunc ergo habetur $p\,a\ \log.\ a - \frac{f\,a}{2} = \mathrm{A}$; & æquatio correcta, eſt $p\,a\ \log.\ r - \frac{f\,b\,b\,r\,r}{2\,a} = p\,a\ \log.\ a - \frac{f\,a}{2}$; vel $2\,p\,a\ \log.\left(\frac{r}{a}\right) = \frac{f\,b\,b\,r\,r}{a} - f\,a$; vel tranſeundo ad numeros & ſumendo $c =$ numero cujus log. $= 1$, habetur $r = a c^{\left(\frac{f\,b\,b\,r\,r}{2\,p\,a\,a} - \frac{f}{2\,p}\right)}$.

Patet meridianos ſphæroidum ſemper prodire curvas algebraicas excepta tantum iſta hac hypotheſi.

Si harum omnium curvarum deſideretur aequatio more ſolito per coordinatas rectangulas, facile haberetur. Nam faciendo CE $= x$, & DE $= y$, erit $r\,r = x\,x + y\,y$, & $b\,r = y$. Exterminando ergo b & r ex æquatione generali, invenietur $2p\,(xx+yy)^{\frac{n+1}{2}} - (n+1)\,f a^{n-1}\,yy = (2p-nf-f)\,a^{n+1}$.

Et in caſu $n = -1$, $xx+yy = aac^{\left(\frac{fyy}{paa}-\frac{f}{p}\right)}$.

Sed prima noſtra ratio definiendi curvas per radios & angulos æque, & forſan hic magis commoda eſt quam illa quæ definit curvas per coordinatas.

Quamvis h, ut variabilis tractatur, tamen non ultra certos limites variat, & hi limites ſunt o & 1; noſtra itaque æquatio radialis non definit niſi partem curvæ cujus amplitudo eſt angulus rectus; ſed cum curvæ iſtæ ex quatuor arcubus ſimilibus & æqualibus conſtent, dantur curvæ meridianorum integræ per æquationem noſtram.

Jam facile determinatur ratio inter ambos Sectionis axes in quavis Hypotheſi.

Cum æquatio generalis ſit $2\,pr^{n+1} - (n+1)\,fhh a^{n-1}\,rr = (2p-nf-f)\,a^{n+1}$; ut inveniatur r quando $h = o$, habetur $2\,pr^{n+1} = (2p-nf-f)\,a^{n+1}$. Ex quo elicitur C A. C P :: $(2p)^{\frac{1}{n+1}}.\ (2p-nf-f+)^{\frac{1}{n+1}}$.

Et in Hypotheſi gravitatis ſimplici diſtantiæ reciproce proportionalis, habetur $\text{Log.}\left(\frac{r}{a}\right) = -\frac{f}{2p}$. Ex quo elicitur Log. C A — Log. C P $= \frac{f}{2p}$.

Patet quod n exiſtente numero affirmativo, integro, ſeu fracto, hoc eſt in omnibus hypotheſibus gravitatis directe proportionalis alicui diſtantiæ dignitati, diameter æquatoris axe revolutionis major ſemper erit. Sed ſi ſit n numerus aliquis negativus, hoc eſt, ſi gravitas propornalis ſit inverſe alicui dignitati diſtantiæ, habebitur C A. C P :: $(2p)^{\frac{1}{-n+1}}\ (2p+nf-f)^{\frac{1}{-n+1}}$; nunc ſi $n < 1$, ſit $k = 1 - n$; & habebitur C A. C P :: $(2p)^{\frac{1}{k}}.\ (2p-kf)^{\frac{1}{k}}$; & ſi $n > 1$, ſit

$n - 1$

$n - 1 = k$; & habebitur $CA . CP :: (2p)^{\frac{1}{-k}} . (2p + kf)^{\frac{1}{-k}}$, vel $CA . CP :: (2p + kf)^{\frac{1}{k}} . (2p)^{\frac{1}{k}}$. Inſuper invenimus quod n exiſtente $= -1$, habetur $\text{Log. } CA - \text{Log. } CP = \frac{f}{2p}$. Ex quibus patet nullam eſſe hypotheſin in qua diameter æquatoris non ſuperet meridiani diametrum.

Sphæroidum figura, ut ſatis apparet, a ratione, quam habet vis centrifuga ad gravitatem, dependet. Nunc, qualis eſſe poſſit in quibuſdam hypotheſibus iſta ratio, videamus, & quæ inde figura ſphæroidibus eveniet.

Si gravitas uniformis ſupponatur, erit $n = 0$ & habebitur $CA . CP :: 2p . 2p - f$. Itaque in terra ubi vis centrifuga ſub æquatore 289am gravitatis partem æquat, ſi quæratur ratio quam habet diameter æquatoris ad axem in hypotheſi gravitatis uniformis (ponendo 289 pro p, & 1 pro f) habebitur $CA . CP :: 578, 577$.

Poſſet vis centrifuga æquari gravitati, quod obtineret ſi terræ revolutio diurna 17 vicibus celerior redderetur; & tunc haberetur $CA . CP :: 2 . 1$. Sed ſi revolutio magis ac magis cita fieret, partes ſucceſſive diſſiparentur donec tandem terra ad atomum unicam redigeretur. Ex quo patet quod in hac hypotheſi gravitatis uniformis, terra circa polos nunquam poteſt eſſe depreſſior quam ſi diameter æquatoris ſit duplo major axe revolutionis. In hoc caſu terra conſtaret ex duobus paraboloidibus, ſicut invenit D. Huygens *in tractatu de cauſa gravitatis* hac hypotheſi particulari quam ſolam examinavit.

Si gravitas diſtantiæ a centro proportionalis ſtatuatur, erit $n = 1$, & habebitur $CA . CP :: \surd p . \surd (p - f)$. Si igitur vis centrifuga gravitati fieret æqualis, diameter æquatoris axe revolutionis fieret infinite major. Hoc eſt, ſphærois planum tantum circulare foret. Et cum in hac hypotheſi vis centrifuga ad gravitatem omnes poſſit habere rationes, à ratione nullâ, uſque ad æqualitatis rationem, patet æquatoris diametrum ad axem revolutionis omnes has rationes habere poſſe; & ſphæroidem quæ in hac hypotheſi ſemper eſt Ellipſois, poſſe eſſe omnes Ellipſoides à ſphæra uſque ad circulum. Sed in hac etiam hypotheſi, vis centrifuga ultra creſcere nequit.

Si gravitas quadrato diſtantiæ reciproce proportionalis ponatur, erit $n = -2$; & habebitur $CA . CP :: 2p + f . 2p$. Ex quo liquet in hac hypotheſi vim centrifugam ſemper creſcere poſſe, vel quod eodem redit, motum revolutionis citiorem ſemper fieri poſſe, nec tamen ſphæroidis partes diſſiparentur.

Scholion. Cæterum, ex his omnibus hypotheſibus nullam quaſi in natura revera datam hic uſurpo: ſiquidem interiores corporum partes non gravitant verſus centrum aliquod unicum juxta proportionem quamvis diſtantiarum ab hoc centro in corporibus poſito. Attractio partium ex forma corporis dependet, ut & viciſſim forma dependet ex attractione. Idcirco omnes hæ determinationes, ſunt magis mathematicæ quam phyſicæ. Unde fit, quod D. Newton in determinatione axis terræ & diametri æquatoris rationem invenerit diverſam ab Huygeniana & a noſtris, nempe eam quæ eſt inter 229 & 230. Summus vir ſolutionem mere geometricam per hypotheſes neglexit, ut naturæ magis conſentaneam daret.

Prob. II. Poſito quod materia fluens circa axem extra fluentum ſumtum, attrahatur verſus centrum in hoc axe poſitum vi alicui diſtantiæ a centro dignitati proportionali; dum interea propter fluenti partium attractionem mutuam, fit altera attractio verſus aliud centrum intra fluentum ſumtum, quæ in quavis ſectione fluenti revolutionis perpendiculariter per centrum exterius facta, ſit alicui diſtantiæ a centro interiori dignitati proportionalis: invenire figuram quam fluentum induet.

Fig. 162. *Solutio.* Sit A D P a d Q A ſectio fluenti gyrantis circa axem Λ λ per planum revolutioni rectum quod tranſit per centrum γ facta. Sit γ centrum virium centripetarum extra fluentum ſumtum; & C centrum verſus quod partes fluenti attrahuntur in ſectione ſumtum.

Ut fluidi partes in æquilibrio maneant, oportet pondus cujuſque columnæ C D tum a gravitate verſus γ, tum verſus C, tum a vi centrifuga ortum, idem ubique maneat.

Sit ergo gravitas in A verſus γ, data & $= \pi$, gravitas in A verſus C, data & $= p$, & vis centrifuga in A, etiam data & $= f$. Sit A C $= a$, C $\gamma = b$, $c g = r$; ſinus ang. D C P $= b$ pro radio $= 1$; erit G L $= hr$, & è γ demiſſa perpendiculari γ R in radium C D productum, erit C R $= hb$, & γ G $=$ (per 12^m Elem. lib. 2.) $\surd\,(bb + 2hbr + rr.)$

Jam cum ſit gravitas in A verſus $\gamma = \pi'$; dicendo $\pi . \pi' :: (a+b)^m$ $(bb + 2hbr + rr)^{\frac{1}{2}m}$ habebitur gravitas in G ſeu $\pi' =$

$$\frac{\pi\,(bb + 2hbr + rr)^{\frac{1}{2}m}}{(a+b)^m}.$$

Et ut verſus C derivetur, dicatur $\pi' . \pi'' ::$ G γ. G R, vel

$$\frac{\pi\,(bb + 2hbr + rr)^{\frac{1}{2}m}}{(a+b)^m} . \pi'' :: (bb + 2hbr + rr)^{\frac{1}{2}} . hb + r;$$

unde habetur vis ab attractione verſus γ, derivata verſus C, ſeu

$$''\pi = \frac{\pi(bb+r)(bb+2bbr+rr)^{\frac{m-1}{2}}}{(a+b)^m}.$$

Habetur inſuper (cum gravitas in A verſus C, ſit $=p$) gravitas in G verſus C $= \frac{pr^n}{a^n}$; Gravitas ergo tota verſus C ex gravitatibus ambabus verſus γ & C orta habebitur $= \frac{\pi.(bb)(bb+2bbr+rr)^{\frac{m-1}{2}}}{(a+b)} + \frac{pr^n}{a^n}$;

Nunc cum ſit vis centrifuga in A, $=f$; dicendo $f.f' :: a+b.b+br$, habetur vis centrifuga in G ſeu $f' = \frac{f(b+br)}{a+b}$; & ut pars iſtius vis quæ verſus D trahit inveniatur; fiat $f'.f'' ::$ GH. GK, vel $\frac{f(b+br)}{a+b}.''f :: 1.h$; unde habetur vis gravitati verſus C oppoſita ſeu $''f = \frac{fb(b+br)}{a+b}$.

Vis ergo verſus C ex omnibus his viribus reſultans, erit

$$\frac{\pi(bb+r)(bb+2bbr+rr)^{\frac{m-1}{2}}}{(a+b)^m} + \frac{pr^n}{a^n} - \frac{fb(b+br)}{a+b}.$$

Concipiendo ergo ut in primo problemate columnam C D, ex infinitis cylindrulis $\dot{r}$ compoſitam habebitur $\mathrm{F}\left(\frac{\pi(bb+r)(bb+2bbr+rr)^{\frac{m-1}{2}}}{(a+b)^m} + \frac{pr^n}{a^n} - \frac{fb(b+br)}{a+b}\right)\dot{r}$, quod æquari debet ali-

cui

cui conſtanti ponderi. Erit ergo $\frac{\pi(bb+2bhr+rr)^{\frac{m+1}{2}}}{\overline{m+1}.(a+b)^{m}}+\frac{pr^{n+1}}{(n+1).a^{n}}-\frac{fbhr}{a+b}-\frac{fbhrr}{2.(a+b)}=A.$

Ut corrigatur hæc æquatio, oportet quando $h=1$, eſſe $r=a$; tunc ergo habetur $\frac{\pi(a+b)}{m+1}+\frac{pa}{n+1}-\frac{fab}{a+b}-\frac{faa}{2(a+b)}=A.$ Et æquatio corecta, erit $\frac{\pi(bb+2bhr+rr)^{\frac{m+1}{2}}}{(m+1).(a+b)^{m}}+\frac{pr^{n+1}}{(n+1)a^{n}}-\frac{fbbr}{a+b}-\frac{fbhrr}{2(a+b)}=\frac{\pi(a+b)}{m+1}+\frac{pa}{n+1}-\frac{fab}{a+b}-\frac{faa}{2(a+b)};$

Vel ſcribendo c pro $a+b$, & q pro $(m+1)\times(n+1)$ $2(n+1)$

$$\pi a^{n}(bb+2bhr+rr)^{\frac{m+1}{2}}+2(m+1)pc^{m}r^{n+1}-2qfa^{n}bc^{m-1}hr-qfa^{n}c^{m-1}bhrr=2(n+1)\pi a^{n}c^{m+1}+2(m+1)pa^{n+1}c^{m}-2qfa^{n+1}bc^{m-1}-qfa^{n+1}c^{m-2}.$$

Patet, in omnibus hypotheſibus, ſectionem fluenti eſſe curvam algebraicam, exceptis tantum hypotheſibus attractionis verſus γ vel verſus C in ratione ſimplicis diſtantiæ inverſâ; nam ſi ſit tantum $m=-1$, habebitur pro ſectione fluenti

$$\frac{\pi(a+b)}{2}L(bb+2bhr+rr)+\frac{pr^{n+1}}{\overline{n+1}.a^{n}}-\frac{fbbr}{a+b}-\frac{fbhrr}{2(a+b)}=\frac{\pi(a+b)}{2}L(a+b)^{2}+\frac{pa}{n+1}-\frac{fab}{a+b}-\frac{faa}{2(a+b)}.\text{ vel. }\frac{\pi c}{2}L\left(\frac{bb+2bhr+rr}{cc}\right)=-\frac{pr^{n+1}}{(n+1)a^{n}}+\frac{fbbr}{c}+\frac{fbhrr}{2c}+\frac{pa}{n+1}-\frac{fab}{c}-\frac{fac}{2c}.$$

Et ſi tantum $n=-1$, habebitur $\frac{\pi(bb+2bhr+rr)^{\frac{m+1}{2}}}{(m+1.(a+b)^{m}}+paLr-\frac{fbbr}{a+b}-\frac{fbhrr}{2(a+b)}=\frac{\pi(a+b)}{m+1}+paLa-\frac{fab}{a+b}-$

faa

$$\frac{faa}{2(a+b)}.\ \text{vel}\ paL\left(\frac{r}{a}\right)=-\frac{\varpi(bb+2bbr+rr)^{\frac{m+1}{2}}}{(m+1).\ c^{m}}+\frac{fbbr}{c}+\frac{fbbrr}{2c}+\frac{\varpi c}{m+1}-\frac{fab}{c}-\frac{faa}{2c}.$$

Sed ſi ſint ſimul $m=-1$, & $n=-1$, habebitur $\frac{\varpi(a+b)}{2}$ L

$$(bb+2bbr+rr)+paLr-\frac{fbbr}{a+b}-\frac{fbbrr}{2(a+b)}=\frac{\varpi(a+b)}{2}L\ (a+b)_{2}+paLa-\frac{fab}{a+b}-\frac{faa}{2(a+b)}.\ \text{vel}\ \frac{\varpi c}{2}L\left(\frac{bb+2bbr+rr}{cc}\right)+paL\left(\frac{r}{a}\right)=\frac{fbbr}{c}+\frac{fbbrr}{2c}-\frac{fab}{c}-\frac{faa}{2c}.$$

Si deſideretur æquatio ſectionis fluenti per coordinatas rectangulas; faciendo C E $=x$ & D E $=y$ habebuntur duæ æquationes $rr=xx+yy$ & $br=y$, quarum ope exterminabuntur r & b ex æquationibus ſupra inventis; & habebitur pro caſu generali, $2(n+1)\varpi a^{n}(bb+2by+yy+xx)^{\frac{m+1}{2}}+2(m+1)pc^{m}(xx+yy)^{\frac{n+1}{n}}-2qfa^{n}bc^{m-1}y-qfa^{n}c^{m-1}yy=2(n+1)\varpi a^{n}c^{m+1}+2(m+1)pa^{n+1}c^{m}-2qfa^{n+1}bc^{m-1}-qfa^{n+1}c^{m-1}$.

Et eodem modo in caſibus $m=-1$, $n=-1$, reperientur æquationes per coordinatas rectangulas.

Ut curvam P A Q invenimus, ita quoque invenietur curva P a Q mutatis mutandis. Nam tunc ſi ſit gravitas in a verſus γ data & $=\varpi$, gravitas in a verſus C $=p$, vis centrifuga in $a=f$; C $a=a$. C $\gamma=b$, C $g=r$, $gl=br$, & $\gamma g=\surd(bb-2bbr+rr)$ invenietur gravitas in g verſus C, ab attractione verſus γ orta, $\varpi=$

$$\frac{\varpi(bb-r)(bb-2bbr+rr)^{\frac{m-1}{2}}}{(b-a)^{m}}$$

Habetur inſuper gravitas in g verſus C, $p=\frac{pr^{n}}{a^{n}}$.

Sic etiam vis centrifugæ pars in *g* quæ trahit verſus C invenietur $f'' = \frac{fb\,(b - br)}{b - a}$.

Sed hæ poſteriores vires nunc primæ opponuntur. Habebitur ergo

$$F\left(\frac{\varpi\,(-bb + r)\,(bb - 2bbr + rr)^{\frac{m-1}{2}}}{(b-a)^{m}} + \frac{pr^{n}}{a^{n}} + \frac{fb\,(b - br)}{b-a}\right)\dot{r} = A.$$

Unde deducitur

$$\frac{\varpi\,(bb - 2bbr + rr)^{\frac{m+1}{2}}}{(m+1)\,(b-a)^{m}} + \frac{pr^{n+1}}{(m+1)\,a^{n}} + \frac{fbbr}{b-a} - \frac{fbbrr}{2\,(b-a)} = \frac{\varpi\,(b-a)}{m+1} + \frac{pa}{n+1} + \frac{fab}{b-a} - \frac{faa}{2\,(b-a)}.$$

Et in caſibus $m = -1$, $n = -1$, invenientur ut ſupra æquationes ſectionum, debitis tantum ſignis mutatis.

Et per has æquationes radiales invenientur æquationes ad coordinatas ut factum eſt pro curva P A Q.

Et cum pondus columnæ tam in ſuperiori quam in inferiori curva debeat idem eſſe, habebitur æquatio inter pondus A in curva ſuperiori, & pondus in A inferiori, ex qua determinabitur C*a* pro determinata CA, & ſic ſectio fluenti integra determinabitur.

Quæcunque ſit hypotheſis gravitatis, ſemper pro dato angulo DCP, radius C D obtineri poteſt datæ longitudinis, & ſic figura fluenti vel craſſior vel tenuior fiet, & quidem modis infinitis; ponendo in æquatione pro *b* & *r* valores determinatos. Sic fieri poteſt ut puncta P & Q coeant, ſcribendo *o* pro *b* & *r*; & tunc ſectio fluenti ex duabus ovalibus figuris in C junctis conſtabit. Nam infinitæ rationes inter ϖ, *p*, & *f* quæ ad id efficiendum conveniunt, obtinebuntur.

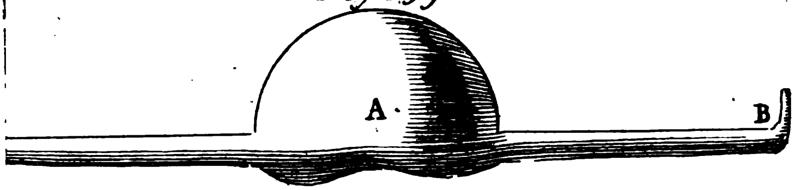
Si ex. grat. ultimum hoc deſideretur, nempe ut P & Q coeant in C, habebitur $2\,(n+1)\,\varpi\, b^{m+1} = 2\,(n+1)\,\varpi\, c^{m+1} + 2\,(m+1)\,pac^{m} - 2qfabc^{m-1} - qfaac^{m-1}$. Unde eliciuntur infinitæ rationes inter ϖ, *p*, & *f*.

Si ponatur gravitas tum verſus γ, tum verſus C ſimplici diſtantiæ a centro proportionalis; ſecti fluenti erit coniſectio. Et ſi tunc deſideretur ut puncta P, Q & C coeant, figura ex duabus Ellipſibus in C junctis, conſtabit.

Nunc ſi diſtantia Cγ evaneſcat, vel duo centra coeant; erit $b = a$, & $c = a$; & fluentum fiet ſphærois.

Si inſuper ponatur $m = n$, & $\varpi = o$, æquatio generalis ſectionis flu-

Fig. 159



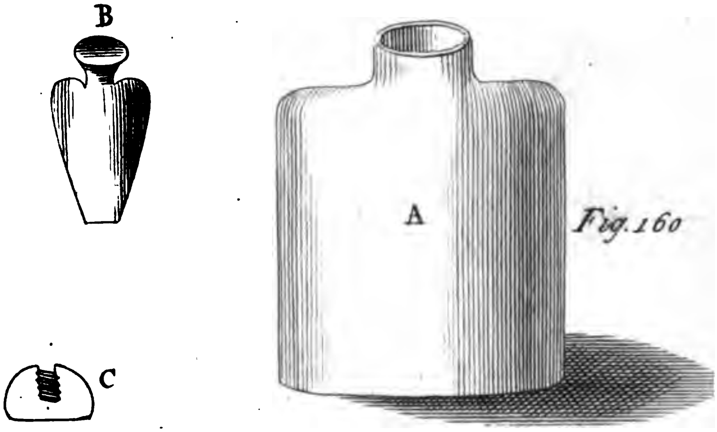
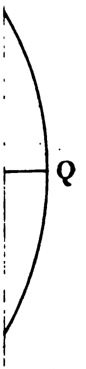


Fig. 160

ig. 161



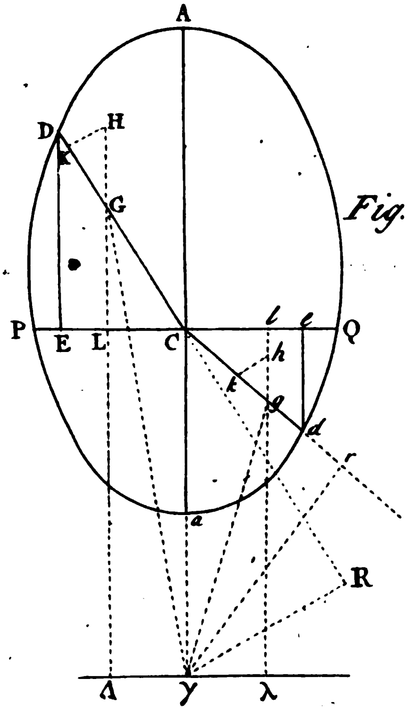


Fig. 162

enti fiet $2pr^{n+1} - (n+1)fa^{n-1}bbrr = (2p - nf - f)a^{n+1}$. Vel in caſu $n = -1$, $2paL\left(\frac{r}{a}\right) = \frac{fbbrr}{a} - fa$, ut invenimus in primo problemate quod eſt iſtius caſus tantum ſpecialis.

IV. In pervolvendo Opere *Petri Antonii Michelotti*, de ſeparatione fluidorum in corpore animali, qui *Venetiis* nuper ad nos delatus eſt, pluribus in locis Diſſertationem meam de Motu aquarum fluentium, in *Actis Philoſoph.* N° 355. ante aliquot annos editam, non leviter notatam deprehendi. Cum autem alia ex iis, quæ reprehendit, ex minus perſpecto Diſſertationis meæ inſtituto profecta videantur; alia vero ita demonſtrari poſſint, ut ipſum ea mihi non difficulter conceſſurum putem: operæ pretium erit, ſi primo in loco Diſſertationes meæ propoſitum & conſilium paulo luculentius exponam; quod cum fecero, ad reliqua deinceps breviter expendenda progrediar.

Of the Motion of running Waters, by James Jurin R. S. Secr. N. 373. p. 179.

Principio igitur explicandum eſt, quid in Diſſertatione iſtâ intelligendum velim, per *Motum Aquæ ex imi vaſis foramine defluentis*. Eſt enim alius *Motus*, ſive *quantitas Motûs*, Aquæ, quæ ex vaſe per foramen delabitur; qui *Motus* eſt in ratione compoſitâ, ex ratione quantitatis aquæ dato quovis tempore effluentis ex foramine, & ratione velocitatis, quâcum effluit. Alius vero eſt *Motus* totius aquæ, ſeu Cataractæ aqueæ, quæ intra vas verſus foramen deſcendit, & mox effluxura eſt. Hic eſt in ratione ſummæ omnium factorum, ex ſingulis aquæ particulis, Cataractam conſtituentibus, ductis in velocitates earundem reſpectivas. Quorum Motuum cum alterum ſæpe pro altero accipi viderem, animus mihi erat poſteriorem illum in prædictâ Diſſertatione illuſtrare, ad calculum revocare, & liquoribus in Animalium corpore fluentibus applicare.

Hic ergo cum ſemper mihi intelligeretur per *Motum Aquæ defluentis*, ſive per *Motum Aquarum fluentium*, quod ex omnibus meis Propoſitionibus luculenter apparet, jure meo dicere poteram *Motum hunc a nemine adhuc, quod ſcirem, fuiſſe determinatum:* quippe quem nemo Mathematicorum, quos quidem ego viderim, nec etiam verbo tenus attigerit. Quod cum ita ſit, miror profecto non animadvertiſſe neque acutiſſimum *Michelottum*, nec etiam ſubtiliſſimi & perſpicaciſſimi Ingenii Virum, *Johannem Bernoullium*, me in illius Diſſertationis Proœmio, quod toties citat & tantopere reprehendit *Michelottus*, ne verbum quidem ſcripſiſſe de velocitate, quâcum aqua effluit ex foramine, multo minus de Bernoullianâ determinatione illius velocitatis. Hoc ſi perſpexiſſet Vir Cl. noluiſſet ſane, pro ſuâ humanitate, tam inclementer & inique mecum agere, ut me *Bernoullianam Demonſtrationem extenuare verbis conari* [*] diceret, & *meram eſſe cavillationem id, quod Bernoullio objiciam*. Quod vero ſubjicit, verba iſta mea, " fieri om-

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[*] Pag. 131, 132.

“ nino non poſſe, ut Motus aliquis cum pondere quieſcente conferatur,” *ne umbram quidem habere rationis contra Bernoullianam Demonſtrationem pugnantis*,, libens agnoſco, quippe qui, cum iſta ſcriberem, tantum de Bernoullio, quantum de Sinenſium Imperatore cogitabam. Aio autem Lectorem quemvis non iniquum, neque præjudiciis occupatum, ne *umbram quidem* veriſimilitudinis reperturum, quod iſta verba ad Bernoullianam Demonſtrationem quicquam pertineant, quibus ſcilicet de re longe diverſa agatur, nempe de quantitate Motûs totius aquæ verſus foramen contendentis. Quoniam vero ita penitus inſedit animo Viri Doctiſſimi illa Demonſtratio, eandem in illius gratiam, ubi prius meipſum ab reliquis ejus Animadverſionibus vindicavero, ad examen revocare decrevi.

Ad *Motum* prædictum definiendum non alio nobis opus erat, quam Theoremate noſtro generali, quod tertio loco poſuimus: ſed cum Curvæ Hyperbolicæ Newtonianæ proprietatem, quâ Cataractam aquæ deſcendentis format, non indignam cenſeremus contemplatione Geometrarum, voluimus obiter quædam de Cataractâ illâ præmittere. Hanc autem ex Viri Incomparabilis, *Prop.* 36. *Lib.* 2. *Princip.* deſumptam proponebam, non ut *ex Tripode editam*, ſed evidentiâ Mathematicâ, omnibus oraculis certiore, munitam.

Quod enim Cataracta talis formari debeat ex aquâ libere deſcendente, & acceleratâ in modum corporum omnium gravium, quam nullâ aliâ aquâ circumcingatur, aperta res eſt; ut patet *Newtoni* Propoſitionem attente perlegenti. Si etiam Cataracta glacie concavâ, figuræ Cataractæ aqueæ adamuſſim congruente, & propter ſummam polituram nullam reſiſtentiam adferente ambiatur; ea glaciem ne minimâ quidem vi premet, ſed tanget ſolum liberrime cadendo, unde nihil mutabitur non modo in figurâ, ſed nec etiam in velocitate Cataractæ deſcendentis. At ſi circumpoſita Glacies in aquam reſolvatur, neutiquam jam opus eſt tanto *Machinarum* & *Arietum validiſſimorum* apparatu, quos magno molimine * adduxerunt tum ipſe *Vir Cl.* tum etiam Geometra Eximius, *Johannes Bernoullius*, ad *fragilem* noſtram *Cataractam confringendam & comminuendam*; quippe quam ipſe prius *Newtonus* hiſce verbis, † *Liqueſcat jam glacies in vaſe*; *&c.* diſſolverit penitus ac diſſipaverit. Nullo igitur aut *Genio* nobis opus eſt, aut *Erythræi Maris Miraculo*, ad Cataractam iſtam ſive indicandam, ſive conſervandam, quippe qui non adeo uſque ſtolidi aut inſulſi ſimus, ut conſervatum iri eam ſperemus ab omni aquæ circumpoſitæ communione puram & illibatam. Ignoſcat autem nobis, pro æquitate ſuâ, *Michelottus*, quod, quæ Providentiſſimus *Newtonus* de glacie ambiente, & eâdem poſtea in aquam reſolutâ fuſius tradiderit, ea noſmet Lectorem ex ipſo potius Auctore petere voluerimus, quam ingratâ & minime nobis neceſſariâ repetitione detineri.

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* Pag. 128, 129, 130.

† Princip. Pag. 304.

Non diffitemur ſane paulum diſcriminis, ut id obiter notemus, inter caſum a *Newtono* poſitum & noſtrum intereſſe. Quem enim fingit ille Cylindrum glaciei, velocitate datâ uniformi deſcendentem, ac liqueſcentem & in aquam converſum, quam primum ſuperficiem attingit aquæ vaſe contentæ, in eum ſcilicet finem, ut vas ſemper æque plenum conſervetur; hunc nos omiſimus, & ejus loco ſuperficiem aquæ infinitam poſuimus, ut eâ ratione integrum Solidum, ſive Cataractam Hyperbolicam repræſentaremus. At hæc poſitio nihil mutat neque in velocitate, nec in *Motu* aquæ decurrentis.

Quod autem * ait Vir. Cl., *me ſumere, quod eſt in contentione*, & paulo infra, *ceſſare igitur quæſtionem, & totam Demonſtrationem abire in Hypotheſin*, non mehercule intelligo, quid ſibi velit. Mihi enim, in loco citato, nulla movebatur quæſtio de velocitate aquæ effluentis, nec demonſtrationem ullam de eâ velocitate adferebam, ſed id unum agebatur, ut ex poſitâ illâ velocitate Æquationem Curvæ Hyperbolicæ Newtonianæ deducerem. Velocitatem nempe aquæ effluentis jam ante determinaveram, vel etiam, ſi placet, ſumpſeram, poſitis ſcilicet iis, quæ a *Newtono* poſita fuerant, aquam nempe gravitatis vi libere cadere, & inter cadendum accelerari. Hoc autem quî fieri poſſet, prius tradiderat *Newtonus*, ponendo aquam per glaciem politiſſimam ambientem, vel etiam per eandem in aquam ſolutam, ſed quietem adhuc ſervantem, tanquam per infundibulum, ſine ullâ reſiſtentiâ tranſire; quod in eum finem ponebatur, ut ſimplicior & magis Mathematica redderetur Problematis ſolutio.

Libet hic loci, propter argumenti affinitatem, erroris meminiſſe, cujus *Newtonum*, *Hugenium*, *Keillium* temere nimis, uti nobis videtur, ex Bernoullianæ demonſtrationis fiduciâ † incuſat *Michelottus*; quod ſcilicet vim, quâ totus aquæ exilientis *Motus* generari poteſt, æqualem ſtatuerint ponderi Cylindricæ columnæ aquæ, cujus baſis eſt foramen, cujuſque altitudo dupla eſt altitudinis aquæ vaſe contentæ. Hanc paucis admodum verbis, nec tamen idcirco minus perſpicue, demonſtravit *Newtonus* in Corollario ſecundo Propoſitionis ſupradictæ. Potuiſſet alia quoque deduci demonſtratio ex contemplatione Cataractæ integræ Hyperbolicæ, quæ huic Cylindro æqualis eſt, cujuſque pondus totum in aquæ deſcenſum impenditur: ſed hâc minime opus eſt, cum idem ex ipſâ Propoſitione Bernoullianâ, quam toties laudat, ac tam vehementer defendit *Michelottus*, apertiſſime ſequatur. Id nullo negotio animadvertet *Vir Doctiſſimus*, ſi ſepoſitâ parumper Columnæ foramini incumbentis conſideratione, calculo inſtituto, ex mole aquæ dato quovis tempore ex foramine effluentis, & ex velocitate, quâcum aquam effluere ſtatuit *Bernoullius*, ejus aquæ *Motum* determinare voluerit, & deinde pondus invenire, quod eodem dato temporis ſpatio, libere cadendo gravitatis vi, eandem *Motûs* quantitatem generare poſſit. Hoc autem pondus reperiet ponderi duplæ Columnæ aquæ foramini inſiſtentis æquale, prorſus uti definivit *Newtonus* in Corollario prædicto. Idem vero pondus, alteri Libræ Radio appenſum, ab impetu aquæ, cum primum ex foramine effluit, continuato rivo in alterum Libræ æqualem Radium impingentis, atque ſtatim poſt impulſum delabentis, in quiete ſuſtinebitur; quod poſito calculo facile patebit.

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* Pag. 127. † Pag. 112, 113.

Videor mihi non malam gratiam a *Michelotto*, initurus, ſi altero inſuper * præjudicio, quo & alios plures teneri video, ipſum liberavero. *Newtonus*, *Prop.* 37. *Lib.* 2. *Princip. primæ editionis*, aquam demonſtravit ex foramine in fundo vaſis eâ cum velocitate erumpere, quâ aſſurgere poſſit ad dimidiam altitudinem aquæ in vaſe exiſtentis. Demonſtrandi rationem nemo refellit; concluſionem plures redarguunt. Experientia, inquiunt, contradicit, quâ deprehenditur aqua exiliens ad totam altitudinem aſſurgere: quin etiam *Newtonus* ipſe in Problematis ejuſdem ſolutione, *Prop.* 36. *Lib.* 2. *editionis ſecundæ*, eam tribuit aquæ velocitatem, quâ ad totam altitudinem proſilire poſſit; adeoque ipſe ſibi contradicere videtur. Atqui ſi res iſta accuratius & cum judicio perpendatur, reperietur primæ ſolutioni *Newtonianæ* & cum ſecundâ, & cum experientiâ ipsâ, optime convenire. Nam in ſecundâ ſolutione, aquæ venam exilientem, ad parvam a foramine diſtantiam contractiorem diametro ſtatuit *Vir Perſpicaciſſimus*, quam in ipſo foramine, in ratione 21 ad 25. Eſt itaque ſectio venæ in eâ diſtantiâ, ad foramen ipſum, ut 21 x 21, ad 25 x 25, h. e. ut 1 ad $\sqrt{2}$ proxime. Cumque eadem aquæ quantitas, ſive per foraminis, ſive per venæ contractæ ſectionem, dato tempore perfluat, & proinde velocitates aquæ in iis ſectionibus ſint in ratione ipſarum ſectionum reciprocâ; erit velocitas in foramine ad velocitatem venæ contractæ, ut 1, ad $\sqrt{2}$; proinde, ſi ea ſit velocitas venæ contractæ, quâ aqua proſiliat ad integram altitudinem aquæ in vaſe, non major erit aquæ velocitas in ipſo foramine, quam quâ ad dimidiam altitudinem deferatur. Conſentiunt itaque inter ſe hæ duæ ſolutiones; & experientia porro cum iiſdem conſentire deprehenditut. Nam ſi per alterutram earum ſolutionum, ex definitâ velocitate, quâ aqua, ſive per foramen, ſive per venam contractam, tranſire ſtatuitur, calculo inſtituto inveniatur quantitas aquæ effluxuræ; reperietur eadem cum quantitate aquæ, quæ per experimenta effluere deprehenditur, proxime convenire. Certe experimentum ab ipſo *Newtono* ſumptum, adhibito foramine, cujus diameter erat quinque octavarum digiti partium, huic calculo reſpondit; ut etiam alia plura experimenta minoribus diametris *Londini* facta, quibus ipſe cum pluribus Regiæ Societatis Sodalibus, ante aliquot annos operam dedi. Abludunt quidem aliquantum † *Poleni* experimenta, ſed tamen minorem aquæ quantitatem exhibent, quam ſecundum hunc calculum, nunquam majorem, forte quod anguſtiora fuerint vaſa pro ratione amplitudinis foraminum.

Supereſt adhuc nobis conſideranda ‡ Animadverſio una, ſive potius Scrupulus *Viri Cl.* ex eo natus, quod in *Coroll.* 17. *Theorem.* 3. Diſſertationis prædictæ majorem ſtatuimus *Motum*, ſive *Impetum*, ſanguinis in Arteriis omnibus capillaribus ſimul ſumptis, quam in ipsâ Aorta.

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* Pag. 113. † Polin. de Caſtellis. ‡ Pag. 101, 102.

Hoc ut explicet ille neſcio quam Hypotheſin nobis affingit, de majore ſanguinis denſitate in capillaribus Arteriis, quam in Aorta. Nos vero nullam ejuſmodi conditionem poſuimus, ſed Corollarium deduximus ex Theoremate præcedente, in quo agitur de *Motu* aquæ per Canalem plenum quemcunq; fluentis: unde patet ſanguinem non aliter conſiderari in noſtris Corollariis, quam quatenus fluidus eſt & aquam æmulatur. Sed patet inde provenire Scrupulum *Viri Cl.* quod per ſanguinis *Impetum* intelligat, *quantitatem Motûs ejus effectam ex multiplicatione velocitatis per maſſam dato tempore transfluentem.* Atqui hic longe alius eſt ac noſter ſanguinis *Motus*, ſive *Impetus*, quippe qui in iſto Theoremate *æqualis* ſtatuitur *Motui molis aquæ, quæ dato quovis tempore effluit ex Canali, cujuſque ea ſit velocitas, quâ percurratur eodem dato tempore ſpatium æquale longitudini Canalis.* Facile autem ex hoc Theoremate fluit Corollarium prædictum, quippe cum dato tempore transfluat eadem ſanguinis moles per Aortam & per Arterias capillares, major autem ſit Canalis longitudo ex Aortâ & Arteriis capillaribus compoſiti, quam Aortæ ſolius. Hoc eo libentius notavi, quod videam non ſolum *Michelottum*, ſed alios etiam ſcriptores Mathematicos, pluribus in locis, ubi agitur de potentiis, quæ liquorem per Canales eodem plenos aut in motum impellunt, aut effluentem ſiſtunt, nihil aliud conſiderare præter molem & velocitatem fluidi effluentis; quum debuiſſet etiam longitudinis ipſorum Canalium ratio haberi. Nam cæteris paribus, eo difficilius vel expellitur fluidum ex pleno Canali, vel in effluxu ſiſtitur, quo Canalis longior fuerit; quippe quum tota moles fluidi Canale contenti in motum concitandus ſit, priuſquam ulla pars ejuſdem effluere poſſit ex orificio; ſicuti etiam tota eadem moles neceſſario ſiſtenda eſt, ſi exitum parti jamjam effluxuræ prohibere volueris.

Accedo jam ad expendendam Demonſtrationem de velocitate aquæ ex foramine vaſis pleni effluentis. In quem finem legi diligenter ac relegi, tum quæ protulit *Michelottus* de * principiis illius Demonſtrationis, tum ipſam Demonſtrationem ab *Hermanno* communicatam in *Actis Lipſienſibus*, Anni 1716. Quæ quamvis nullâ ex parte mihi ſatisfaciat, tamen cum imbecillitatis meæ conſcius longe facilius accidere poſſe ſentiam, ut ipſe a vero aberrem, quam ut Virum nobiliſſimis inventis clarum, & acerrimo, ſi quis alius ingenio pollentem, erroris alicujus redarguam; cunctanter idcirco & dubitantius proponam, quid in illâ Demonſtratione minus firmum mihi videatur.

“ Fundamentum Demonſtrationis (*ſcribit Vir Cl.*) in hoc conſiſtit,
“ ut conſideretur guttula liquoris infima, & foramini vaſis immediate
“ incumbens, tanquam preſſa, vel (ut ego voco) animata a gravitate
“ quâdam acceleratrice quæ ſe habet ad gravitatem naturalem, ut altitudo
“ aquæ vel liquoris totius foramini vaſis incumbentis ad altitudinem
“ guttulæ, ſcilicet ut pondus abſolutum columnæ aquæ foramini
“ inſiſtentis ad pondus abſolutum guttulæ; Sic quippe nihil aliud re-

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* Pag. 131.

to. Idem vero pondus, alteri Libræ Radio appenſum, ab impetu aquæ, cum primum ex foramine effluit, continuato rivo in alterum Libræ æqualem Radium impingentis, atque ſtatim poſt impulſum delabentis, in quiete ſuſtinebitur; quod poſito calculo facile patebit.

Videor mihi non malam gratiam a *Michelotto*, initurus, ſi altero inſuper * præjudicio, quo & alios plures teneri video, ipſum liberavero. *Newtonus*, *Prop.* 37. *Lib.* 2. *Princip. primæ editionis*, aquam demonſtravit ex foramine in fundo vaſis eâ cum velocitate erumpere, quâ aſſurgere poſſit ad dimidiam altitudinem aquæ in vaſe exiſtentis. Demonſtrandi rationem nemo refellit; concluſionem plures redarguunt. Experientia, inquiunt, contradicit, quâ deprehenditur aqua exiliens ad totam altitudinem aſſurgere: quin etiam *Newtonus* ipſe in Problematis ejuſdem ſolutione, *Prop.* 36. *Lib.* 2. *editionis ſecundæ*, eam tribuit aquæ velocitatem, quâ ad totam altitudinem proſilire poſſit; adeoque ipſe ſibi contradicere videtur. Atqui ſi res iſta accuratius & cum judicio perpendatur, reperietur primæ ſolutioni *Newtonianæ* & cum ſecundâ, & cum experientiâ ipsâ, optime convenire. Nam in ſecundâ ſolutione, aquæ venam exilientem, ad parvam a foramine diſtantiam contractiorem diametro ſtatuit *Vir Perſpicaciſſimus*, quam in ipſo foramine, in ratione 21 ad 25. Eſt itaque ſectio venæ in eâ diſtantiâ, ad foramen ipſum, ut 21 x 21, ad 25 x 25, h. e. ut 1 ad ✓ 2 proxime. Cumque eadem aquæ quantitas, ſive per foraminis, ſive per venæ contractæ ſectionem, dato tempore perfluat, & proinde velocitates aquæ in iis ſectionibus ſint in ratione ipſarum ſectionum reciprocâ; erit velocitas in foramine ad velocitatem venæ contractæ, ut 1, ad ✓ 2; proinde, ſi ea ſit velocitas venæ contractæ, quâ aqua proſiliat ad integram altitudinem aquæ in vaſe, non major erit aquæ velocitas in ipſo foramine, quam quâ ad dimidiam altitudinem deferatur. Conſentiunt itaque inter ſe hæ duæ ſolutiones; & experientia porro cum iiſdem conſentire deprehenditur. Nam ſi per alterutram earum ſolutionum, ex definitâ velocitate, quâ aqua, ſive per foramen, ſive per venam contractam, tranſire ſtatuitur, calculo inſtituto inveniatur quantitas aquæ effluxuræ; reperietur eadem cum quantitate aquæ, quæ per experimenta effluere deprehenditur, proxime convenire. Certe experimentum ab ipſo *Newtono* ſumptum, adhibito foramine, cujus diameter erat quinque octavarum digiti partium, huic calculo reſpondit; ut etiam alia plura experimenta minoribus diametris *Londini* facta, quibus ipſe cum pluribus Regiæ Societatis Sodalibus, ante aliquot annos operam dedi. Abludunt quidem aliquantum † *Poleni* experimenta, ſed tamen minorem aquæ quantitatem exhibent, quam ſecundum hunc calculum, nunquam majorem, forte quod anguſtiora fuerint vaſa pro ratione amplitudinis foraminum.

Supereſt adhuc nobis conſideranda ‡ Animadverſio una, ſive potius Scrupulus *Viri Cl.* ex eo natus, quod in *Coroll.* 17. *Theorem.* 3. Diſſertationis prædictæ majorem ſtatuimus *Motum*, ſive *Impetum*, ſanguinis in Arteriis omnibus capillaribus ſimul ſumptis, quam in ipsâ Aorta.

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* Pag. 113. † Polin. de Caſtellis. ‡ Pag. 101, 102.

Hoc ut explicet ille neſcio quam Hypotheſin nobis affingit, de majore ſanguinis denſitate in capillaribus Arteriis, quam in Aorta. Nos vero nullam ejuſmodi conditionem poſuimus, ſed Corollarium deduximus ex Theoremate præcedente, in quo agitur de *Motu* aquæ per Canalem plenum quemcunq; fluentis: unde patet ſanguinem non aliter conſiderari in noſtris Corollariis, quam quatenus fluidus eſt & aquam æmulatur. Sed patet inde provenire Scrupulum *Viri Cl.* quod per ſanguinis *Impetum* intelligat, *quantitatem Motûs ejus effectam ex multiplicatione velocitatis per maſſam dato tempore transfluentem.* Atqui hic longe alius eſt ac noſter ſanguinis *Motus*, ſive *Impetus*, quippe qui in iſto Theoremate *æqualis* ſtatuitur *Motui molis aquæ, quæ dato quovis tempore effluit ex Canali, cujuſque ea ſit velocitas, quâ percurratur eodem dato tempore ſpatium æquale longitudini Canalis.* Facile autem ex hoc Theoremate fluit Corollarium prædictum, quippe cum dato tempore transfluat eadem ſanguinis moles per Aortam & per Arterias capillares, major autem ſit Canalis longitudo ex Aortâ & Arteriis capillaribus compoſiti, quam Aortæ ſolius. Hoc eo libentius notavi, quod videam non ſolum *Michelottum*, ſed alios etiam ſcriptores Mathematicos, pluribus in locis, ubi agitur de potentiis, quæ liquorem per Canales eodem plenos aut in motum impellunt, aut effluentem ſiſtunt, nihil aliud conſiderare præter molem & velocitatem fluidi effluentis; quum debuiſſet etiam longitudinis ipſorum Canalium ratio haberi. Nam cæteris paribus, eo difficilius vel expellitur fluidum ex pleno Canali, vel in effluxu ſiſtitur, quo Canalis longior fuerit; quippe quum tota moles fluidi Canale contenti in motum concitandus ſit, priuſquam ulla pars ejuſdem effluere poſſit ex orificio; ſicuti etiam tota eadem moles neceſſario ſiſtenda eſt, ſi exitum parti jamjam effluxuræ prohibere volueris.

Accedo jam ad expendendam Demonſtrationem de velocitate aquæ ex foramine vaſis pleni effluentis. In quem finem legi diligenter ac relegi, tum quæ protulit *Michelottus* de * principiis illius Demonſtrationis, tum ipſam Demonſtrationem ab *Hermanno* communicatam in *Actis Lipſienſibus*, Anni 1716. Quæ quamvis nullâ ex parte mihi ſatisfaciat, tamen cum imbecillitatis meæ conſcius longe facilius accidere poſſe ſentiam, ut ipſe a vero aberrem, quam ut Virum nobiliſſimis inventis clarum, & acerrimo, ſi quis alius ingenio pollentem, erroris alicujus redarguam; cunctanter idcirco & dubitantius proponam, quid in illâ Demonſtratione minus firmum mihi videatur.

“ Fundamentum Demonſtrationis (*ſcribit Vir Cl.*) in hoc conſiſtit,
“ ut conſideretur guttula liquoris infima, & foramini vaſis immediate
“ incumbens, tanquam preſſa, vel (ut ego voco) animata a gravitate
“ quâdam acceleratrice quæ ſe habet ad gravitatem naturalem, ut alti-
“ tudo aquæ vel liquoris totius foramini vaſis incumbentis ad altitudi-
“ nem guttulæ, ſcilicet ut pondus abſolutum columnæ aquæ foramini
“ inſiſtentis ad pondus abſolutum guttulæ; Sic quippe nihil aliud re-

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* Pag. 131.

cui conſtanti ponderi. Erit ergo $\frac{\pi(bb+2bbr+rr)^{\frac{m+1}{2}}}{\overline{m+1}.(a+b)^{m}}+\frac{pr^{n+1}}{(n+1).a^{n}}-\frac{fbbr}{a+b}-\frac{fbbrr}{2.(a+b)}=A.$

Ut corrigatur hæc æquatio, oportet quando $b=1$, eſſe $r=a$; tunc ergo habetur $\frac{\pi(a+b)}{m+1}+\frac{pa}{n+1}-\frac{fab}{a+b}-\frac{faa}{2(a+b)}=A.$ Et æquatio corecta, erit $\frac{\pi(bb+2bbr+rr)^{\frac{m+1}{2}}}{(m+1).(a+b)^{m}}+\frac{pr^{n+1}}{(n+1)a^{n}}-\frac{fbbr}{a+b}-\frac{fbbrr}{2(a+b)}=\frac{\pi(a+b)}{m+1}+\frac{pa}{n+1}-\frac{fab}{a+b}-\frac{faa}{2(a+b)};$

Vel ſcribendo c pro $a+b$, & q pro $(m+1)\times(n+1)$ $2(n+1)$ $\pi a^{n}(bb+2bbr+rr)^{\frac{m+1}{2}}+2(m+1)pc^{m}r^{n+1}-2qfa^{n}bc^{m-1}br-qfa^{n}c^{m-1}bbrr=2(n+1)\pi a^{n}c^{m+1}+2(m+1)pa^{n+1}c^{m}-2qfa^{n+1}bc^{m-1}-qfa^{n+1}c^{m-2}.$

Patet, in omnibus hypotheſibus, ſectionem fluenti eſſe curvam algebraicam, exceptis tantum hypotheſibus attractionis verſus γ vel verſus C in ratione ſimplicis diſtantiæ inverſâ; nam ſi ſit tantum $m=-1$, habebitur pro ſectione fluenti

$$\frac{\pi(a+b)}{2}L(bb+2bbr+rr)+\frac{pr^{n+1}}{\overline{n+1}.a^{n}}-\frac{fbbr}{a+b}-\frac{fbbrr}{2(a+b)}=\frac{\pi(a+b)}{2}L(a+b)^{2}+\frac{pa}{n+1}-\frac{fab}{a+b}-\frac{faa}{2(a+b)}.\ \text{vel.}\ \frac{\pi c}{2}L\left(\frac{bb+2bbr+rr}{cc}\right)=-\frac{pr^{n+1}}{(n+1)a^{n}}+\frac{fbbr}{c}+\frac{fbbrr}{2c}+\frac{pa}{n+1}-\frac{fab}{c}-\frac{fac}{2c}.$$

Et ſi tantum $n=-1$, habebitur $\frac{\pi(bb+2bbr+rr)^{\frac{m+1}{2}}}{(m+1.(a+b)^{m}}+paLr-\frac{fbbr}{a+b}-\frac{fbbrr}{2(a+b)}=\frac{\pi(a+b)}{m+1}+paLa-\frac{fab}{a+b}-$

faa

$$\frac{faa}{2\,(a+b)}.\ \text{vel}\ p\,a\,\mathrm{L}\left(\frac{r}{a}\right)=-\frac{\pi(bb+2\,bbr+rr)^{\frac{m+1}{2}}}{(m+1).\,c^{m}}+\frac{fbbr}{c}+\frac{fbbrr}{2c}+\frac{\pi c}{m+1}-\frac{fab}{c}-\frac{faa}{2c}.$$

Sed ſi ſint ſimul $m=-1$, & $n=-1$, habebitur $\frac{\pi(a+b)}{2}$ L

$$(bb+2\,bbr+rr)+p\,a\,\mathrm{L}\,r-\frac{fbbr}{a+b}-\frac{fbbrr}{2\,(a+b)}=\frac{\pi(a+b)}{2}\,\mathrm{L}\ (a+b)_{2}+p\,a\,\mathrm{L}\,a-\frac{fab}{a+b}-\frac{faa}{2\,(a+b)}.\ \text{vel}\ \frac{\pi c}{2}\,\mathrm{L}\left(\frac{bb+2bbr+rr}{cc}\right)+pa\mathrm{L}\left(\frac{r}{a}\right)=\frac{fbbr}{c}+\frac{fbbrr}{2c}-\frac{fab}{c}-\frac{faa}{2c}.$$

Si deſideretur æquatio ſectionis fluenti per coordinatas rectangulas; faciendo C E $=x$ & D E $=y$ habebuntur duæ æquationes $rr=xx+yy$ & $br=y$, quarum ope exterminabuntur r & b ex æquationibus ſupra inventis; & habebitur pro caſu generali, $2\,(n+1)\,\pi\,a^{n}\,(bb+2\,by+yy+xx)^{\frac{m+1}{2}}+2\,(m+1)\,p\,c^{m}\,(xx+yy)^{\frac{n+1}{n}}-2\,q\,f\,a^{n}\,b\,c^{m-1}\,y-q\,f\,a^{n}\,c^{m-1}\,yy=2\,(n+1)\,\pi\,a^{n}\,c^{m+1}+2\,(m+1)\,p\,a^{n+1}\,c^{m}-2\,q\,f\,a^{n+1}\,b\,c^{m-1}-q\,f\,a^{n+1}\,c^{m-1}$.

Et eodem modo in caſibus $m=-1$, $n=-1$, reperientur æquationes per coordinatas rectangulas.

Ut curvam P A Q invenimus, ita quoque invenietur curva P a Q mutatis mutandis. Nam tunc ſi ſit gravitas in a verſus γ data & $=\pi$, gravitas in a verſus C $=p$, vis centrifuga in $a=f$; C $a=a$. C $\gamma=b$, C $g=r$, $g\,l=b\,r$, & $\gamma\,g=\sqrt{(bb-2\,bbr+rr)}$ invenietur gravitas in g verſus C, ab attractione verſus γ orta, $\pi=$

$$\frac{\pi\,(bb-r)\,(bb-2\,bbr+rr)^{\frac{m-1}{2}}}{(b-a)^{m}}.$$

Habetur inſuper gravitas in g verſus C, $p=\frac{p\,r^{n}}{a^{n}}$.

Sic etiam vis centrifugæ pars in g quæ trahit verſus C invenietur $f'' = \frac{fb\,(b - br)}{b-a}$.

Sed hæ poſteriores vires nunc primæ opponuntur. Habebitur ergo

$$F\left(\frac{\pi\,(-bb+r)\,(bb-2bbr+rr)^{\frac{m-1}{2}}}{(b-a)^m} + \frac{pr^n}{a^n} + \frac{fb\,(b-br)}{b-a}\right)\dot{r} = A.$$

Unde deducitur

$$\frac{\pi\,(bb-2bbr+rr)^{\frac{m+1}{2}}}{(m+1)\,(b-a)^m} + \frac{pr^{n+1}}{(m+1)\,a^n} + \frac{fbbr}{b-a} - \frac{fbbrr}{2\,(b-a)} = \frac{\pi\,(b-a)}{m+1} + \frac{pa}{n+1} + \frac{fab}{b-a} - \frac{faa}{2\,(b-a)}.$$

Et in caſibus $m = -1$, $n = -1$, invenientur ut ſupra æquationes ſectionum, debitis tantum ſignis mutatis.

Et per has æquationes radiales invenientur æquationes ad coordinatas ut factum eſt pro curva P A Q.

Et cum pondus columnæ tam in ſuperiori quam in inferiori curva debeat idem eſſe, habebitur æquatio inter pondus A in curva ſuperiori, & pondus in A inferiori, ex qua determinabitur Ca pro determinata CA, & ſic ſectio fluenti integra determinabitur.

Quæcunque ſit hypotheſis gravitatis, ſemper pro dato angulo DCP, radius C D obtineri poteſt datæ longitudinis, & ſic figura fluenti vel craſſior vel tenuior fiet, & quidem modis infinitis; ponendo in æquatione pro b & r valores determinatos. Sic fieri poteſt ut puncta P & Q coeant, ſcribendo o pro b & r; & tunc ſectio fluenti ex duabus ovalibus figuris in C junctis conſtabit. Nam infinitæ rationes inter π, p, & f quæ ad id efficiendum conveniunt, obtinebuntur.

Si ex. grat. ultimum hoc deſideretur, nempe ut P & Q coeant iu C, habebitur $2\,(n+1)\,\pi\,b^{m+1} = 2\,(n+1)\,\pi\,c^{m+1} + 2\,(m+1)\,pac^m - 2qfabc^{m-1} - qfaac^{m-1}$. Unde eliciuntur infinitæ rationes inter π, p, & f.

Si ponatur gravitas tum verſus γ, tum verſus C ſimplici diſtantiæ a centro proportionalis; ſecti fluenti erit coniſectio. Et ſi tunc deſideretur ut puncta P, Q & C coeant, figura ex duabus Ellipſibus in C junctis, conſtabit.

Nunc ſi diſtantia Cγ evaneſcat, vel duo centra coeant; erit $b = o$, & $c = a$; & fluentum fiet ſphærois.

Si inſuper ponatur $m = n$, & $\pi = o$, æquatio generalis ſectionis fluenti

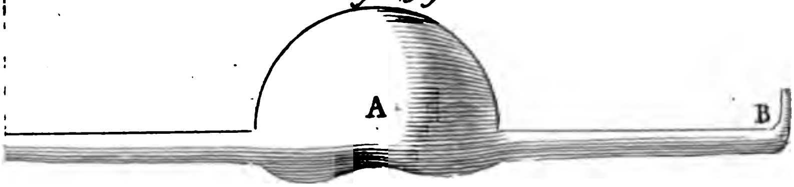


Fig. 159

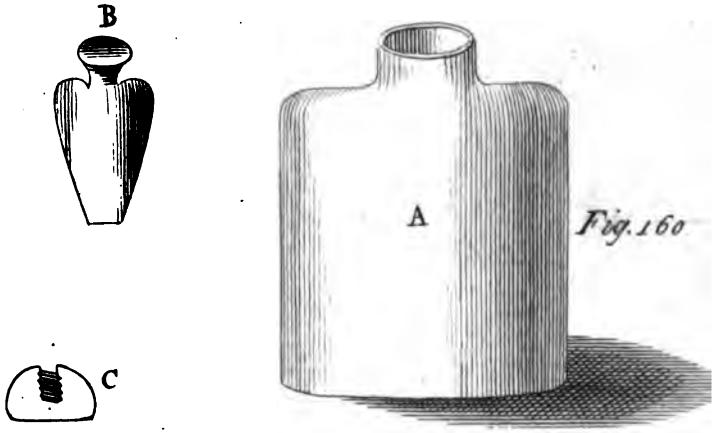
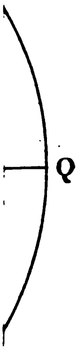


Fig. 160



ig. 161

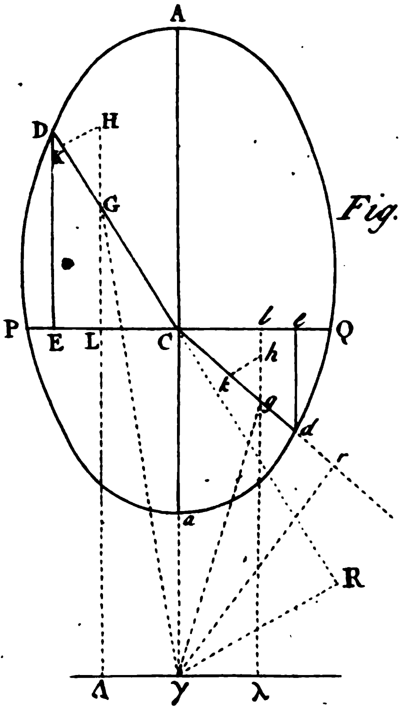


Fig. 162

enti fiet $2pr^{n+1} - (n+1)fa^{n-1}bbrr = (2p - nf - f)a^{n+1}$. Vel in caſu $n = -1$, $2paL\left(\frac{r}{a}\right) = \frac{fbbrr}{a} - fa$, ut invenimus in primo problemate quod eſt iſtius caſus tantum ſpecialis.

Of the Motion of running Waters, by James Jurin R. S. Secr. N. 373. p. 179.

IV. In pervolvendo Opere *Petri Antonii Michelotti*, de ſeparatione fluidorum in corpore animali, qui *Venetiis* nuper ad nos delatus eſt, pluribus in locis Diſſertationem meam de Motu aquarum fluentium, in *Actis Philoſoph.* N° 355. ante aliquot annos editam, non leviter notatam deprehendi. Cum autem alia ex iis, quæ reprehendit, ex minus perſpecto Diſſertationis meæ inſtituto profecta videantur; alia vero ita demonſtrari poſſint, ut ipſum ea mihi non difficulter conceſſurum putem: operæ pretium erit, ſi primo in loco Diſſertationes meæ propoſitum & conſilium paulo luculentius exponam; quod cum fecero, ad reliqua deinceps breviter expendenda progrediar.

Principio igitur explicandum eſt, quid in Diſſertatione iſtâ intelligendum velim, per *Motum Aquæ ex imi vaſis foramine defluentis*. Eſt enim alius *Motus*, ſive *quantitas Motûs*, Aquæ, quæ ex vaſe per foramen delabitur; qui *Motus* eſt in ratione compoſitâ, ex ratione quantitatis aquæ dato quovis tempore effluentis ex foramine, & ratione velocitatis, quâcum effluit. Alius vero eſt *Motus* totius aquæ, ſeu Cataractæ aqueæ, quæ intra vas verſus foramen deſcendit, & mox effluxura eſt. Hic eſt in ratione ſummæ omnium factorum, ex ſingulis aquæ particulis, Cataractam conſtituentibus, ductis in velocitates earundem reſpectivas. Quorum Motuum cum alterum ſæpe pro altero accipi viderem, animus mihi erat poſteriorem illum in prædictâ Diſſertatione illuſtrare, ad calculum revocare, & liquoribus in Animalium corpore fluentibus applicare.

Hic ergo cum ſemper mihi intelligeretur per *Motum Aquæ defluentis*, ſive per *Motum Aquarum fluentium*, quod ex omnibus meis Propoſitionibus luculenter apparet, jure meo dicere poteram *Motum hunc a nemine adhuc, quod ſcirem, fuiſſe determinatum:* quippe quem nemo Mathematicorum, quos quidem ego viderim, nec etiam verbo tenus attigerit. Quod cum ita ſit, miror profecto non animadvertiſſe neque acutiſſimum *Michelottum*, nec etiam ſubtiliſſimi & perſpicaciſſimi Ingenii Virum, *Johannem Bernoullium*, me in illius Diſſertationis Proœmio, quod toties citat & tantopere reprehendit *Michelottus*, ne verbum quidem ſcripſiſſe de velocitate, quâcum aqua effluit ex foramine, multo minus de Bernoullianâ determinatione illius velocitatis. Hoc ſi perſpexiſſet Vir Cl. noluiſſet ſane, pro ſuâ humanitate, tam inclementer & inique mecum agere, ut me *Bernoullianam Demonſtrationem extenuare verbis conari* * diceret, & *meram eſſe cavillationem id, quod Bernoullio objiciam*. Quod vero ſubjicit, verba iſta mea, " fieri om-

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Pag. 131, 132.

Receiver, and drives up the Water thro' the Elbow B, the forcing Valve *u*, and ſo up the forcing Pipe *a* 2 *a* 1: But when once the forcing Pipe (which here was 46 Foot high) is full, before any *Mercury* can enter into the Receiver, and force any Water out at the Top of the Pipe *a* 1, the *Mercury* between the Piſton and Barrel muſt riſe up to *q q* near 3½ Feet above the Bottom of the Receiver, and as it continues to riſe up to *p p*, the Water is thrown out with a Velocity proportionable to the Height that the *Mercury* is raiſed above the 14th Part of the Height of the Water. Now tho' the Friction of Solids is here avoided, it is plain that the *Mercury* muſt move from *m m* to *q q* without raiſing any Water, and that it can only force in going from *q q* to *p p*, and only ſuck in falling from *o o* to *m m*: And unleſs the Piſton is ſtopped a little while when at loweſt, the Water will not have time to run out: So likewiſe the Piſton muſt be ſtopped when at higheſt, that the Receiver may have time to fill.

Fig. 172. Mr. *Haskins* likewiſe propoſed another Way, repreſented in *Fig.* 172; where the ſame Letters repreſent the ſame Parts, only here the Barrel is moveable by the two Chains E 1 E 2, and inſtead of a ſolid Piſton, the hollow Cylinder C 1 *c c* is fixed, and the *Mercury* moving up and down in the lower Part of it, ſucks and forces the Water thro' the Elbow. The Figure repreſents the Engine ſucking by means of the *Mercury* hanging from *o o* to *m m*. In order to force, before any Water can be driven out, the *Mercury* in the inner Cylinder muſt deſcend from *o o* to *m m*, and riſe up to *p p* between that Cylinder and the Barrel; ſo that here alſo a great deal of Time is loſt, beſides the great Quantity of *Mercury* uſed, which is very expenſive; becauſe as much *Mercury* is moved every Stroke as the Water rais'd.

Theſe Difficulties very much puzzled Mr. *Haskins*, and quite diſcouraged ſome other Perſons that had got the Secret of the Invention, and were ſetting up againſt him. But when I had conſidered the Matter a little, tho' I had not time to contrive a Machine for it, I told him, That a little *Mercury* might be made to raiſe a great Quantity of Water, and there ſhould not be ſuch a Loſs of Time as in his Engines; but that I would have him find it out before I aſſiſted him farther. In a little time he found out the Contrivance repreſented in *Fig.* 174, and afterwards that of *Fig.* 173, which laſt was what I had thought of: And both theſe were alſo found out by the late Mr. *William Ureim*, who was an excellent Mechanick.

Fig. 173. Here the Barrel is moved as in *Fig.* 172, but the Plug *d d d* taking up a great deal of Space, there is occaſion for no more Mercury than what will make a concave Cylinder or Shell up to *p p* between the Barrel D 1 D 2, and the hanging Cylinder C 1 C 2 *c c*, when the Stroke is made for forcing; and a concave Cylinder between the Plug and C 1 C 2 *c c*, when the Suction is made. I gave Mr. *Haskins* the Proportions for an Engine this Way, of which he made a Draught, and ſhewed it to the Lord Chancellour about ſix Months ago. This I

mention here, that no body may endeavour to get a Patent for this Invention, to the Prejudice of Mr. *Haskins*'s Aſſignees; who, ſince his Death, have deſired me to aſſiſt them in perfecting the Engine.

Fig. 174. Here the Barrel with a third Cylinder *dddd* inſtead of the Plug of Fig. 173, is lifted up and down every Stroke, and the Water paſſes thro' *dddd*, the Mercury making a Shell ſometimes between the middle and inner Cylinder, as in the Suction; and ſometimes between the Barrel and the middle Cylinder, as in the forcing Stroke.

Mr. *Haskins* had contrived ſuch a Machine as is repreſented by this
Figure, and beſpoke the ſeveral Parts before he dy'd; and therefore
when I was deſired by his Aſſignees to direct the ſetting up the Ma-
chine, I was obliged to make uſe of the Pieces already made, in order
to ſave the Expence of a new Engine: And now the whole put toge-
Fig. 175. ther with ſome Alterations, make the Engine repreſented by Fig. 175.
as it is ſet up at my Houſe in *Weſtminſter*, and by the Force of one
Man, raiſes a Hogſhead of Water in little more than a Minute and a
half to the Height of 27 Feet. All the Fault of the Machine of
Fig. 175 is, that the Pendulum Handle *Ff* is too long, and the Bot-
tom of the middle Cylinder C ought to be juſt in the middle of the
Height to which the Water is to be raiſed, ſuppoſing three Copper
Cylinders to be as they are here: If likewiſe the Barrel D 1 D 2 work-
ed under the forcing Pipe, the Lift would be eaſier. Therefore I de-
Fig. 176. ſcribe the Machine with the ſmall Alteration repreſented in Fig. 176.

The ſucking and forcing Pipe and Valves are marked with the ſame Letters as in the other Figures; and the Chains E 1 E 2 muſt be ſuppoſed to hang from ſuch Pullies, and to be moved by ſuch a Pendulum as is in Fig. 175. The Barrel D 1 D 2 (called otherwiſe the outer Cylinder, and repreſented by the ſame Letters in Fig. 177.) has within it another Cylinder (called the inner Cylinder or Plug, as *dddd* Fig. 177.) between which two Cylinders a certain Quantity of Mercury is poured in, and the hanging Cylinder C coming down into the Mercury, a Stroke of 13 Inches may be made by the Motion of the Barrel, which in going down ſucks by making a Vacuum in C, and in going up forces the Water out of the Top of the forcing Pipe, performing the Office of a common Piſton; only that inſtead of Leather to make it tight to the Cylinder C, there is always a thin Shell of Quickſilver either between the middle Cylinder C and the inner one, (*dddd* Fig. 177.) as happens when the Suction is made, or between the middle and outer Cylinder, as happens in lifting up the Barrel to force. In the Suction, the Mercury is higher in the inner Shell than in the outer Shell, by a Height equal to a little more than $\frac{1}{14}$ Part of the Height of the Barrel above the Water to be raiſed: And in forcing, it is higher in the outer Shell than in the inner by a little more than $\frac{1}{14}$ of the Height of the Pillar of Water to be forced. And therefore if the Water is not required to be raiſed above 64 Feet, the Barrel ſhould move ſo as to make the Middle of its Stroke at the Height of 30 Feet, or at the Middle

of

Fig. 172.

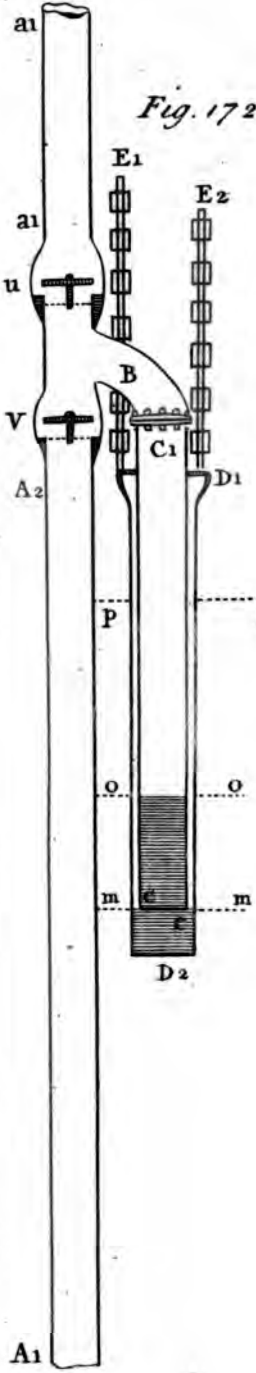
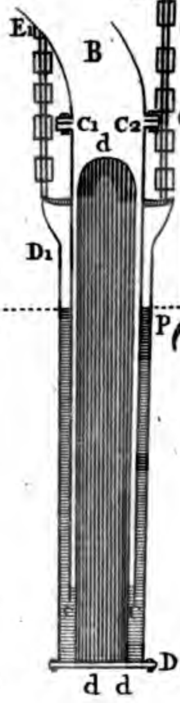


Fig. 173.



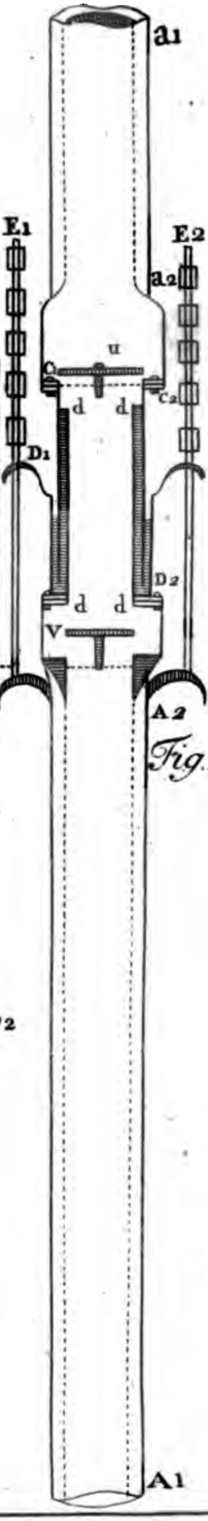


Fig. 174

of the Way from the Water to be raiſed, to the Delivery at Top.

Fig. 177. The 177th Figure drawn by a larger Scale, repreſents the three Cylinders, which are here made of Copper in their juſt Proportions: And for the Sake of thoſe that would conſider this Matter fully, I have here given their Lengths, Diameters within and without, and Thickneſs.

	Outer Cylinder or Barrel, D1 D2.	Middle or hanging Cylinder, in which the Stroke is made C1 C2 *cc*.	Inner Cylinder or Plug cloſed at top by a Cap, and moving up and down with the Barrel to which it is joined at bottom. *d d d d.*
	Inches	*Inches*	*Inches*
Length	30	29,0	31,2
Diameter within	6,74	6,35	6,03
Thickneſs	0,10	0,08	0,13
Diameter without	6,94	6,51	6,29

Here B B repreſents Part of the Elbow of Fig. 175. or of the forcing Pipe of Fig. 174. But as the Spaces between the Cylinders are ſo ſmall, as not to be viſible even in a large Draught made by a Scale; I have here given three more Draughts of the three Cylinders, where the Height is agreeable to the Scale of the 177th Figure, but the Diameters of the middle and inner Cylinders are made leſs than they are in the Engine, to make the Space between (where the Mercury riſes and falls) viſible; and the Cylinders themſelves are repreſented by ſingle Lines.

The Quantity of Mercury uſed in this Engine is 36 ½ Pounds, which being poured in between the outer and inner Cylinder riſes to the Height of 16 Inches.

Fig. 179. When the Barrel is pulled up ſo as to have the middle Cylinder within an Inch of the Bottom of the Barrel; the Mercury on both Sides the middle Cylinder will riſe up to the Height of 23,1 Inches, that is, about two Inches below the Cup D 1, to the Line *q q*. When the Barrel is going down to fill the ſucking Pipe and middle Cylinder C, the Mercury in the inner Shell will be 25 Inches high, and only 13 in the outer Shell, Fig. 179, where the ſhaded Part repreſents the ☿.

Fig. 178. At the End of the ſucking Stroke the Mercury is up to the Top of the inner Cylinder, and ſcarce an Inch in the outer Shell.

In raiſing the Piſton from forcing to the Sucking firſt 1 ¼ Inch drives the Mercury out of the inner Shell, and raiſes it in the outer Shell 13, 28 Inches.

The Depth of an Inch of Water in the middle Cylinder above the inner one or Plug is equal to a Space in the outer Shell of 13,28 Inches, and $\frac{1}{4}$ of an Inch is equal to the ſame Height in the inner Shell.

Therefore when the Mercury is equally high in both Shells, a Motion of $\frac{1}{4}$ of an Inch of the Barrel will charge for Suction. That is, upon letting down the Barrel only $\frac{1}{4}$ of an Inch, the Preſſure of the Atmoſphere in the outer Shell will raiſe the Mercury in the inner one 13,28 Inches, at the ſame time, that it puſhes up the Water from the Well 13 Foot and a half high into the ſucking Pipe. And when all the Pipes are full, if the Mercury be equally high in both Shells, upon raiſing the Barrel one Inch, the Mercury will riſe 13,28 Inches in the outer Shell; which I call charging for forcing; becauſe in continuing to raiſe the Barrel, the forcing Valve immediately riſes, and the Water comes out at Top during the reſt of the Stroke, which is 12 Inches, and delivers 1,6 Gallon of Water, Wine Meaſure.

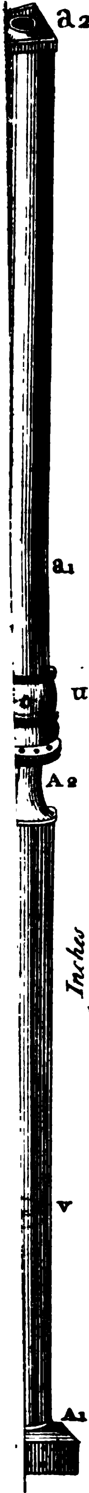
Fig. 180. Fig. 180 repreſents the forcing Stroke half way up; with the ☿ 17 Inches in the outer Shell, 4 Inches in the inner, and the whole Space at Bottom under the middle Cylinder 7 Inches.

From this it appears, that in the whole Stroke of 13 Inches in Length, there is only $\frac{1}{4}$ of an Inch loſt to charge for Suction, and in the next Stroke, which is likewiſe of 13 Inches there is only one Inch loſt to charge for forcing; ſo that in a Motion of 26 Inches, there is but 1 $\frac{1}{4}$ Inch, or about $\frac{1}{20}$ part ineffectual. But this is owing to the too large Space of the outer Shell, which contains 4 Times more than the inner one, becauſe the Cylinders were only hammered, and not turned; for if the outer Space had been no bigger than the inner, then $\frac{1}{4}$ of an Inch of the Stroke would have charged for forcing; ſo that only $\frac{1}{2}$ an Inch in 26, or $\frac{1}{52}$ Part of the whole Stroke would have been ineffectual; and in that Caſe, $\frac{2}{5}$ of the Quantity of Mercury, or a little more than 12 Pounds, would have been ſufficient.

There may ſtill leſs Mercury be uſed, if the middle Cylinder be made of Plate Iron turned on the Outſide, and bored within, the outer Cylinder bored, and the inner one turned; ſo that if the Work be well performed, eight or ten Pounds of Mercury will be ſufficient in this Engine, tho' the Bore of the middle Cylinder, or Diameter of the Pillar of Water which is raiſed, be of 6,35 Inches. If the Bore of the ſaid Cylinder was but 3 Inches, leſs than 3 Pounds of Mercury would ſuffice, and leſs than ſix if there were two Barrels, in order to keep a conſtant Stream thro' a Pipe of almoſt the ſame Diameter. This will very much leſſen the Expence of Mercury, which would otherwiſe be an Objection againſt this Engine; and by making the inner and outer Cylinder of hard Wood, as *Box*, or *Lignum Vitæ*, the Coſt of the Engine may ſtill be reduced. But if the Engine be very large, Caſt Iron bor'd will be proper for the outer Cylinder, and Caſt Iron turn'd on the Outſide for the inner Cylinder or Plug, and hammered Iron bored and turned for the middle Cylinder.

	Inner Cylinder			2^{d} or Mid Cylinder		
	feet	Inch.	tenths	f	i	t
Length	2	7	2	2	5	0
Diameter within	0	6	03		6	35
Thickneſs		0	13		0	08

Distance between outer & Inner Cylinder 0 225



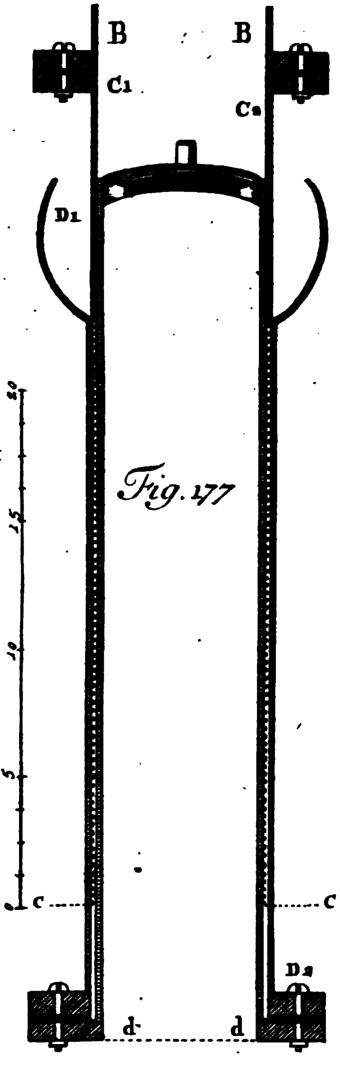


Fig. 177

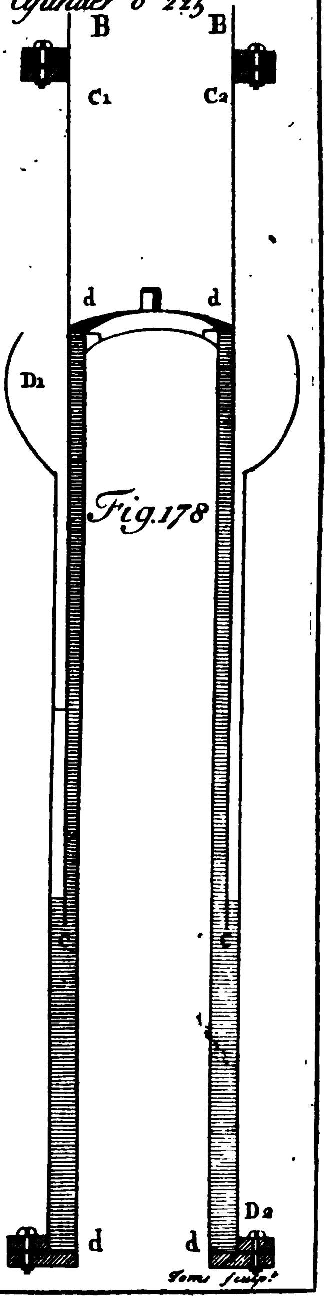


Fig. 178

Toms Sculp.

There is an Objection, which ſeems at firſt to take off the intended Advantage of this Engine, That inſtead of the Friction of the Leather of a Piſton, when we lift up our Barrel to force, the Reſiſtance, that the Mercury finds to riſe in the outer Shell, is at leaſt as great as the Friction that we avoid. Now that Reſiſtance is never greater than the Weight of a concave Cylinder of Mercury, whoſe Height is the greateſt to which the Mercury riſes in the ſaid Shell, and the Baſe is the Area of the Shell itſelf. This Weight in our Engine is equal to 57,5 Pounds, and therefore one would think it greater than the Reſiſtance made by the Friction of a Piſton. But if it be conſidered, that in the Deſcent of the Barrel for ſucking, the Mercury ſhifts immediately into the inner Shell, riſing to the ſame Height, and ſtill keeping the ſame Baſe; the aforeſaid Weight of 57,5 Pounds helps down the Barrel, and facilitates the overcoming of the Force of the Atmoſphere, conſequently the Weight of the Mercury being balanced, is no Hindrance, whether you work with a ſingle or a double Barrel.

There remains only then the Hindrance by loſs of Time in the Beginning of any Stroke: But I have ſhewed that to be but $_5\frac{1}{2}$ Part of the Stroke. I have found that the beſt Engines now in uſe generally loſe near $\frac{1}{5}$ of the Water that they ought to give, according to their Number of Strokes. And Mr. *Beighton*, having a great many times meaſured the Water that is rais'd by Engines in Mines, found that ſome Engines loſt $\frac{1}{4}$, and none ever loſt leſs than $\frac{1}{5}$ of what they ought to give according to the Number of the Strokes in their Pumps, whatever auxiliary Powers they were moved with.

There is indeed another Objection, but ſcarce worth Notice; which is, that ſome Particles of Mercury will mix with the Water that is raiſed, and make it unwholeſome; but no body that conſiders ſpecifick Gravity, will imagine any ſuch thing. However, to ſatisfy thoſe that might ſtill apprehend it, it is to be obſerved, that none of the Water that is raiſed comes near the Mercury: For in the Cylinder C, and Part of the Elbow B, (Fig. 175.) there is always above the Mercury a certain Quantity of Water that riſes and falls with the Barrel, and never goes into the forcing Pipe. The ſame happens alſo in the Machine of Fig. 176. for the Water having once run into the Cylinder C, all that is raiſed afterwards, comes thro' the forcing Valve without coming down to the Mercury.

Provided Care be taken to make the Barrel with its Plug tight, I don't ſee that this Machine will want Repair in a long time, except ſome of the auxiliary Powers be out of Order, which do not relate to this Invention. The Numbers given will ſerve to examine the Truth of what I have aſſerted concerning the Motion of the Mercury; and from them one may make Tables to ſerve to proportion theſe Engines for raiſing any Quantity of Water to any Height, according to the Power one has to apply.

VIII. The

A Deſcription of the Water-Works *at* London-Bridge. *By* H. Beighton, *F. R. S.* n. 417. p. 5. Fig. 181.

VIII. The Wheels are placed under the Arches of *London-Bridge*, and moved by the common Stream of the Tide-Water of the River *Thames*.

A B the Axle-tree of the Water-Wheel, 19 Feet long, 3 Feet Diameter, in which C, D, E, F, are four Sets of Arms, eight in each Place, on which are fixed GGGG, four Rings, or Sets of Felloes, in Diameter 20 Feet, and the Floats HHH, 14 Feet long and 18 Inches deep, being about 26 in Number.

The Wheel lies with its two Gudgeons, or Centers, A B, upon two Braſſes in the Pieces M N, which are two great Levers, whoſe Fulcrum, or Prop, is an arched Piece of Timber L, the Levers being made circular on their lower Sides to an Arch of the Radius M O, and kept in their Places by two arching Studs fixed in the Stock L, through two Mortiſes in the Lever M N.

The Wheel is, by theſe Levers, made to riſe and fall with the Tide, which is performed in this Manner. The Levers MN are 16 Feet long; from M, the Fulcrum of the Lever, to O the Gudgeon of the Water-Wheel, 6 Feet; and from O to the Arch at N, 10 Feet. To the Bottom of the Arch N is fixed a ſtrong triple Chain P, made after the Faſhion of a Watch-Chain, but the Links arched to a Circle of one Foot Diameter, having Notches, or Teeth, to take hold of the Leaves of a Pinion of Caſt Iron Q, 10 Inches Diameter, with eight Teeth in it moving on an Axis. The other looſe End of this Chain has a large Weight hanging at it, to help to counterpoiſe the Wheel, and preſerve the Chain from ſliding on the Pinion. On the ſame Axis is fixed a Cog-Wheel R, 6 Feet Diameter, with 48 Cogs. To this is applied a Trundle, or Pinion, S, of ſix Rounds, or Teeth; and upon the ſame Axis is fixed T, a Cog-Wheel of 51 Cogs, into which the Trundle V, of ſix Rounds, works; on whoſe Axis is a Winch, or Windlaſs, W, by which one Man, with the two Windlaſſes, raiſes or lets down the Wheel as there is Occaſion.

And becauſe the Fulcra of theſe Levers, M N, are in the Axis of the Trundle K, *viz.* at M or X, in what Situation ſoever the Wheel is raiſed or let down, the Cog-Wheel II, is always equidiſtant from M, and works or geers truly.

By Means of this Machine the Strength of an ordinary Man will raiſe about fifty Ton Weight.

I, I, is a Cog-Wheel fixed near the End of the great Axis, 8 Feet Diameter, and 44 Cogs working into a Trundle K, of $4\frac{1}{2}$ Foot Diameter, and 20 Rounds, whoſe Axis or Spindle is of Caſt Iron 4 Inches in Diameter, lying in Braſſes at each End, as at X.

Z Z is a quadruple Crank of Caſt Iron, the Metal being 6 Inches ſquare, each of the Necks being turned one Foot from the Centre, which is fixed in Braſſes at each End in two Head-ſtocks faſtened down by Caps. One End of this Crank at Y is placed cloſe abutting to the End of

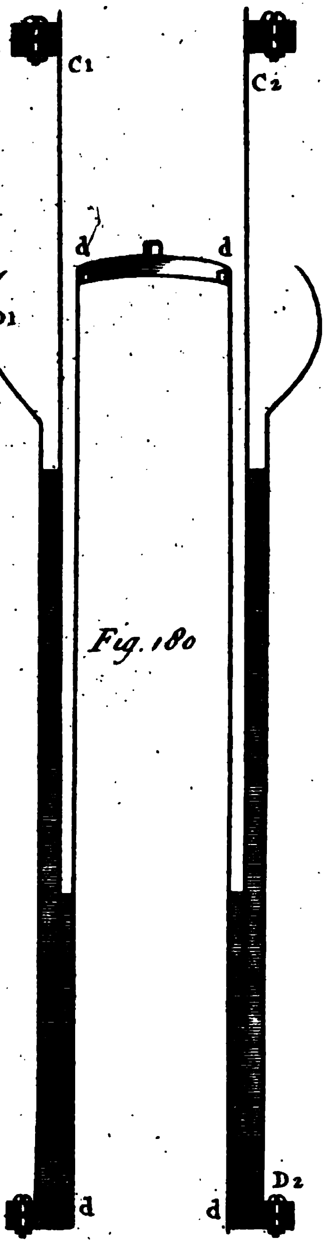


Fig. 180

of the Axle-tree X, where they are at thoſe Ends ſix Inches Diameter, each having a Slit in the Ends, where an Iron Wedge is put, one half into the End X, the other half into Y, by Means of which the Axis X turns about the Crank Z Z.

The four Necks of the Crank have each an Iron Spear, or Rod, fixed at their upper Ends to the reſpective Libra, or Lever, *a* 1, 2, 3, 4, within three Foot of the End. Theſe Levers are 24 Feet long, moving on Centers in the Frame *b b b b*; at the End of which, at *c* 1, 2, 3, 4, are jointed four Rods with their forcing Plugs working into *d* 1, 2, 3, 4, four Caſt Iron Cylinders four Feet three Quarters long, ſeven Inches Bore above, and nine below where the Valves lie, faſtened by ſcrewed Flanches, over the four Holes of a hollow Trunk of Caſt Iron, having four Valves in it juſt over *e e e e*, at the joining on of the Bottom of the Barrels, or Cylinders, and at one End a ſucking Pipe and Grate *f*, going into the Water, which ſupplies all the four Cylinders alternately.

From the lower Part of the Cylinders *d* 1, *d* 2, *d* 3, *d* 4, come out Necks turning upward Arch-wiſe, as *g g g g*, whoſe upper Parts are caſt with Flanches to ſcrew up to the Trunk *b b b b*; which Necks have Bores of 7 Inches Diameter, and Holes in the Trunk above communicating with them, at which Joining are placed four Valves. The Trunk is caſt with four Boſſes, or Protuberances, ſtanding out againſt the Valves to give room for their opening and ſhutting; and on the upper Side are four Holes ſtopped with Plugs, to take out on Occaſion, to cleanſe the Valves. One End of this Trunk is ſtopped by a Plug *i*. To the other, Iron Pipes are joined, as *i* 2, by Flanches, through which the Water is forced up to any Height or Place required.

Beſides theſe four Forcers, there are four more placed at the other Ends of the Libræ, or Levers (not ſhewn here to avoid Confuſion, but to be ſeen on the left Hand) the Rods being fixed at *a* 1, 2, 3, 4, working in four ſuch Cylinders, with their Parts *d d*, *&c.* *e e*, *f*, *g g*, and *i*, as before deſcribed, ſtanding near *k k*.

At the other End of the Wheel (at B) is placed all the ſame Sort of Work as at the End A is deſcribed, *viz.*

The Cog-Wheel	I.	The four Levers	*a c*, *a c*, *&c.*
The Trundle	K.	8 forcing Rods	*a d*, *a d*, *&c.*
The Spindle	X.	8 Cylinders,	*d e*, *d e*, *&c.*
The Crank	Y, Z.	4 Trunks, ſuch as	*e e*, *b b*.
The ſucking Pipes	*f*.	2 forcing Pipes, as	*i*.

So that one ſingle Wheel works 16 Pumps.

All which Work could not be drawn in one perſpective View, without making it very much confuſed.

In the 1ſt Arch next the City is one Wheel with double Work of } 16 Forcers.

A Calculation of the Quantity of Water raiſed by the Engines at London-Bridge.

In the 3d Arch	1ſt Wheel double Work at one End, and ſingle at the other	12	
	2d Wheel in the Middle	8	
	3d Wheel	16	
	In all	52 Forcers.	
One Revolution of a Wheel makes in every Forcer		2⅕ Strokes.	
So that one Turn of the 4 Wheels makes		114 Strokes.	
When the River is at beſt, the Wheels go ſix times round in a Minute, and but 4½ at middle Water		6	
The Number of Strokes in a Minute		684	
The Stroke is 2½ Feet, in a 7 Inch Bore, raiſes		3	Ale Gall.
They raiſe *per* Minute		2052	

That is, 123120 Gallons = 1954 Hogſheads *per* Hour, and at the Rate of 46896 Hogſheads in a Day, to the Height of 120 Feet.

This is the utmoſt Quantity they can raiſe, ſuppoſing there were no Imperfections or Loſs at all.

But it is certain from the Conſiderations following, that no Engine can raiſe ſo much as will anſwer the Quantity of Water the Cylinder contains in the Length of the Forcer, or Piſton's Motion: For,

Firſt, The opening and ſhutting of the Valves loſe nearly ſo much of that Column, as the Height they riſe and fall.

Secondly, No Leather is ſtrong enough for the Piſton, but there muſt continually ſlip or ſqueeze by ſome Water, when it is raiſed to a great Height; and when the Column is ſhort, it will not preſs the Leather enough to the Cylinder, or Barrel: But eſpecially at the Beginning, or firſt moving of the Piſton, there is ſo little Weight on it, that before the Leather can expand, there is ſome Loſs.

Thirdly, And this Loſs is more or leſs, as the Piſtons are looſer or ſtraighter leathered.

Fourthly, When the Leathers grow too ſoft, they are not capable of ſuſtaining the Pillar to be raiſed.

Fifthly, If they are leathered very tight, as to loſe no Water, then a great Part of the Engine's Force is deſtroyed by the Friction.

By ſome Experiments I have accurately made, on Engines, whoſe Parts are large and excellently performed, they will loſe ⅕ and ſometimes ¼ of the calculated Quantity.

However, the Perfections or Errors of Engines are to be compared together, by the calculated Quantities or Forces; for as they differ in thoſe, they will proportionably differ in their actual Performances.

The Power by which the Wheels are moved.

The Weight of the Pillar of Water on a Forcer 7 Inches Diameter, and 120 Foot high.

7 × 7

7 × 7 = 49 ℔ The Pounds *Averdupoiſe* in
40 Yards high. [a Yard nearly.
1960 ℔ on one Forcer.
8 Forcers always lifting.
The whole Weight 15680 ℔ = 140 *Ct.* = 7 Tun Weight on the Engine at once.

Then the Crank pulls the *Libra* 3 Feet from the Forcer, and 8,3 Feet from the Center,

7 Tun
× 11.3
8,3)79.1 (9,5 Tun on the Crank.
Tun.
Wallower 2,2)9,5(4,3 on Trundle.
The Spur Wheel 4
The Radius of the great Wheel 10) 17,2 (1,72 Tun.
20
The Force on the Floats 18 *Ct.* 40 ℔ 34,40 *Ct.*

But to allow for Friction and Velocity, may be reckoned 1 Tun ½.

The Ladles or Paddles 14 Foot long, 18 Inches deep, — } = 22,4 ſquare Feet.
The Fall of Water is ſometimes 2 Feet.
44,8
6 Gall. in a Cub. Ft.
)268,8
10, ℔ in a Gallon.
112)2688.(24 Hundred.

The Velocity of the Water, 4 Feet in 21‴ of Time.
21‴ — 4 Ft. : : — 60′ : = 685 Feet *per* Minute.

The Velocity of the Wheel = 310 Feet *per* Minute.

Quantity expended on the Wheel, according to the Velocity of the Stream 1433 Hogſheads *per* Second.

But at the Velocity of the Wheel 645 Hogſheads *per* Second.

The Velocity of the Wheel to the Velocity of the Water, as 1 to 22.

Some Obſervations on theſe Water-Works.

Although they may juſtly be eſteemed as good as any in *Europe*, yet are there, as I conceive, ſome Things which might be altered very much for the better.

Firſt, if inſtead of ſixteen Forcers they worked only eight, the Stroke might be five Feet in each Forcer, which would draw a great deal more Water with the ſame Power on the Wheel; for then there would be but half the opening and ſhutting of Valves, conſequently but half that Loſs: And a five Foot Stroke draws above double the Quantity of two Strokes of 2 ½ each, by near ⅕, in regard the Velocity is double, which is the moſt valuable Conſideration in an Engine, where the Pipes will ſuſtain ſuch Force.

Secondly, the Bores that carry off the Water from the Forcers are too ſmall, there being (nearly) always two Pillars of 7 Inches Diameter, forcing into one Pipe of the ſame Diameter, and $7 \times 7 = 49 + 49 = 98$.

Therefore thoſe Pipes of Conveyance ſhould be near nine Inches Diameter.

The Perfections of the Machine. The Timber-work is all admirably well performed, and the Compoſition and Contrivance, for Strength and Uſefulneſs, not exceeded by any I have ſeen.

The caſt Iron Cranks are better than wrought ones, by reaſon they are very ſtiff, and will not be ſtrained, but ſooner break; but then they are cheap, and new ones eaſily put in.

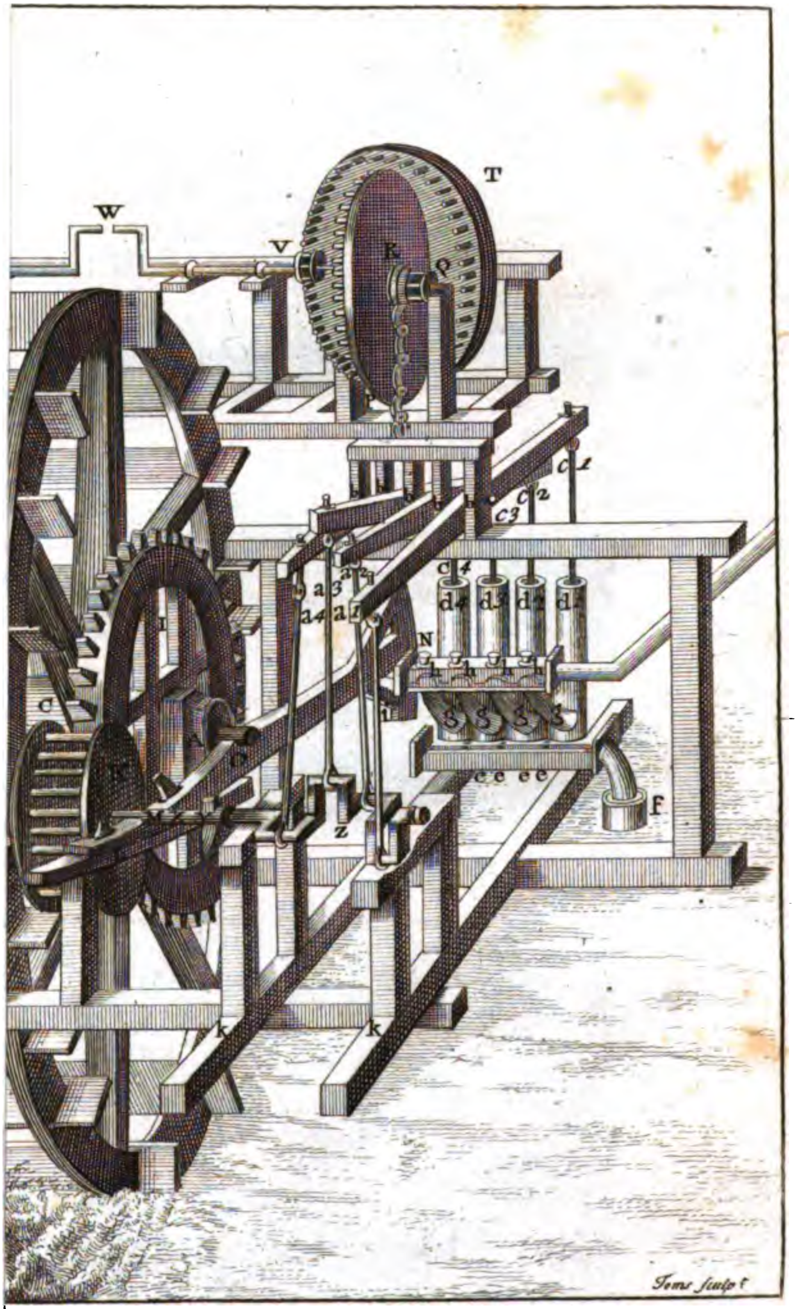
The Wedge for putting on or releaſing the Crank and Forcers, is better than the ſliding Sockets commonly uſed.

The forcing Barrels, Trunks, and all their Apparatus, are very curiouſly contrived for putting together, mending, altering or cleanſing, and ſubject to as little Friction as poſſible in that Part.

The Machine for raiſing and falling the Wheels is very good, tho' but ſeldom uſed, as they tell me; for they will go at almoſt any Depth of Water, and as the Tide turns, the Wheels go the ſame Way with it.

Theſe Machines at *London-Bridge* are far ſuperior to thoſe ſo much famed at *Marly* in *France*, in regard the latter are very ill deſigned in their Cranks, and ſome other Parts.

CHAP.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CHAP. VI.

GEOGRAPHY, NAVIGATION.

A Diſſertation concerning the Figure of the Earth, by the Rev. J. T. Deſaguliers, *LL. D. F. R. S.* n. 386. p. 201.

I. 1. THAT the Earth is of a ſpherical Figure, or nearly ſuch, has been proved ſo often, and by ſo many unanſwerable Arguments, that to repeat them here would be tedious. But, as a little Variation from a true Sphere (beſides the Irregularity of high Hills and deep Vallies) does not hinder us from calling the Earth a Globe; ſo, to determine what that Variation may be, ſince modern Philoſophers are divided about it, may be a Subject not ungrateful at this Time.

Monſieur *Caſſini* ſays: "That the Earth is an oblong Spheroid, " higher at the Poles than the Æquator, making the Axis longer than " a Diameter of the Æquator about thirteen *French* Leagues, which " he deduces from comparing his Father's Meaſures of the Meridian, " from *Paris* to the *Pyrenæan* Mountains, with thoſe of Monſ. *Picard*; of which an Account may be ſeen in the Memoirs of the " Royal Academy for 1713. But having afterwards continued the " Meridian, which is drawn through *France*, from *Paris* to *Dunkerque*, " he ſtill draws Conſequences to prove the Earth an oblong Spheroid; but then makes the Axis exceed the Æquatorial Diameter " 34 Leagues.

" Sir *Iſaac Newton* makes the Earth higher at the Æquator, and, " conſequently, flatted towards the Poles, reckoning its Æquatorial " Diameter 34 *Engliſh* Miles longer than the Axis; which he proves " from the Principles of Gravity, and the Centrifugal Force that a- " riſes from the Diurnal Rotation of the Earth; and, to confirm this, " mentions [illegible]eral Experiments on Pendulums, which have been made " ſhorter, to ſwing Seconds, near the Æquator, than in greater La- " titudes."

Theſe are the two Opinions which have divided Philoſophers, and which we propoſe to examine here.

M. *Caſſini*, taking the Meaſures above-mentioned to be exact enough, not only to determine the Magnitude of a Degree of the Earth, correſponding with a Degree of the great Circle of the Heavens, but alſo to ſhew the Difference in the Degrees of the Earth; (reckoning thoſe, that were meaſured in the South of *France*, to exceed thoſe towards the North, by a certain Number of Toiſes and Feet) demonſtrates, that if the Degrees of the Earth are longer towards the Æquator than the Poles, the Plane of the Meridian muſt be an Ellipſe, whoſe long Axis is that of the Earth. Here follows his firſt Demonſtration. [*See the* French *Memoirs for the Year* 1713.]

" Let

Fig. 182. “ Let BDCR be an Ellipſe that repreſents a Meridian of the Earth, whoſe Poles B and C are at Ends of the great Axis BC, and whoſe Foci E and F are taken at Pleaſure. *Now, to divide this Ellipſe into Degrees, that is, to find ſeveral Points* H, I, V, *ſuch, that the Diſtance, from the Pole to the Zenith, of every one of them, ſhall be of any given Number of Degrees.*

Demonſtration. “ From E, one of the Foci of the Ellipſe, draw the Line ET, ſo that it may, with the Axis BC, make the Angle BET equal to the Diſtance given from the Pole to the Zenith. From the other Focus F, with the Diſtance BC equal to the Axis, draw an Arc, to cut the Line ET at T. I ſay, that the Line FT, drawn from the Point T to the Focus F, will cut the Ellipſe at the Point H; which Point is ſuch, that the Diſtance of the Pole, from its Zenith, contains the given Number of Degrees.

“ From the Point H, raiſe H*Z*, perpendicular to the Ellipſe, which will paſs through the Zenith Z; and, being produced inwards, will meet the Axis of the Earth at O, and (by the Property of the Ellipſe) divide the Angle EHF into two equal Parts. From the Point H, draw likewiſe HP, parallel to the Axis BC, and directed to the Pole P, ſuppoſed at an infinite Diſtance. The Angle PHZ, or POZ, meaſures the Diſtance, from the Pole to the Zenith, of an Inhabitant dwelling upon the Earth at the Point H. FT is equal to the Axis BC, by Conſtruction; but, by the Property of the Ellipſe, BC is equal to EH *plus* HF; taking away from both FH, which is common, EH will remain equal to HT. The Angles ETH, TEH, will therefore be equal, and, conſequently, each of them will be half of the external Angle EHF; but the Angle EHO is likewiſe equal to half of the Angle EHF; therefore the Angles TEH, EHO, will be equal to one another; and, conſequently, the Lines E T and H O will be parallel to one another; and the Angle POZ, which meaſures the Diſtance from the Pole to the Zenith of the Point H, will be equal to the Angle BET, which was, by Conſtruction, taken equal to the given Diſtance of the Pole from the Zenith; *which was to be demonſtrated.*

“ Now, if the Proportion of the longeſt Diameter of the Ellipſe BC to EF, the Diſtance of the Foci, be taken at Pleaſure, one may by Calculation find all the Points of the Ellipſe as H, to determine the Degrees by this Analogy.

“ *As FT or BC:*

“ *Is to E F::*

“ *So is the Sine of the Angle PET (the given Diſtance from the Pole to the Zenith):*

“ *To the Sine of the Angle ETF, or TEH:* Whoſe Quantity will conſequently be known. This Angle TEH being added to the

" Angle PET, the given Diſtance from the Pole to the Zenith of the " Point H, will give the Quantity of the Angle BEH, which a Line " drawn from the Focus to H, the Point required, makes with the " Axis of the Ellipſe.

" Then in the Triangle EHF, whoſe Side EF is known, as well " as the Angle EHF, which is the Double of the Angle TEH, and " the Angle FEH Supplement of the Angle BEH; one ſhall have " the Length of the Side EH, known in Parts of the Axis BC.

" After the ſame Manner, may be found the Angles BEI, BEV, " *&c.* and the Length of the Lines EI, EV, to determine the Diſtance, from the Pole to the Zenith, of all the Degrees of the " Circumference of the Earth; and in the rectilinear Triangles HEI, " IEV, whoſe Sides HE, EI, EV, are known as well as the Angles comprehended between the Sides HE, EI, IE, EV, which " are the Differences of the Angles BEH, BEI, BEV, determined " above; one ſhall find the Length of the Chords HI, IV, comprehended between each Degree".

Monſ. *Caſſini*, in the Memoirs for the Year 1718, repeats the ſame Demonſtration; except that, before it, he ſhews, that if ſeveral Points be taken upon a Terreſtrial Meridian, on the Surface of an Elliptick Earth, as G, H, I, K, in ſuch Manner, that their reſpective Zeniths Z, L, M, N, are diſtant from one another, an equal Number of Degrees meaſured in a Celeſtial Meridian; the Lines Z G, L H, M I, N K (which are perpendicular to the Ellipſe) being produced, will meet in the Points O, R, and S, making equal Angles; but as thoſe angular Points are not equally diſtant from the Curve of the Ellipſe, that Elliptic Arc muſt be the longeſt whoſe angular Point is fartheſt off. Now, by the former Demonſtration, it appears, that thoſe Arcs, which are taken neareſt to the leſſer Axis, will have their angular Points farther removed, *&c.* Fig. 183.

If M. *Caſſini*'s Meaſures of Terreſtrial Degrees, decreaſing from the Æquator towards the Pole, were grounded on Obſervations liable to no Error, he would have fully proved his Figure of the Earth. But ſince thoſe Meaſures (however accurately taken) are not built upon a mathematical Certainty, his Premiſes may be called in Queſtion, and his Concluſion, tho' mathematically drawn from theſe Premiſes, is only probable.

Now therefore, if I can ſhew from undoubted *Phænomena*, that his Concluſion will lead to an Abſurdity, his Meaſures muſt be falſe; becauſe his Reaſoning from them is juſt. This I ſhall endeavour to do firſt, which will diſprove his Figure of the Earth; and afterwards endeavour to point out ſome of the Errors which I ſuppoſe to have occaſioned the Miſtake in the Meaſures.

M. *Caſſini*, as well as the *Engliſh* Aſtronomers, believes that the Earth makes one Revolution about its Axis, once in 23 Hours 56', becauſe in that Time, the Plane of any Meridian returns to the ſame fixed Star from which it had departed.

Let H be taken in any Parallel of Latitude, as for Example, in the Latitude of 51° 46′, a Plumb Line, L H, will be perpendicular to the Curve BH, at H, and produced paſs thro' the Zenith of the Point H, if the Earth had no Diurnal Rotation; but ſince the Earth moves round its Axis, all Bodies upon its Surface, endeavour to fly from the Axis of their Motion with a Force proportionable to their Diſtance from it in a Direction along the Plane of that Parallel, in which they are. Let that Force (explained by M. *Huygens*, and called a Centrifugal Force) be repreſented by the Line H *l*, or its Equal and Parallel *L b*; now a Plummet placed at L, if the Earth ſtood ſtill, would deſcend in the Line L H, but as it is at the ſame Time acted upon by the Force H *l* in the Direction L *b*, it will move in the Direction *L l* Diagonal of the Parallelogram H *l*, according to the known Laws of Mechanicks; and the Plumb Line L H, inſtead of being perpendicular to the Curve at H, will in the Latitude 51° 46′ make an Angle of 5′ with HL. This Angle will be leſs towards the Poles, till at the very Pole it quite vaniſhes, as it alſo does at the Æquator. Now ſince there is no ſuch Angle obſerved, but in all Water Levels we find the Plumb Line always perpendicular to the Line of Level, the Surface of the Earth muſt be depreſſed towards G, and riſe farther from the Axis towards I, in order to become perpendicular (that is, to have its Tangent perpendicular) to the Line L *l*, in which we have ſhewn that the Plumb Line muſt deſcend.

If there is any body ſo fond of M. *Caſſini*'s Hypotheſis, as to deny the Diurnal Motion of the Earth for the Sake of it, I hope they will be convinced, when I ſhew the Meaſures, upon which it is founded, to be inſufficient for determining the different Lengths of the Degrees of a Terreſtrial Meridian.

But here I would not be thought to endeavour to leſſen the Praiſe due to the Gentlemen of the *Royal Academy*, for carrying on a Meridian the whole Length of *France*, from *Dunkerque* thro' the Royal Obſervatory at *Paris*, quite to the *Pyrenæan* Mountains on the Borders of *Spain*. Aſtronomy and Geography are doubtleſs much indebted to the Encouragement given by the *French* Government, and to the Care of their Mathematicians, who have omitted no proper Method for drawing their Meridian, and correcting it as they went on. So many Obſervations of the riſing and ſetting Sun, ſo many equal Altitudes of the ſame Stars accurately taken, ſo many Digreſſions of Stars, ſo many other Obſervations made with the Teleſcope and good Pendulum-Clocks—all compared together, for the true ſettling of the direct Way of this famous Meridian, leave no Doubt but that it is as perfect as the Nature of the Thing is capable of. And, certainly, by the Help of this Line, and the ſeveral Triangles made uſe of for carrying it on, a better Map of *France* is made, than has ever been of any Country before: Nay, beſides, I believe we may, at a Medium, very well receive their Number of 57060, or 57061 Toiſes, for the Meaſure of

a Degree of a Meridian of the Earth, one with another. But to ſay, that thoſe Gentlemen could obſerve the Latitude ſo nicely, as to find a Difference in the Length of the Terreſtrial Degrees, and that only of eleven or twelve Toiſes (when they made it the leaſt) or of thirty one Toiſes (when they made it the moſt) is attributing to them an Exactneſs, ſo far beyond the Nature of the Inſtruments which they made uſe of, that it would be rather a Diſpraiſe than a Commendation to inſiſt upon it.

For in the firſt Place, the Inſtrument, with which they took Obſervations for the Latitude at the two Ends of their Meridian, was a ten Foot Sector (which was worſe than that which M. *Picard* had made Uſe of before, becauſe the Teleſcope of his Sector was of ten Foot, whereas M. *Caſſini*'s was but of three Foot, tho' applied to the ten Foot Sector) where the two hundredth Part of an Inch anſwers to eight Seconds of a Degree: Now the two hundredth Part of an Inch, being one of the leaſt viſible Parts that we can ſee in a divided Line; they could not take an Angle nearer than that; nay, their Inſtrument, according to their own Deſcription of it, was divided but to every twenty Seconds. Now they allow, that ſixteen Toiſes, upon the Surface of the Earth, anſwer to one Second in the Heavens; and they don't pretend to have taken an Obſervation nearer than to about three Seconds, which therefore cannot determine a Difference leſs than forty eight Toiſes; whereas the Degrees are only ſuppoſed to decreaſe at moſt, thirty one Toiſes each, from *Collioure* to *Dunkerque*. But an Error of eight Seconds would make a Difference of one hundred and twenty eight Toiſes, on the Surface of the Earth; above ten times greater than the Difference of Degrees in the firſt Suppoſition, and four Times greater than that Difference in the laſt. Beſides, the Latitude was not obſerved in the intermediate Places between *Paris* and *Collioure*, with the abovementioned Inſtrument of ten Foot Radius; but they made uſe of a Quadrant, whoſe Radius was only thirty nine Inches, and ſometimes an Octant of three Foot Radius. Nay, they ſay themſelves, in their Account, that it is not the Obſervations made at the Ends of the Meridian, that we are to deduce the Difference of the Length of a Degree from, but the Altitudes taken at ſeveral Places between the Extreams; and, if we grant, that they can take an Angle very well, to four or five Seconds, with the great Inſtrument, they cannot come nearer than twelve or fifteen Seconds, with the Quadrant or Octant, which we muſt depend upon for the Difference of the Meaſure of Degrees: So that upon the whole, we are to determine a Length of thirty one Toiſes, by an Inſtrument which is liable to err above two hundred.

If any Conſequences of this Kind could be drawn from actual meaſuring, a Degree of Latitude ſhould be meaſured at the Æquator, and a Degree of Longitude likewiſe meaſured there; and a Degree very northerly, as for Example, a whole Degree might be actually

meaſured upon the *Baltick* Sea, when frozen, in the Latitude of ſixty Degrees.—There, according to M. *Caſſini*'s laſt Suppoſition, a Degree would be of 56653 Toiſes, whereas, at the Æquator, it would be of 58019 Toiſes, the Difference being 1364 Toiſes, about the two and fortieth Part of a Degree, which muſt be ſenſible; and likewiſe the Degree of Longitude would, according to him, be of 56817 Toiſes, leſs by 1202, or the forty eighth Part, than a Degree of Latitude at the ſame Place.

But here it may be objected, that tho' the Latitude was not taken with the ten Foot Sector, in the intermediate Places between *Paris* and *Collioure*, yet the Latitude was taken with that Inſtrument at *Dunkerque*, *Paris*, and *Collioure*, and therefore the ſouthern Part of the Meridian, containing 6° 18′ 56″ may be compared with the northern Part of it, which contains 2° 12′ 16″; and that the former appears to contain more Toiſes, in Proportion to the Difference of Latitude at its Extremities, than the latter. To this may be anſwered, that, even in this Caſe, the Obſervations made cannot be nice enough to determine the Difference of the Length of Degrees; but there is another Error, which might conſiderably miſlead the *French* Gentlemen, and make the Degrees appear longer in the South of *France*; that is, the Error in taking the true Height of ſeveral Mountains in *Auvergne*, *Languedoc*, and among the *Pyræneans*. For if they have allowed too much for the Air's Refraction (which, by the Obſervations of Travellers, is greater towards the northern Regions, and diminiſhes as we go Southward) the Heights of thoſe Mountains will be taken too little, and their Baſes conſequently longer, which will make the Degrees appear bigger than they are. Fig. 184. Let ABCD, for Example, be a Mountain, as the Mountain of *Rodez*, in the Latitude of 44° 21′, whoſe Height BD is 300 Toiſes, and whoſe Sides AB and BC (ſuppoſed to make an Angle of 26° 33′, with the Horizon) are found by Trigonometry, to be of 670,8 Toiſes each; if by a Miſtake, in taking the Height, it be ſuppoſed only equal to ED, or 257 Toiſes, then the Lines AB and BC will become EF and EG; ſo that the Baſe AC, which before was of 1200 Toiſes, will become equal to FG, which will appear to be 1279,6 Toiſes, by *Eucl.* 47. 1. Now one ſuch Miſtake, in one Degree, will give a Difference above twice as great as the ſuppoſed Difference of Degrees in that Latitude, which they make of 31 Toiſes. And that there was a Miſtake of this Kind in taking the Height of that Mountain, I ſhall ſhew.

The Vapours, that generally float in the Air about the Tops of high Hills, make it ſo difficult to take their Height exactly, that Experiments, made with the Barometer, will, by obſerving the Fall of the Mercury, ſhew the Height nearer than any Thing elſe we know of. There were, indeed, ſeveral Experiments made with the Barometer [*], where the Differences of the Height of the Mercury, from the Heights at which it ſtood at the Royal Obſervatory, are ſaid to anſwer to ſo

many Toiſes; but of nine Obſervations mentioned by M. *Caſſini*, there are not two where the Number of Toiſes, ſaid to correſpond to the Heights of the Barometer, do agree together.

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[*] *Mem. of the* Royal Academy, *for* 1718, Chap. 10.

The firſt Experiment of the Barometer there mentioned, made at *Collioure*, was this; ‘ At the Height of $11\frac{1}{2}$ Toiſes above the Sea, ‘ the Barometer was ſet up, and the Mercury ſtood $3\frac{1}{3}$ Lines higher ‘ than at the Royal Obſervatory (in the Tower of the eaſtern Hall) ‘ at the ſame Time; and therefore, ſince that Tower is 44 Toiſes ‘ higher than the Sea, $3\frac{1}{2}$ Lines of Mercury muſt anſwer to $32\frac{1}{2}$ ‘ Toiſes.

Now, reducing theſe Toiſes to Feet, and dividing by $3\frac{1}{3}$ it will appear that a Height of 58,5 Feet will anſwer to the Fall of one Line of Mercury in the Barometer. Let this be taken as the Standard, and the other Obſervations be compared with it. This may be done by the following Table, where the firſt Column ſhews the Place where the Obſervation was made; the ſecond, the Fall or Riſe of Mercury at each Place expreſs'd in Lines, or 12th Parts of a *French* Inch; the third, the Heights or Depths anſwering to thoſe Lines of Mercury, which, in the Memoirs, are given in Toiſes, but here reduced to Feet; the fourth, the Number of Feet anſwering to one Line of Mercury in each Obſervation, which is the Quotient of the Feet in the third Column, divided by the Number of Lines in the Second.

Obſervations of the Barometer made at	*Lines of* Mercury.	*Said to correſpond with Feet.*	*The Fall of one Line of* Mercury *anſwers to Feet.*
I. Collioure	$03\frac{1}{3}$	195	58,5
II. *The Tower of* Maſſane.	31	2382	76,8
III. Bugarac	42	3636	86,5
IV. Rupeyroux	30	2181	72,7
V. Rodez	24	1647	68,6
VI. Rodez	20	1425	71,25
VII. Courlande	54	4812	89,1
VIII. Coſte	54	4890	92,4
IX. Clermont	03	200	66,6

A Sight of this Table will convince any one, that theſe Obſervations are not to be depended upon for determining the Height of the Mountains in the South of *France*; for the Differences are not ſmall, ſuch as might happen in making the Experiments; but ſuch as render the Obſervations uſeleſs for the Purpoſes abovementioned. For Example, the firſt and the ſeventh differ almoſt $\frac{1}{3}$: And if 58,5 Feet were allowed for the Fall of one Line of Mercury in the ſeventh Obſervation, inſtead of 944 Feet, then the Mountain of *Coſte* would be but 3085 Feet, inſtead of 4890. Nay, upon examining the Memoirs, I find that in ſeveral Obſervations the Number of Toiſes, ſaid to correſpond

reſpond to a certain Height of Mercury, are only anſwerable to the Height of the Mountain above the Level of the Sea found by Trigonometry, from which the Height of the Royal Obſervatory, above the Sea, is ſubſtracted; though, by the Manner of the Expreſſion, a curſory Reader would imagine, that the Number of Toiſes named, was always proportionable to the Fall of the Mercury, and think all the Experiments and Obſervations very accurately made, when they ſeem to agree ſo well in every Reſpect.

Now after all, I do not queſtion but that the Height of the Barometer, might be as it is ſet down in the Memoirs, and well enough obſerved; but it was wrong to compare the Height of the Mercury in the South of *France*, with the Height that the Mercury was at in the Barometer of the Royal Obſervatory at the ſame Time; for, at that great Diſtance and Difference of Latitude, the Weather (and conſequently the Preſſure of the Air and Height of the Barometer at the ſame Level) might very much vary.

Even when there is fair Weather all over *France*, it does not follow that the Barometer ſhall ſtand at the ſame Height. Let us ſuppoſe, for Example, that a North Wind blows: Where-ever the Air is checked by a Chain of Mountains that run Eaſt and Weſt, it will be accumulated over thoſe Mountains, and conſequently preſs more as its Columns are higher; which will make the Mercury riſe higher than it would do with the ſame Wind, if there were no Mountains, or if they ran North and South.

The Way, to have made the Experiments with the Barometer exactly, would have been to have obſerved the Height of the Mercury at the Bottom and at the Top of the Mountain, and that with a Tube of a pretty large Bore (with a proportionably large Ciſtern for the ſtagnant Mercury) becauſe, in a ſmall Tube, the Mercury will often ſtick to the Sides, and riſe irregularly, as it will alſo in inclined Barometers. Simple Barometers are the beſt, and a magnifying Glaſs may be made uſe of to obſerve ſmall Riſes or Falls, having two fine and well made Indices to the Tube.

Dr. *Halley*, has given us * the Falls of *Mercury* in the Barometer, correſponding with the Heights to which the Barometer muſt be carried to produce thoſe Falls. The firſt tenth Part of an Inch in the Fall of the Mercury, he makes to anſwer to a Height of 90 Feet; the next tenth, to a Height ſomething greater, and ſo in Proportion, as the Air diminiſhes in Denſity, according as we riſe in Height. The Proportion of the firſt Tenth of the Mercury's Fall, he has built upon the Compariſon of the different Specifick Gravities of Air and Mercury; and taking Mercury to be 13 $\frac{1}{2}$ Times heavier than Water, and Water (in cold Weather) to be 800 Times heavier than Air; it follows, that 13, 5x800, will give 10800; which Number, if it be taken in Feet, and divided by 120 (the Number of the 10th of an Inch in a Foot) we ſhall have 90 Feet anſwerable to the 10th Part of an Inch, and 75 Feet to a Line or the 12th Part of an Inch.

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* *See* Lowthorp's *Abridgment*, Vol. II. Chap. II. Sect. VIII.

Now, as very few Mountains in the World are 3 Miles high, and, generally ſpeaking, thoſe that we look upon as high Hills (except the *Andes*, and ſome others in *America*) are not much above a Mile high; we may, for finding the Height of Mountains, take a fixed Number of Feet in Altitude to anſwer to every 10th or 12th of an Inch in the Fall of the Mercury; becauſe 90 Feet are by Dr. *Halley* only taken for the firſt Tenth, and greater Heights for other Tenths, encreaſing with the Fall of the Mercury. Therefore I would propoſe another for a round Number, namely 96 Feet for every Tenth, and 80 Feet for every 12th of an Inch, very near the Number that I have found by my Calculation, which is as follows.

Fine Mercury (ſuch as is made uſe of in Barometers) is, generally ſpeaking, $13\frac{2}{3}$ Times heavier than Water, and, I found ſome brought from the *Eaſt-Indies*, to be 14 Times heavier. I have found Air in Summer, to be near 900 Times lighter than Water; and 800 Times in Winter; therefore I take 850 at a Medium. Now $850 \times 13\frac{2}{3} =$ 11606,6, which, divided by 120, gives 96,7 Feet, for $\frac{1}{10}$ of an Inch of Mercury, or 80.5 Feet for $\frac{1}{12}$ of an Inch. This Number, taken invariable, will, in taking the Height of ſeveral Hills, agree pretty well with the Numbers that come out, when Dr. *Halley*'s Table is made uſe of; and with the Experiment made by the late Profeſſor, Mr. *J. Caſwell*, who, having taken the Height of *Snowdon* Hill in *Caernarvonſhire* very accurately, and finding it to be 3720 Feet above the Level of the Sea, tried how much lower the Mercury would ſtand in the Barometer upon that Hill, than at the Level of the Sea, and obſerved it to ſubſide 3,9 Inches. I am ſenſible that it will be alledged, that the Air will be denſer than I may imagine on the Top of high Hills, becauſe of the great Cold, ſince they are generally covered with Snow; but then we are to conſider, that when we are got above a Mile higher than the Level of the Sea, the incumbent Atmoſphere has loſt almoſt a fifth Part of its Weight; and therefore the Air at the Top of the Hill, being ſo much leſs preſs'd, will, notwithſtanding the intenſe Cold, be more rarified than at the Bottom of the Hill.

Now if we go back to the Obſervations of the Barometer, made by the Gentlemen that drew the Meridian in *France*, we ſhall find, that on the Mountain of *Rodez*, in the Latitude of 44°, 21′ the Barometer fell 24 Lines below the Level of that in the Obſervatory, and they allowed only $274\frac{1}{2}$ Toiſes to correſpond to that Fall; whereas, according to Dr. *Halley*'s Proportion of a Tenth of an Inch for 90 Feet, they ſhould have taken 300 Toiſes; and tho' the *Hypotenuſes* AB, and BC, were taken longer than the bare Declivity of the Mountain (which would make the Error leſs than the 79 Toiſes I mentioned above) yet if my Proportion be made uſe of, *viz.* of 80 Feet for

CHAP. VI.

GEOGRAPHY, NAVIGATION.

I. 1. THAT the Earth is of a ſpherical Figure, or nearly ſuch, has been proved ſo often, and by ſo many unanſwerable Arguments, that to repeat them here would be tedious. But, as a little Variation from a true Sphere (beſides the Irregularity of high Hills and deep Vallies) does not hinder us from calling the Earth a Globe; ſo, to determine what that Variation may be, ſince modern Philoſophers are divided about it, may be a Subject not ungrateful at this Time.

A Diſſertation concerning the Figure of the Earth, by the Rev. J. T. Deſaguliers, *LL. D. F. R. S.* n. 386. p. 201.

Monſieur *Caſſini* ſays: “ That the Earth is an oblong Spheroid,
“ higher at the Poles than the Æquator, making the Axis longer than
“ a Diameter of the Æquator about thirteen *French* Leagues, which
“ he deduces from comparing his Father's Meaſures of the Meridian,
“ from *Paris* to the *Pyrenæan* Mountains, with thoſe of Monſ. *Picard*;
“ of which an Account may be ſeen in the Memoirs of the
“ Royal Academy for 1713. But having afterwards continued the
“ Meridian, which is drawn through *France*, from *Paris* to *Dunkerque*,
“ he ſtill draws Conſequences to prove the Earth an oblong Spheroid;
“ but then makes the Axis exceed the Æquatorial Diameter
“ 34 Leagues.

“ Sir *Iſaac Newton* makes the Earth higher at the Æquator, and,
“ conſequently, flatted towards the Poles, reckoning its Æquatorial
“ Diameter 34 *Engliſh* Miles longer than the Axis; which he proves
“ from the Principles of Gravity, and the Centrifugal Force that a-
“ riſes from the Diurnal Rotation of the Earth; and, to confirm this,
“ mentions [illegible]eral Experiments on Pendulums, which have been made
“ ſhorter, to ſwing Seconds, near the Æquator, than in greater La-
“ titudes.”

Theſe are the two Opinions which have divided Philoſophers, and which we propoſe to examine here.

M. *Caſſini*, taking the Meaſures above-mentioned to be exact enough, not only to determine the Magnitude of a Degree of the Earth, correſponding with a Degree of the great Circle of the Heavens, but alſo to ſhew the Difference in the Degrees of the Earth; (reckoning thoſe, that were meaſured in the South of *France*, to exceed thoſe towards the North, by a certain Number of Toiſes and Feet) demonſtrates, that if the Degrees of the Earth are longer towards the Æquator than the Poles, the Plane of the Meridian muſt be an Ellipſe, whoſe long Axis is that of the Earth. Here follows his firſt Demonſtration. [*See the* French *Memoirs for the Year* 1713.] “ Let

Fig. 182. “ Let BDCR be an Ellipſe that repreſents a Meridian of the Earth,
“ whoſe Poles B and C are at Ends of the great Axis BC, and whoſe
“ Foci E and F are taken at Pleaſure. *Now, to divide this Ellipſe*
“ *into Degrees, that is, to find ſeveral Points* H, I, V, *ſuch, that the*
“ *Diſtance, from the Pole to the Zenith, of every one of them, ſhall be of*
“ *any given Number of Degrees.*

“ From E, one of the Foci of the Ellipſe, draw the Line ET, ſo
Demonſtration. “ that it may, with the Axis BC, make the Angle BET equal to the
“ Diſtance given from the Pole to the Zenith. From the other Focus
“ F, with the Diſtance BC equal to the Axis, draw an Arc, to cut
“ the Line ET at T. I ſay, that the Line FT, drawn from the
“ Point T to the Focus F, will cut the Ellipſe at the Point H; which
“ Point is ſuch, that the Diſtance of the Pole, from its Zenith, con-
“ tains the given Number of Degrees.

“ From the Point H, raiſe HZ, perpendicular to the Ellipſe,
“ which will paſs through the Zenith Z; and, being produced in-
“ wards, will meet the Axis of the Earth at O, and (by the Proper-
“ ty of the Ellipſe) divide the Angle EHF into two equal Parts.
“ From the Point H, draw likewiſe HP, parallel to the Axis BC,
“ and directed to the Pole P, ſuppoſed at an infinite Diſtance. The
“ Angle PHZ, or POZ, meaſures the Diſtance, from the Pole to
“ the Zenith, of an Inhabitant dwelling upon the Earth at the Point
“ H. FT is equal to the Axis BC, by Conſtruction; but, by the
“ Property of the Ellipſe, BC is equal to EH *plus* HF; taking a-
“ way from both FH, which is common, EH will remain equal to
“ HT. The Angles ETH, TEH, will therefore be equal, and,
“ conſequently, each of them will be half of the external Angle
“ EHF; but the Angle EHO is likewiſe equal to half of the Angle
“ EHF; therefore the Angles TEH, EHO, will be equal to one
“ another; and, conſequently, the Lines ET and HO will be pa-
“ rallel to one another; and the Angle POZ, which meaſures the
“ Diſtance from the Pole to the Zenith of the Point H, will be equal
“ to the Angle BET, which was, by Conſtruction, taken equal to
“ the given Diſtance of the Pole from the Zenith; *which was to be de-*
“ *monſtrated.*

“ Now, if the Proportion of the longeſt Diameter of the Ellipſe
“ BC to EF, the Diſtance of the Foci, be taken at Pleaſure, one
“ may by Calculation find all the Points of the Ellipſe as H, to de-
“ termine the Degrees by this Analogy.
“ *As FT or BC:*
“ *Is to EF::*
“ *So is the Sine of the Angle PET (the given Diſtance from the Pole to*
“ *the Zenith):*
“ *To the Sine of the Angle ETF, or TEH:* Whoſe Quantity will
“ conſequently be known. This Angle TEH being added to the
“ Angle

" Angle PET, the given Diſtance from the Pole to the Zenith of the
" Point H, will give the Quantity of the Angle BEH, which a Line
" drawn from the Focus to H, the Point required, makes with the
" Axis of the Ellipſe.

" Then in the Triangle EHF, whoſe Side EF is known, as well
" as the Angle EHF, which is the Double of the Angle TEH, and
" the Angle FEH Supplement of the Angle BEH; one ſhall have
" the Length of the Side EH, known in Parts of the Axis BC.

" After the ſame Manner, may be found the Angles BEI, BEV,
" *&c.* and the Length of the Lines EI, EV, to determine the Diſtance, from the Pole to the Zenith, of all the Degrees of the
" Circumference of the Earth; and in the rectilinear Triangles HEI,
" IEV, whoſe Sides HE, EI, EV, are known as well as the Angles comprehended between the Sides HE, EI, IE, EV, which
" are the Differences of the Angles BEH, BEI, BEV, determined
" above; one ſhall find the Length of the Chords HI, IV, comprehended between each Degree".

Monſ. *Caſſini*, in the Memoirs for the Year 1718, repeats the ſame Demonſtration; except that, before it, he ſhews, that if ſeveral Points be taken upon a Terreſtrial Meridian, on the Surface of an Elliptick Earth, as G, H, I, K, in ſuch Manner, that their reſpective Zeniths Fig. 183. Z, L, M, N, are diſtant from one another, an equal Number of Degrees meaſured in a Celeſtial Meridian; the Lines ZG, LH, MI, NK (which are perpendicular to the Ellipſe) being produced, will meet in the Points O, R, and S, making equal Angles; but as thoſe angular Points are not equally diſtant from the Curve of the Ellipſe, that Elliptic Arc muſt be the longeſt whoſe angular Point is fartheſt off. Now, by the former Demonſtration, it appears, that thoſe Arcs, which are taken neareſt to the leſſer Axis, will have their angular Points farther removed, *&c.*

If M. *Caſſini's* Meaſures of Terreſtrial Degrees, decreaſing from the Æquator towards the Pole, were grounded on Obſervations liable to no Error, he would have fully proved his Figure of the Earth. But ſince thoſe Meaſures (however accurately taken) are not built upon a mathematical Certainty, his Premiſes may be called in Queſtion, and his Concluſion, tho' mathematically drawn from theſe Premiſes, is only probable.

Now therefore, if I can ſhew from undoubted *Phænomena*, that his Concluſion will lead to an Abſurdity, his Meaſures muſt be falſe; becauſe his Reaſoning from them is juſt. This I ſhall endeavour to do firſt, which will diſprove his Figure of the Earth; and afterwards endeavour to point out ſome of the Errors which I ſuppoſe to have occaſioned the Miſtake in the Meaſures.

M. *Caſſini*, as well as the *Engliſh* Aſtronomers, believes that the Earth makes one Revolution about its Axis, once in 23 Hours 56', becauſe in that Time, the Plane of any Meridian returns to the ſame fixed Star from which it had departed.

Let H be taken in any Parallel of Latitude, as for Example, in the Latitude of 51° 46', a Plumb Line, L H, will be perpendicular to the Curve BH, at H, and produced paſs thro' the Zenith of the Point H, if the Earth had no Diurnal Rotation; but ſince the Earth moves round its Axis, all Bodies upon its Surface, endeavour to fly from the Axis of their Motion with a Force proportionable to their Diſtance from it in a Direction along the Plane of that Parallel, in which they are. Let that Force (explained by M. *Huygens*, and called a Centrifugal Force) be repreſented by the Line H *l*, or its Equal and Parallel *L b*; now a Plummet placed at L, if the Earth ſtood ſtill, would deſcend in the Line L H, but as it is at the ſame Time acted upon by the Force H *l* in the Direction L *b*, it will move in the Direction *L l* Diagonal of the Parallelogram H *l*, according to the known Laws of Mechanicks; and the Plumb Line L H, inſtead of being perpendicular to the Curve at H, will in the Latitude 51° 46' make an Angle of 5' with HL. This Angle will be leſs towards the Poles, till at the very Pole it quite vaniſhes, as it alſo does at the Æquator. Now ſince there is no ſuch Angle obſerved, but in all Water Levels we find the Plumb Line always perpendicular to the Line of Level, the Surface of the Earth muſt be depreſſed towards G, and riſe farther from the Axis towards I, in order to become perpendicular (that is, to have its Tangent perpendicular) to the Line L *l*, in which we have ſhewn that the Plumb Line muſt deſcend.

If there is any body ſo fond of M. *Caſſini*'s Hypotheſis, as to deny the Diurnal Motion of the Earth for the Sake of it, I hope they will be convinced, when I ſhew the Meaſures, upon which it is founded, to be inſufficient for determining the different Lengths of the Degrees of a Terreſtrial Meridian.

But here I would not be thought to endeavour to leſſen the Praiſe due to the Gentlemen of the *Royal Academy*, for carrying on a Meridian the whole Length of *France*, from *Dunkerque* thro' the Royal Obſervatory at *Paris*, quite to the *Pyrenæan* Mountains on the Borders of *Spain*. Aſtronomy and Geography are doubtleſs much indebted to the Encouragement given by the *French* Government, and to the Care of their Mathematicians, who have omitted no proper Method for drawing their Meridian, and correcting it as they went on. So many Obſervations of the riſing and ſetting Sun, ſo many equal Altitudes of the ſame Stars accurately taken, ſo many Digreſſions of Stars, ſo many other Obſervations made with the Teleſcope and good Pendulum-Clocks—all compared together, for the true ſettling of the direct Way of this famous Meridian, leave no Doubt but that it is as perfect as the Nature of the Thing is capable of. And, certainly, by the Help of this Line, and the ſeveral Triangles made uſe of for carrying it on, a better Map of *France* is made, than has ever been of any Country before: Nay, beſides, I believe we may, at a Medium, very well receive their Number of 57060, or 57061 Toiſes, for the Meaſure of a Degree

a Degree of a Meridian of the Earth, one with another. But to ſay, that thoſe Gentlemen could obſerve the Latitude ſo nicely, as to find a Difference in the Length of the Terreſtrial Degrees, and that only of eleven or twelve Toiſes (when they made it the leaſt) or of thirty one Toiſes (when they made it the moſt) is attributing to them an Exactneſs, ſo far beyond the Nature of the Inſtruments which they made uſe of, that it would be rather a Diſpraiſe than a Commendation to inſiſt upon it.

For in the firſt Place, the Inſtrument, with which they took Obſervations for the Latitude at the two Ends of their Meridian, was a ten Foot Sector (which was worſe than that which M. *Picard* had made Uſe of before, becauſe the Teleſcope of his Sector was of ten Foot, whereas M. *Caſſini*'s was but of three Foot, tho' applied to the ten Foot Sector) where the two hundredth Part of an Inch anſwers to eight Seconds of a Degree: Now the two hundredth Part of an Inch, being one of the leaſt viſible Parts that we can ſee in a divided Line; they could not take an Angle nearer than that; nay, their Inſtrument, according to their own Deſcription of it, was divided but to every twenty Seconds. Now they allow, that ſixteen Toiſes, upon the Surface of the Earth, anſwer to one Second in the Heavens; and they don't pretend to have taken an Obſervation nearer than to about three Seconds, which therefore cannot determine a Difference leſs than forty eight Toiſes; whereas the Degrees are only ſuppoſed to decreaſe at moſt, thirty one Toiſes each, from *Collioure* to *Dunkerque*. But an Error of eight Seconds would make a Difference of one hundred and twenty eight Toiſes, on the Surface of the Earth; above ten times greater than the Difference of Degrees in the firſt Suppoſition, and four Times greater than that Difference in the laſt. Beſides, the Latitude was not obſerved in the intermediate Places between *Paris* and *Collioure*, with the abovementioned Inſtrument of ten Foot Radius; but they made uſe of a Quadrant, whoſe Radius was only thirty nine Inches, and ſometimes an Octant of three Foot Radius. Nay, they ſay themſelves, in their Account, that it is not the Obſervations made at the Ends of the Meridian, that we are to deduce the Difference of the Length of a Degree from, but the Altitudes taken at ſeveral Places between the Extreams; and, if we grant, that they can take an Angle very well, to four or five Seconds, with the great Inſtrument, they cannot come nearer than twelve or fifteen Seconds, with the Quadrant or Octant, which we muſt depend upon for the Difference of the Meaſure of Degrees: So that upon the whole, we are to determine a Length of thirty one Toiſes, by an Inſtrument which is liable to err above two hundred.

If any Conſequences of this Kind could be drawn from actual meaſuring, a Degree of Latitude ſhould be meaſured at the Æquator, and a Degree of Longitude likewiſe meaſured there; and a Degree very northerly, as for Example, a whole Degree might be actually

meaſured upon the *Baltick* Sea, when frozen, in the Latitude of ſixty Degrees.—There, according to M. *Caſſini*'s laſt Suppoſition, a Degree would be of 56653 Toiſes, whereas, at the Æquator, it would be of 58019 Toiſes, the Difference being 1364 Toiſes, about the two and fortieth Part of a Degree, which muſt be ſenſible; and likewiſe the Degree of Longitude would, according to him, be of 56817 Toiſes, leſs by 1202, or the forty eighth Part, than a Degree of Latitude at the ſame Place.

But here it may be objected, that tho' the Latitude was not taken with the ten Foot Sector, in the intermediate Places between *Paris* and *Collioure*, yet the Latitude was taken with that Inſtrument at *Dunkerque*, *Paris*, and *Collioure*, and therefore the ſouthern Part of the Meridian, containing 6° 18′ 56″ may be compared with the northern Part of it, which contains 2° 12′ 16″; and that the former appears to contain more Toiſes, in Proportion to the Difference of Latitude at its Extremities, than the latter. To this may be anſwered, that, even in this Caſe, the Obſervations made cannot be nice enough to determine the Difference of the Length of Degrees; but there is another Error, which might conſiderably miſlead the *French* Gentlemen, and make the Degrees appear longer in the South of *France*; that is, the Error in taking the true Height of ſeveral Mountains in *Auvergne*, *Languedoc*, and among the *Pyræneans*. For if they have allowed too much for the Air's Refraction (which, by the Obſervations of Travellers, is greater towards the northern Regions, and diminiſhes as we go Southward) the Heights of thoſe Mountains will be taken too little, and their Baſes conſequently longer, which will make the Degrees appear bigger than they are. Let ABCD, for Example, be a Mountain, as the Mountain of *Rodez*, in the Latitude of 44° 21′, whoſe Height BD is 300 Toiſes, and whoſe Sides AB and BC (ſuppoſed to make an Angle of 26° 33′, with the Horizon) are found by Trigonometry, to be of 670,8 Toiſes each; if by a Miſtake, in taking the Height, it be ſuppoſed only equal to ED, or 257 Toiſes, then the Lines AB and BC will become EF and EG; ſo that the Baſe AC, which before was of 1200 Toiſes, will become equal to FG, which will appear to be 1279,6 Toiſes, by *Eucl.* 47. 1. Now one ſuch Miſtake, in one Degree, will give a Difference above twice as great as the ſuppoſed Difference of Degrees in that Latitude, which they make of 31 Toiſes. And that there was a Miſtake of this Kind in taking the Height of that Mountain, I ſhall ſhew. Fig. 184.

The Vapours, that generally float in the Air about the Tops of high Hills, make it ſo difficult to take their Height exactly, that Experiments, made with the Barometer, will, by obſerving the Fall of the Mercury, ſhew the Height nearer than any Thing elſe we know of. There were, indeed, ſeveral Experiments made with the Barometer *, where the Differences of the Height of the Mercury, from the Heights at which it ſtood at the Royal Obſervatory, are ſaid to anſwer to ſo

many Toiſes; but of nine Obſervations mentioned by M. *Caſſini*, there are not two where the Number of Toiſes, ſaid to correſpond to the Heights of the Barometer, do agree together.

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* *Mem. of the* Royal Academy, *for* 1718, Chap. 10.

The firſt Experiment of the Barometer there mentioned, made at *Collioure*, was this; ‘ At the Height of 11 ½ Toiſes above the Sea, ‘ the Barometer was ſet up, and the Mercury ſtood 3 ⅓ Lines higher ‘ than at the Royal Obſervatory (in the Tower of the eaſtern Hall) ‘ at the ſame Time; and therefore, ſince that Tower is 44 Toiſes ‘ higher than the Sea, 3 ½ Lines of Mercury muſt anſwer to 32 ½ ‘ Toiſes.

Now, reducing theſe Toiſes to Feet, and dividing by 3⅓ it will appear that a Height of 58,5 Feet will anſwer to the Fall of one Line of Mercury in the Barometer. Let this be taken as the Standard, and the other Obſervations be compared with it. This may be done by the following Table, where the firſt Column ſhews the Place where the Obſervation was made; the ſecond, the Fall or Riſe of Mercury at each Place expreſs'd in Lines, or 12th Parts of a *French* Inch; the third, the Heights or Depths anſwering to thoſe Lines of Mercury, which, in the Memoirs, are given in Toiſes, but here reduced to Feet; the fourth, the Number of Feet anſwering to one Line of Mercury in each Obſervation, which is the Quotient of the Feet in the third Column, divided by the Number of Lines in the Second.

Obſervations of the Barometer made at	*Lines of* Mercury.	*Said to correſpond with Feet.*	*The Fall of one Line of* Mercury *anſwers to Feet.*
I. Collioure	03⅓	195	58,5
II. *The Tower of* Maſſane.	31	2382	76,8
III. Bugarac	42	3636	86,5
IV. Rupeyroux	30	2181	72,7
V. Rodez	24	1647	68,6
VI. Rodez	20	1425	71,25
VII. Courlande	54	4812	89,1
VIII. Coſte	54	4890	92,4
IX. Clermont	03	200	66,6

A Sight of this Table will convince any one, that theſe Obſervations are not to be depended upon for determining the Height of the Mountains in the South of *France*; for the Differences are not ſmall, ſuch as might happen in making the Experiments; but ſuch as render the Obſervations uſeleſs for the Purpoſes abovementioned. For Example, the firſt and the ſeventh differ almoſt ⅓: And if 58,5 Feet were allowed for the Fall of one Line of Mercury in the ſeventh Obſervation, inſtead of 944 Feet, then the Mountain of *Coſte* would be but 3085 Feet, inſtead of 4890. Nay, upon examining the Memoirs, I find that in ſeveral Obſervations the Number of Toiſes, ſaid to correſpond

reſpond to a certain Height of Mercury, are only anſwerable to the Height of the Mountain above the Level of the Sea found by Trigonometry, from which the Height of the Royal Obſervatory, above the Sea, is ſubſtracted; though, by the Manner of the Expreſſion, a curſory Reader would imagine, that the Number of Toiſes named, was always proportionable to the Fall of the Mercury, and think all the Experiments and Obſervations very accurately made, when they ſeem to agree ſo well in every Reſpect.

Now after all, I do not queſtion but that the Height of the Barometer, might be as it is ſet down in the Memoirs, and well enough obſerved; but it was wrong to compare the Height of the Mercury in the South of *France*, with the Height that the Mercury was at in the Barometer of the Royal Obſervatory at the ſame Time; for, at that great Diſtance and Difference of Latitude, the Weather (and conſequently the Preſſure of the Air and Height of the Barometer at the ſame Level) might very much vary.

Even when there is fair Weather all over *France*, it does not follow that the Barometer ſhall ſtand at the ſame Height. Let us ſuppoſe, for Example, that a North Wind blows: Where-ever the Air is checked by a Chain of Mountains that run Eaſt and Weſt, it will be accumulated over thoſe Mountains, and conſequently preſs more as its Columns are higher; which will make the Mercury riſe higher than it would do with the ſame Wind, if there were no Mountains, or if they ran North and South.

The Way, to have made the Experiments with the Barometer exactly, would have been to have obſerved the Height of the Mercury at the Bottom and at the Top of the Mountain, and that with a Tube of a pretty large Bore (with a proportionably large Ciſtern for the ſtagnant Mercury) becauſe, in a ſmall Tube, the Mercury will often ſtick to the Sides, and riſe irregularly, as it will alſo in inclined Barometers. Simple Barometers are the beſt, and a magnifying Glaſs may be made uſe of to obſerve ſmall Riſes or Falls, having two fine and well made Indices to the Tube.

Dr. *Halley*, has given us * the Falls of *Mercury* in the Barometer, correſponding with the Heights to which the Barometer muſt be carried to produce thoſe Falls. The firſt tenth Part of an Inch in the Fall of the Mercury, he makes to anſwer to a Height of 90 Feet; the next tenth, to a Height ſomething greater, and ſo in Proportion, as the Air diminiſhes in Denſity, according as we riſe in Height. The Proportion of the firſt Tenth of the Mercury's Fall, he has built upon the Compariſon of the different Specifick Gravities of Air and Mercury; and taking Mercury to be 13 ½ Times heavier than Water, and Water (in cold Weather) to be 800 Times heavier than Air; it follows, that 13, 5x800, will give 10800; which Number, if it be taken in Feet, and divided by 120 (the Number of the 10th of an Inch in a Foot) we ſhall have 90 Feet anſwerable to the 10th Part of an Inch, and 75 Feet to a Line or the 12th Part of an Inch.

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* *See* Lowthorp's *Abridgment*, Vol. II. Chap. II. Sect. VIII.

Now, as very few Mountains in the World are 3 Miles high, and, generally ſpeaking, thoſe that we look upon as high Hills (except the *Andes*, and ſome others in *America*) are not much above a Mile high; we may, for finding the Height of Mountains, take a fixed Number of Feet in Altitude to anſwer to every 10th or 12th of an Inch in the Fall of the Mercury; becauſe 90 Feet are by Dr. *Halley* only taken for the firſt Tenth, and greater Heights for other Tenths, encreaſing with the Fall of the Mercury. Therefore I would propoſe another for a round Number, namely 96 Feet for every Tenth, and 80 Feet for every 12th of an Inch, very near the Number that I have found by my Calculation, which is as follows.

Fine Mercury (ſuch as is made uſe of in Barometers) is, generally ſpeaking, 13 $\frac{2}{3}$ Times heavier than Water, and, I found ſome brought from the *Eaſt-Indies*, to be 14 Times heavier. I have found Air in Summer, to be near 900 Times lighter than Water; and 800 Times in Winter; therefore I take 850 at a Medium. Now $850 \times 13\frac{2}{3} =$ 11606,6, which, divided by 120, gives 96,7 Feet, for $\frac{1}{10}$ of an Inch of Mercury, or 80.5 Feet for $\frac{1}{12}$ of an Inch. This Number, taken invariable, will, in taking the Height of ſeveral Hills, agree pretty well with the Numbers that come out, when Dr. *Halley*'s Table is made uſe of; and with the Experiment made by the late Profeſſor, Mr. *J. Caſwell*, who, having taken the Height of *Snowdon* Hill in *Caernarvonſhire* very accurately, and finding it to be 3720 Feet above the Level of the Sea, tried how much lower the Mercury would ſtand in the Barometer upon that Hill, than at the Level of the Sea, and obſerved it to ſubſide 3,9 Inches. I am ſenſible that it will be alledged, that the Air will be denſer than I may imagine on the Top of high Hills, becauſe of the great Cold, ſince they are generally covered with Snow; but then we are to conſider, that when we are got above a Mile higher than the Level of the Sea, the incumbent Atmoſphere has loſt almoſt a fifth Part of its Weight; and therefore the Air at the Top of the Hill, being ſo much leſs preſs'd, will, notwithſtanding the intenſe Cold, be more rarified than at the Bottom of the Hill.

Now if we go back to the Obſervations of the Barometer, made by the Gentlemen that drew the Meridian in *France*, we ſhall find, that on the Mountain of *Rodez*, in the Latitude of 44°, 21′ the Barometer fell 24 Lines below the Level of that in the Obſervatory, and they allowed only 274 $\frac{1}{2}$ Toiſes to correſpond to that Fall; whereas, according to Dr. *Halley*'s Proportion of a Tenth of an Inch for 90 Feet, they ſhould have taken 300 Toiſes; and tho' the *Hypotenuſes* AB, and BC, were taken longer than the bare Declivity of the Mountain (which would make the Error leſs than the 79 Toiſes I mentioned above) yet if my Proportion be made uſe of, *viz.* of 80 Feet for

meaſured upon the *Baltick* Sea, when frozen, in the Latitude of ſixty Degrees.—There, according to M. *Caſſini*'s laſt Suppoſition, a Degree would be of 56653 Toiſes, whereas, at the Æquator, it would be of 58019 Toiſes, the Difference being 1364 Toiſes, about the two and fortieth Part of a Degree, which muſt be ſenſible; and likewiſe the Degree of Longitude would, according to him, be of 56817 Toiſes, leſs by 1202, or the forty eighth Part, than a Degree of Latitude at the ſame Place.

But here it may be objected, that tho' the Latitude was not taken with the ten Foot Sector, in the intermediate Places between *Paris* and *Collioure*, yet the Latitude was taken with that Inſtrument at *Dunkerque*, *Paris*, and *Collioure*, and therefore the ſouthern Part of the Meridian, containing 6° 18′ 56″ may be compared with the northern Part of it, which contains 2° 12′ 16″; and that the former appears to contain more Toiſes, in Proportion to the Difference of Latitude at its Extremities, than the latter. To this may be anſwered, that, even in this Caſe, the Obſervations made cannot be nice enough to determine the Difference of the Length of Degrees; but there is another Error, which might conſiderably miſlead the *French* Gentlemen, and make the Degrees appear longer in the South of *France*; that is, the Error in taking the true Height of ſeveral Mountains in *Auvergne*, *Languedoc*, and among the *Pyræneans*. For if they have allowed too much for the Air's Refraction (which, by the Obſervations of Travellers, is greater towards the northern Regions, and diminiſhes as we go Southward) the Heights of thoſe Mountains will be taken too little, and their Baſes conſequently longer, which will make the Degrees appear bigger than they are. Let ABCD, for Example, be a Mountain, as the Mountain of *Rodez*, in the Latitude of 44° 21′, whoſe Height BD is 300 Toiſes, and whoſe Sides AB and BC (ſuppoſed to make an Angle of 26° 33′, with the Horizon) are found by Trigonometry, to be of 670,8 Toiſes each; if by a Miſtake, in taking the Height, it be ſuppoſed only equal to ED, or 257 Toiſes, then the Lines AB and BC will become EF and EG; ſo that the Baſe AC, which before was of 1200 Toiſes, will become equal to FG, which will appear to be 1279,6 Toiſes, by *Eucl.* 47. 1. Now one ſuch Miſtake, in one Degree, will give a Difference above twice as great as the ſuppoſed Difference of Degrees in that Latitude, which they make of 31 Toiſes. And that there was a Miſtake of this Kind in taking the Height of that Mountain, I ſhall ſhew.

Fig. 184.

The Vapours, that generally float in the Air about the Tops of high Hills, make it ſo difficult to take their Height exactly, that Experiments, made with the Barometer, will, by obſerving the Fall of the Mercury, ſhew the Height nearer than any Thing elſe we know of. There were, indeed, ſeveral Experiments made with the Barometer[*], where the Differences of the Height of the Mercury, from the Heights at which it ſtood at the Royal Obſervatory, are ſaid to anſwer to ſo many

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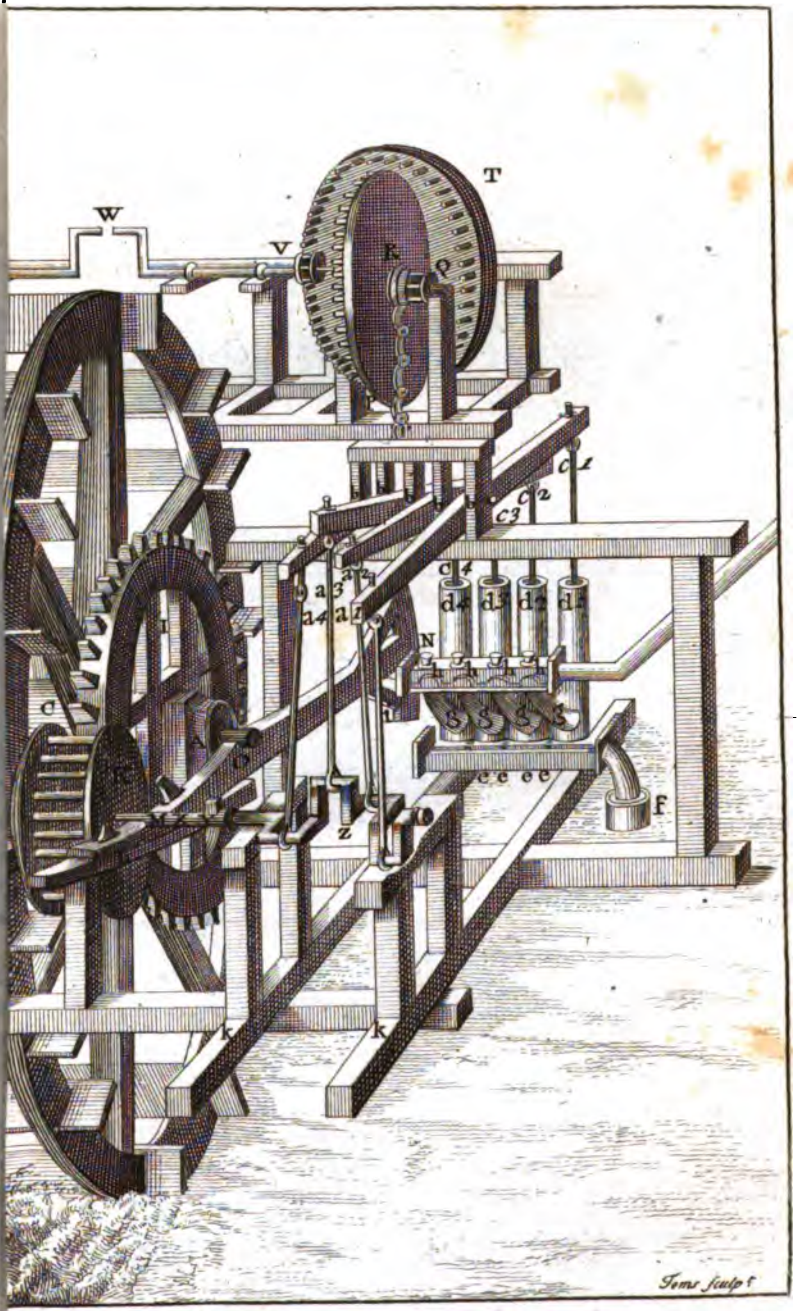
[*] *Mem. of the* Royal Academy, *for* 1718, Chap. 10.



Toms sculp^t

reſpond to a certain Height of Mercury, are only anſwerable to the Height of the Mountain above the Level of the Sea found by Trigonometry, from which the Height of the Royal Obſervatory, above the Sea, is ſubſtracted; though, by the Manner of the Expreſſion, a curſory Reader would imagine, that the Number of Toiſes named, was always proportionable to the Fall of the Mercury, and think all the Experiments and Obſervations very accurately made, when they ſeem to agree ſo well in every Reſpect.

Now after all, I do not queſtion but that the Height of the Barometer, might be as it is ſet down in the Memoirs, and well enough obſerved; but it was wrong to compare the Height of the Mercury in the South of *France*, with the Height that the Mercury was at in the Barometer of the Royal Obſervatory at the ſame Time; for, at that great Diſtance and Difference of Latitude, the Weather (and conſequently the Preſſure of the Air and Height of the Barometer at the ſame Level) might very much vary.

Even when there is fair Weather all over *France*, it does not follow that the Barometer ſhall ſtand at the ſame Height. Let us ſuppoſe, for Example, that a North Wind blows: Where-ever the Air is checked by a Chain of Mountains that run Eaſt and Weſt, it will be accumulated over thoſe Mountains, and conſequently preſs more as its Columns are higher; which will make the Mercury riſe higher than it would do with the ſame Wind, if there were no Mountains, or if they ran North and South.

The Way, to have made the Experiments with the Barometer exactly, would have been to have obſerved the Height of the Mercury at the Bottom and at the Top of the Mountain, and that with a Tube of a pretty large Bore (with a proportionably large Ciſtern for the ſtagnant Mercury) becauſe, in a ſmall Tube, the Mercury will often ſtick to the Sides, and riſe irregularly, as it will alſo in inclined Barometers. Simple Barometers are the beſt, and a magnifying Glaſs may be made uſe of to obſerve ſmall Riſes or Falls, having two fine and well made Indices to the Tube.

Dr. *Halley*, has given us * the Falls of *Mercury* in the Barometer, correſponding with the Heights to which the Barometer muſt be carried to produce thoſe Falls. The firſt tenth Part of an Inch in the Fall of the Mercury, he makes to anſwer to a Height of 90 Feet; the next tenth, to a Height ſomething greater, and ſo in Proportion, as the Air diminiſhes in Denſity, according as we riſe in Height. The Proportion of the firſt Tenth of the Mercury's Fall, he has built upon the Compariſon of the different Specifick Gravities of Air and Mercury; and taking Mercury to be 13 ½ Times heavier than Water, and Water (in cold Weather) to be 800 Times heavier than Air; it follows, that 13, 5x800, will give 10800; which Number, if it be taken in Feet, and divided by 120 (the Number of the 10th of

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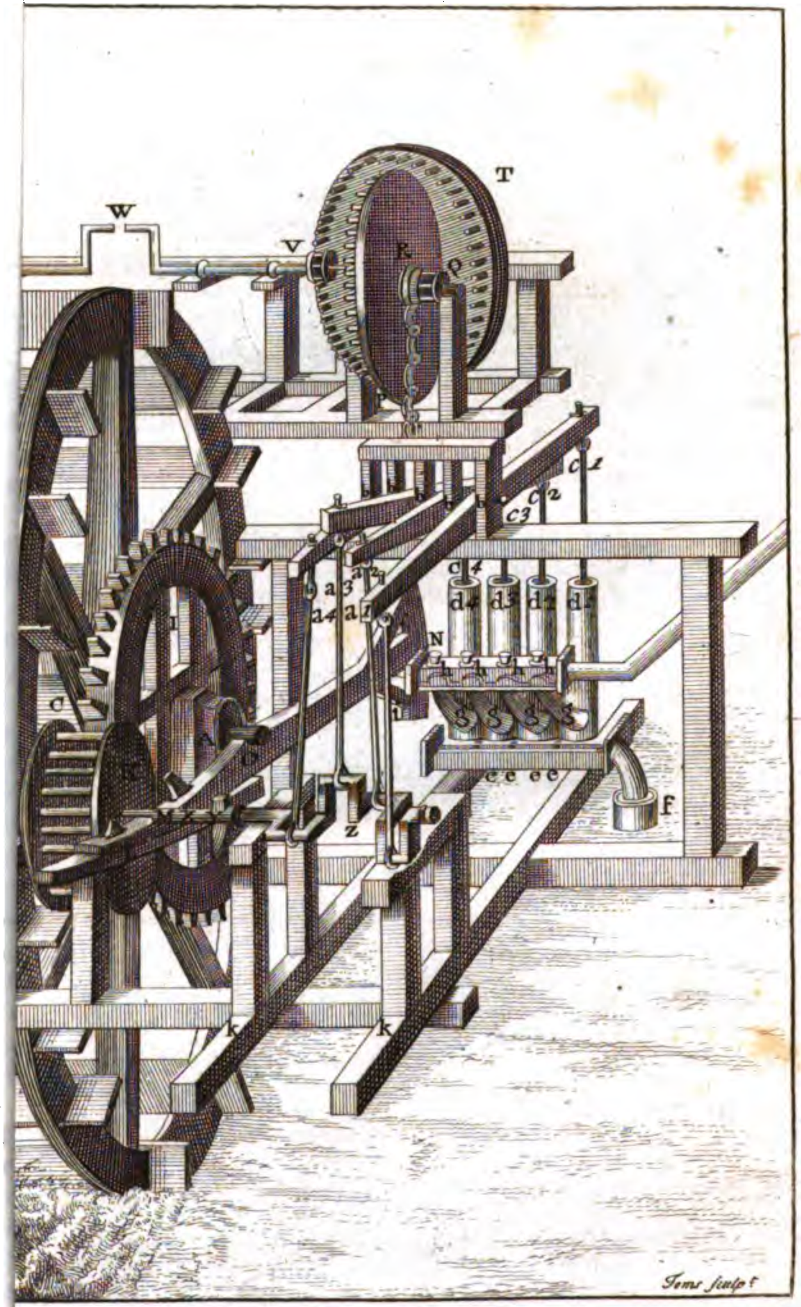
* *See* Lowthorp's *Abridgment*, Vol. II. Chap. II. Sect. VIII.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CHAP. VI.

GEOGRAPHY, NAVIGATION.

I. 1. THAT the Earth is of a ſpherical Figure, or nearly ſuch, has been proved ſo often, and by ſo many unanſwerable Arguments, that to repeat them here would be tedious. But, as a little Variation from a true Sphere (beſides the Irregularity of high Hills and deep Vallies) does not hinder us from calling the Earth a Globe; ſo, to determine what that Variation may be, ſince modern Philoſophers are divided about it, may be a Subject not ungrateful at this Time.

A Diſſertation concerning the Figure of the Earth, by the Rev. J. T. Deſaguliers, *LL. D. F. R. S.* n. 386. p. 201.

Monſieur *Caſſini* ſays: “ That the Earth is an oblong Spheroid,
“ higher at the Poles than the Æquator, making the Axis longer than
“ a Diameter of the Æquator about thirteen *French* Leagues, which
“ he deduces from comparing his Father's Meaſures of the Meridian,
“ from *Paris* to the *Pyrenæan* Mountains, with thoſe of Monſ. *Picard*;
“ of which an Account may be ſeen in the Memoirs of the
“ Royal Academy for 1713. But having afterwards continued the
“ Meridian, which is drawn through *France*, from *Paris* to *Dunkerque*,
“ he ſtill draws Conſequences to prove the Earth an oblong Spheroid;
“ but then makes the Axis exceed the Æquatorial Diameter
“ 34 Leagues.

“ Sir *Iſaac Newton* makes the Earth higher at the Æquator, and,
“ conſequently, flatted towards the Poles, reckoning its Æquatorial
“ Diameter 34 *Engliſh* Miles longer than the Axis; which he proves
“ from the Principles of Gravity, and the Centrifugal Force that ariſes
“ from the Diurnal Rotation of the Earth; and, to confirm this,
“ mentions ſeveral Experiments on Pendulums, which have been made
“ ſhorter, to ſwing Seconds, near the Æquator, than in greater Latitudes.”

Theſe are the two Opinions which have divided Philoſophers, and which we propoſe to examine here.

M. *Caſſini*, taking the Meaſures above-mentioned to be exact enough, not only to determine the Magnitude of a Degree of the Earth, correſponding with a Degree of the great Circle of the Heavens, but alſo to ſhew the Difference in the Degrees of the Earth; (reckoning thoſe, that were meaſured in the South of *France*, to exceed thoſe towards the North, by a certain Number of Toiſes and Feet) demonſtrates, that if the Degrees of the Earth are longer towards the Æquator than the Poles, the Plane of the Meridian muſt be an Ellipſe, whoſe long Axis is that of the Earth. Here follows his firſt Demonſtration. [*See the* French *Memoirs for the Year* 1713.] “ Let

Fig. 182. “ Let BDCR be an Ellipſe that repreſents a Meridian of the Earth, whoſe Poles B and C are at Ends of the great Axis BC, and whoſe Foci E and F are taken at Pleaſure. *Now, to divide this Ellipſe into Degrees, that is, to find ſeveral Points* H, I, V, *ſuch, that the Diſtance, from the Pole to the Zenith, of every one of them, ſhall be of any given Number of Degrees.*

Demonſtration. “ From E, one of the Foci of the Ellipſe, draw the Line ET, ſo that it may, with the Axis BC, make the Angle BET equal to the Diſtance given from the Pole to the Zenith. From the other Focus F, with the Diſtance BC equal to the Axis, draw an Arc, to cut the Line ET at T. I ſay, that the Line FT, drawn from the Point T to the Focus F, will cut the Ellipſe at the Point H; which Point is ſuch, that the Diſtance of the Pole, from its Zenith, contains the given Number of Degrees.

“ From the Point H, raiſe HZ, perpendicular to the Ellipſe, which will paſs through the Zenith Z; and, being produced inwards, will meet the Axis of the Earth at O, and (by the Property of the Ellipſe) divide the Angle EHF into two equal Parts. From the Point H, draw likewiſe HP, parallel to the Axis BC, and directed to the Pole P, ſuppoſed at an infinite Diſtance. The Angle PHZ, or POZ, meaſures the Diſtance, from the Pole to the Zenith, of an Inhabitant dwelling upon the Earth at the Point H. FT is equal to the Axis BC, by Conſtruction; but, by the Property of the Ellipſe, BC is equal to EH *plus* HF; taking away from both FH, which is common, EH will remain equal to HT. The Angles ETH, TEH, will therefore be equal, and, conſequently, each of them will be half of the external Angle EHF; but the Angle EHO is likewiſe equal to half of the Angle EHF; therefore the Angles TEH, EHO, will be equal to one another; and, conſequently, the Lines ET and HO will be parallel to one another; and the Angle POZ, which meaſures the Diſtance from the Pole to the Zenith of the Point H, will be equal to the Angle BET, which was, by Conſtruction, taken equal to the given Diſtance of the Pole from the Zenith; *which was to be demonſtrated.*

“ Now, if the Proportion of the longeſt Diameter of the Ellipſe BC to EF, the Diſtance of the Foci, be taken at Pleaſure, one may by Calculation find all the Points of the Ellipſe as H, to determine the Degrees by this Analogy.

“ *As FT or BC:*
“ *Is to EF::*
“ *So is the Sine of the Angle PET (the given Diſtance from the Pole to the Zenith):*
“ *To the Sine of the Angle ETF, or TEH:* Whoſe Quantity will conſequently be known. This Angle TEH being added to the

" Angle PET, the given Diſtance from the Pole to the Zenith of the " Point H, will give the Quantity of the Angle BEH, which a Line " drawn from the Focus to H, the Point required, makes with the " Axis of the Ellipſe.

" Then in the Triangle EHF, whoſe Side EF is known, as well " as the Angle EHF, which is the Double of the Angle TEH, and " the Angle FEH Supplement of the Angle BEH; one ſhall have " the Length of the Side EH, known in Parts of the Axis BC.

" After the ſame Manner, may be found the Angles BEI, BEV, " *&c.* and the Length of the Lines EI, EV, to determine the Diſ- " tance, from the Pole to the Zenith, of all the Degrees of the " Circumference of the Earth; and in the rectilinear Triangles HEI, " IEV, whoſe Sides HE, EI, EV, are known as well as the An- " gles comprehended between the Sides HE, EI, IE, EV, which " are the Differences of the Angles BEH, BEI, BEV, determined " above; one ſhall find the Length of the Chords HI, IV, compre- " hended between each Degree".

Monſ. *Caſſini*, in the Memoirs for the Year 1718, repeats the ſame Demonſtration; except that, before it, he ſhews, that if ſeveral Points be taken upon a Terreſtrial Meridian, on the Surface of an Elliptick Earth, as G, H, I, K, in ſuch Manner, that their reſpective Zeniths Z, L, M, N, are diſtant from one another, an equal Number of Degrees meaſured in a Celeſtial Meridian; the Lines Z G, L H, M I, N K (which are perpendicular to the Ellipſe) being produced, will meet in the Points O, R, and S, making equal Angles; but as thoſe angular Points are not equally diſtant from the Curve of the Ellipſe, that Elliptic Arc muſt be the longeſt whoſe angular Point is fartheſt off. Now, by the former Demonſtration, it appears, that thoſe Arcs, which are taken neareſt to the leſſer Axis, will have their angular Points farther removed, *&c.* Fig. 183.

If M. *Caſſini*'s Meaſures of Terreſtrial Degrees, decreaſing from the Æquator towards the Pole, were grounded on Obſervations liable to no Error, he would have fully proved his Figure of the Earth. But ſince thoſe Meaſures (however accurately taken) are not built upon a mathematical Certainty, his Premiſes may be called in Queſtion, and his Concluſion, tho' mathematically drawn from theſe Premiſes, is only probable.

Now therefore, if I can ſhew from undoubted *Phænomena*, that his Concluſion will lead to an Abſurdity, his Meaſures muſt be falſe; becauſe his Reaſoning from them is juſt. This I ſhall endeavour to do firſt, which will diſprove his Figure of the Earth; and afterwards endeavour to point out ſome of the Errors which I ſuppoſe to have occaſioned the Miſtake in the Meaſures.

M. *Caſſini*, as well as the *Engliſh* Aſtronomers, believes that the Earth makes one Revolution about its Axis, once in 23 Hours 56′, becauſe in that Time, the Plane of any Meridian returns to the ſame fixed Star from which it had departed.

Let H be taken in any Parallel of Latitude, as for Example, in the Latitude of 51° 46′, a Plumb Line, L H, will be perpendicular to the Curve BH, at H, and produced paſs thro' the Zenith of the Point H, if the Earth had no Diurnal Rotation; but ſince the Earth moves round its Axis, all Bodies upon its Surface, endeavour to fly from the Axis of their Motion with a Force proportionable to their Diſtance from it in a Direction along the Plane of that Parallel, in which they are. Let that Force (explained by M. *Huygens*, and called a Centrifugal Force) be repreſented by the Line H *l*, or its Equal and Parallel *L b*; now a Plummet placed at L, if the Earth ſtood ſtill, would deſcend in the Line L H, but as it is at the ſame Time acted upon by the Force H *l* in the Direction L *b*, it will move in the Direction *L l* Diagonal of the Parallelogram H *l*, according to the known Laws of Mechanicks; and the Plumb Line L H, inſtead of being perpendicular to the Curve at H, will in the Latitude 51° 46′ make an Angle of 5′ with HL. This Angle will be leſs towards the Poles, till at the very Pole it quite vaniſhes, as it alſo does at the Æquator. Now ſince there is no ſuch Angle obſerved, but in all Water Levels we find the Plumb Line always perpendicular to the Line of Level, the Surface of the Earth muſt be depreſſed towards G, and riſe farther from the Axis towards I, in order to become perpendicular (that is, to have its Tangent perpendicular) to the Line L *l*, in which we have ſhewn that the Plumb Line muſt deſcend.

If there is any body ſo fond of M. *Caſſini*'s Hypotheſis, as to deny the Diurnal Motion of the Earth for the Sake of it, I hope they will be convinced, when I ſhew the Meaſures, upon which it is founded, to be inſufficient for determining the different Lengths of the Degrees of a Terreſtrial Meridian.

But here I would not be thought to endeavour to leſſen the Praiſe due to the Gentlemen of the *Royal Academy*, for carrying on a Meridian the whole Length of *France*, from *Dunkerque* thro' the Royal Obſervatory at *Paris*, quite to the *Pyrenæan* Mountains on the Borders of *Spain*. Aſtronomy and Geography are doubtleſs much indebted to the Encouragement given by the *French* Government, and to the Care of their Mathematicians, who have omitted no proper Method for drawing their Meridian, and correcting it as they went on. So many Obſervations of the riſing and ſetting Sun, ſo many equal Altitudes of the ſame Stars accurately taken, ſo many Digreſſions of Stars, ſo many other Obſervations made with the Teleſcope and good Pendulum-Clocks—all compared together, for the true ſettling of the direct Way of this famous Meridian, leave no Doubt but that it is as perfect as the Nature of the Thing is capable of. And, certainly, by the Help of this Line, and the ſeveral Triangles made uſe of for carrying it on, a better Map of *France* is made, than has ever been of any Country before: Nay, beſides, I believe we may, at a Medium, very well receive their Number of 57060, or 57061 Toiſes, for the Meaſure of

a Degree of a Meridian of the Earth, one with another. But to ſay, that thoſe Gentlemen could obſerve the Latitude ſo nicely, as to find a Difference in the Length of the Terreſtrial Degrees, and that only of eleven or twelve Toiſes (when they made it the leaſt) or of thirty one Toiſes (when they made it the moſt) is attributing to them an Exactneſs, ſo far beyond the Nature of the Inſtruments which they made uſe of, that it would be rather a Diſpraiſe than a Commendation to inſiſt upon it.

For in the firſt Place, the Inſtrument, with which they took Obſervations for the Latitude at the two Ends of their Meridian, was a ten Foot Sector (which was worſe than that which M. *Picard* had made Uſe of before, becauſe the Teleſcope of his Sector was of ten Foot, whereas M. *Caſſini*'s was but of three Foot, tho' applied to the ten Foot Sector) where the two hundredth Part of an Inch anſwers to eight Seconds of a Degree: Now the two hundredth Part of an Inch, being one of the leaſt viſible Parts that we can ſee in a divided Line; they could not take an Angle nearer than that; nay, their Inſtrument, according to their own Deſcription of it, was divided but to every twenty Seconds. Now they allow, that ſixteen Toiſes, upon the Surface of the Earth, anſwer to one Second in the Heavens; and they don't pretend to have taken an Obſervation nearer than to about three Seconds, which therefore cannot determine a Difference leſs than forty eight Toiſes; whereas the Degrees are only ſuppoſed to decreaſe at moſt, thirty one Toiſes each, from *Collioure* to *Dunkerque*. But an Error of eight Seconds would make a Difference of one hundred and twenty eight Toiſes, on the Surface of the Earth; above ten times greater than the Difference of Degrees in the firſt Suppoſition, and four Times greater than that Difference in the laſt. Beſides, the Latitude was not obſerved in the intermediate Places between *Paris* and *Collioure*, with the abovementioned Inſtrument of ten Foot Radius; but they made uſe of a Quadrant, whoſe Radius was only thirty nine Inches, and ſometimes an Octant of three Foot Radius. Nay, they ſay themſelves, in their Account, that it is not the Obſervations made at the Ends of the Meridian, that we are to deduce the Difference of the Length of a Degree from, but the Altitudes taken at ſeveral Places between the Extreams; and, if we grant, that they can take an Angle very well, to four or five Seconds, with the great Inſtrument, they cannot come nearer than twelve or fifteen Seconds, with the Quadrant or Octant, which we muſt depend upon for the Difference of the Meaſure of Degrees: So that upon the whole, we are to determine a Length of thirty one Toiſes, by an Inſtrument which is liable to err above two hundred.

If any Conſequences of this Kind could be drawn from actual meaſuring, a Degree of Latitude ſhould be meaſured at the Æquator, and a Degree of Longitude likewiſe meaſured there; and a Degree very northerly, as for Example, a whole Degree might be actually

meaſured upon the *Baltick* Sea, when frozen, in the Latitude of ſixty Degrees.—There, according to M. *Caſſini*'s laſt Suppoſition, a Degree would be of 56653 Toiſes, whereas, at the Æquator, it would be of 58019 Toiſes, the Difference being 1364 Toiſes, about the two and fortieth Part of a Degree, which muſt be ſenſible; and likewiſe the Degree of Longitude would, according to him, be of 56817 Toiſes, leſs by 1202, or the forty eighth Part, than a Degree of Latitude at the ſame Place.

But here it may be objected, that tho' the Latitude was not taken with the ten Foot Sector, in the intermediate Places between *Paris* and *Collioure*, yet the Latitude was taken with that Inſtrument at *Dunkerque*, *Paris*, and *Collioure*, and therefore the ſouthern Part of the Meridian, containing 6° 18′ 56″ may be compared with the northern Part of it, which contains 2° 12′ 16″; and that the former appears to contain more Toiſes, in Proportion to the Difference of Latitude at its Extremities, than the latter. To this may be anſwered, that, even in this Caſe, the Obſervations made cannot be nice enough to determine the Difference of the Length of Degrees; but there is another Error, which might conſiderably miſlead the *French* Gentlemen, and make the Degrees appear longer in the South of *France*; that is, the Error in taking the true Height of ſeveral Mountains in *Auvergne*, *Languedoc*, and among the *Pyræneans*. For if they have allowed too much for the Air's Refraction (which, by the Obſervations of Travellers, is greater towards the northern Regions, and diminiſhes as we go Southward) the Heights of thoſe Mountains will be taken too little, and their Baſes conſequently longer, which will make the Degrees appear bigger than they are. Fig. 184. Let ABCD, for Example, be a Mountain, as the Mountain of *Rodez*, in the Latitude of 44° 21′, whoſe Height BD is 300 Toiſes, and whoſe Sides AB and BC (ſuppoſed to make an Angle of 26° 33′, with the Horizon) are found by Trigonometry, to be of 670,8 Toiſes each; if by a Miſtake, in taking the Height, it be ſuppoſed only equal to ED, or 257 Toiſes, then the Lines AB and BC will become EF and EG; ſo that the Baſe AC, which before was of 1200 Toiſes, will become equal to FG, which will appear to be 1279,6 Toiſes, by *Eucl.* 47. 1. Now one ſuch Miſtake, in one Degree, will give a Difference above twice as great as the ſuppoſed Difference of Degrees in that Latitude, which they make of 31 Toiſes. And that there was a Miſtake of this Kind in taking the Height of that Mountain, I ſhall ſhew.

The Vapours, that generally float in the Air about the Tops of high Hills, make it ſo difficult to take their Height exactly, that Experiments, made with the Barometer, will, by obſerving the Fall of the Mercury, ſhew the Height nearer than any Thing elſe we know of. There were, indeed, ſeveral Experiments made with the Barometer *, where the Differences of the Height of the Mercury, from the Heights at which it ſtood at the Royal Obſervatory, are ſaid to anſwer to ſo

many Toiſes; but of nine Obſervations mentioned by M. *Caſſini*, there are not two where the Number of Toiſes, ſaid to correſpond to the Heights of the Barometer, do agree together.

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* *Mem. of the* Royal Academy, *for* 1718, Chap. 10.

The firſt Experiment of the Barometer there mentioned, made at *Collioure*, was this; ‘ At the Height of 11 ½ Toiſes above the Sea, ‘ the Barometer was ſet up, and the Mercury ſtood 3 ⅓ Lines higher ‘ than at the Royal Obſervatory (in the Tower of the eaſtern Hall) ‘ at the ſame Time; and therefore, ſince that Tower is 44 Toiſes ‘ higher than the Sea, 3 ½ Lines of Mercury muſt anſwer to 32 ½ ‘ Toiſes.

Now, reducing theſe Toiſes to Feet, and dividing by 3⅓ it will appear that a Height of 58,5 Feet will anſwer to the Fall of one Line of Mercury in the Barometer. Let this be taken as the Standard, and the other Obſervations be compared with it. This may be done by the following Table, where the firſt Column ſhews the Place where the Obſervation was made; the ſecond, the Fall or Riſe of Mercury at each Place expreſs'd in Lines, or 12th Parts of a *French* Inch; the third, the Heights or Depths anſwering to thoſe Lines of Mercury, which, in the Memoirs, are given in Toiſes, but here reduced to Feet; the fourth, the Number of Feet anſwering to one Line of Mercury in each Obſervation, which is the Quotient of the Feet in the third Column, divided by the Number of Lines in the Second.

Obſervations of the Barometer made at	*Lines of* Mercury.	*Said to correſpond with Feet.*	*The Fall of one Line of* Mercury *anſwers to Feet.*
I. Collioure	03⅓	195	58,5
II. *The Tower of* Maſſane.	31	2382	76,8
III. Bugarac	42	3636	86,5
IV. Rupeyroux	30	2181	72,7
V. Rodez	24	1647	68,6
VI. Rodez	20	1425	71,25
VII. Courlande	54	4812	89,1
VIII. Coſte	54	4890	92,4
IX. Clermont	03	200	66,6

A Sight of this Table will convince any one, that theſe Obſervations are not to be depended upon for determining the Height of the Mountains in the South of *France*; for the Differences are not ſmall, ſuch as might happen in making the Experiments; but ſuch as render the Obſervations uſeleſs for the Purpoſes abovementioned. For Example, the firſt and the ſeventh differ almoſt ⅓: And if 58,5 Feet were allowed for the Fall of one Line of Mercury in the ſeventh Obſervation, inſtead of 944 Feet, then the Mountain of *Coſte* would be but 3085 Feet, inſtead of 4890. Nay, upon examining the Memoirs, I find that in ſeveral Obſervations the Number of Toiſes, ſaid to cor-

CHAP. VI.

GEOGRAPHY, NAVIGATION.

I. 1. THAT the Earth is of a ſpherical Figure, or nearly ſuch, has been proved ſo often, and by ſo many unanſwerable Arguments, that to repeat them here would be tedious. But, as a little Variation from a true Sphere (beſides the Irregularity of high Hills and deep Vallies) does not hinder us from calling the Earth a Globe; ſo, to determine what that Variation may be, ſince modern Philoſophers are divided about it, may be a Subject not ungrateful at this Time.

A Diſſertation concerning the Figure of the Earth, by the Rev. J. T. Deſaguliers, *LL. D. F. R. S.* n. 386. p. 201.

Monſieur *Caſſini* ſays: "That the Earth is an oblong Spheroid, "higher at the Poles than the Æquator, making the Axis longer than "a Diameter of the Æquator about thirteen *French* Leagues, which "he deduces from comparing his Father's Meaſures of the Meridian, "from *Paris* to the *Pyrenæan* Mountains, with thoſe of Monſ. *Picard*; of which an Account may be ſeen in the Memoirs of the "Royal Academy for 1713. But having afterwards continued the "Meridian, which is drawn through *France*, from *Paris* to *Dunkerque*, "he ſtill draws Conſequences to prove the Earth an oblong Spheroid; but then makes the Axis exceed the Æquatorial Diameter "34 Leagues.

"Sir *Iſaac Newton* makes the Earth higher at the Æquator, and, "conſequently, flatted towards the Poles, reckoning its Æquatorial "Diameter 34 *Engliſh* Miles longer than the Axis; which he proves "from the Principles of Gravity, and the Centrifugal Force that a"riſes from the Diurnal Rotation of the Earth; and, to confirm this, "mentions ſeveral Experiments on Pendulums, which have been made "ſhorter, to ſwing Seconds, near the Æquator, than in greater La"titudes."

Theſe are the two Opinions which have divided Philoſophers, and which we propoſe to examine here.

M. *Caſſini*, taking the Meaſures above-mentioned to be exact enough, not only to determine the Magnitude of a Degree of the Earth, correſponding with a Degree of the great Circle of the Heavens, but alſo to ſhew the Difference in the Degrees of the Earth; (reckoning thoſe, that were meaſured in the South of *France*, to exceed thoſe towards the North, by a certain Number of Toiſes and Feet) demonſtrates, that if the Degrees of the Earth are longer towards the Æquator than the Poles, the Plane of the Meridian muſt be an Ellipſe, whoſe long Axis is that of the Earth. Here follows his firſt Demonſtration. [*See the* French *Memoirs for the Year* 1713.] "Let

Fig. 182.

“ Let BDCR be an Ellipſe that repreſents a Meridian of the Earth, whoſe Poles B and C are at Ends of the great Axis BC, and whoſe Foci E and F are taken at Pleaſure. *Now, to divide this Ellipſe into Degrees, that is, to find ſeveral Points* H, I, V, *ſuch, that the Diſtance, from the Pole to the Zenith, of every one of them, ſhall be of any given Number of Degrees.*

Demonſtration.

“ From E, one of the Foci of the Ellipſe, draw the Line ET, ſo that it may, with the Axis BC, make the Angle BET equal to the Diſtance given from the Pole to the Zenith. From the other Focus F, with the Diſtance BC equal to the Axis, draw an Arc, to cut the Line ET at T. I ſay, that the Line FT, drawn from the Point T to the Focus F, will cut the Ellipſe at the Point H; which Point is ſuch, that the Diſtance of the Pole, from its Zenith, contains the given Number of Degrees.

“ From the Point H, raiſe HZ, perpendicular to the Ellipſe, which will paſs through the Zenith Z; and, being produced inwards, will meet the Axis of the Earth at O, and (by the Property of the Ellipſe) divide the Angle EHF into two equal Parts. From the Point H, draw likewiſe HP, parallel to the Axis BC, and directed to the Pole P, ſuppoſed at an infinite Diſtance. The Angle PHZ, or POZ, meaſures the Diſtance, from the Pole to the Zenith, of an Inhabitant dwelling upon the Earth at the Point H. FT is equal to the Axis BC, by Conſtruction; but, by the Property of the Ellipſe, BC is equal to EH *plus* HF; taking away from both FH, which is common, EH will remain equal to HT. The Angles ETH, TEH, will therefore be equal, and, conſequently, each of them will be half of the external Angle EHF; but the Angle EHO is likewiſe equal to half of the Angle EHF; therefore the Angles TEH, EHO, will be equal to one another; and, conſequently, the Lines E T and H O will be parallel to one another; and the Angle POZ, which meaſures the Diſtance from the Pole to the Zenith of the Point H will be equal to the Angle BET, which was, by Conſtruction, taken equal to the given Diſtance of the Pole from the Zenith; *which was to be demonſtrated.*

“ Now, if the Proportion of the longeſt Diameter of the Ellipſe BC to EF, the Diſtance of the Foci, be taken at Pleaſure, one may by Calculation find all the Points of the Ellipſe as H, to determine the Degrees by this Analogy.

“ *As FT or BC:*
“ *Is to E F ::*
“ *So is the Sine of the Angle PET (the given Diſtance from the Pole to the Zenith):*

“ *To the Sine of the Angle ETF, or TEH:* Whoſe Quantity will conſequently be known. This Angle TEH being added to the

“ Angle PET, the given Diſtance from the Pole to the Zenith of the Point H, will give the Quantity of the Angle BEH, which a Line drawn from the Focus to H, the Point required, makes with the Axis of the Ellipſe.

“ Then in the Triangle EHF, whoſe Side EF is known, as well as the Angle EHF, which is the Double of the Angle TEH, and the Angle FEH Supplement of the Angle BEH; one ſhall have the Length of the Side EH, known in Parts of the Axis BC.

“ After the ſame Manner, may be found the Angles BEI, BEV, *&c.* and the Length of the Lines EI, EV, to determine the Diſtance, from the Pole to the Zenith, of all the Degrees of the Circumference of the Earth; and in the rectilinear Triangles HEI, IEV, whoſe Sides HE, EI, EV, are known as well as the Angles comprehended between the Sides HE, EI, IE, EV, which are the Differences of the Angles BEH, BEI, BEV, determined above; one ſhall find the Length of the Chords HI, IV, comprehended between each Degree”.

Monſ. *Caſſini*, in the Memoirs for the Year 1718, repeats the ſame Demonſtration; except that, before it, he ſhews, that if ſeveral Points be taken upon a Terreſtrial Meridian, on the Surface of an Elliptick Earth, as G, H, I, K, in ſuch Manner, that their reſpective Zeniths Z, L, M, N, are diſtant from one another, an equal Number of Degrees meaſured in a Celeſtial Meridian; the Lines Z G, L H, M I, N K (which are perpendicular to the Ellipſe) being produced, will meet in the Points O, R, and S, making equal Angles; but as thoſe angular Points are not equally diſtant from the Curve of the Ellipſe, that Elliptic Arc muſt be the longeſt whoſe angular Point is fartheſt off. Now, by the former Demonſtration, it appears, that thoſe Arcs, which are taken neareſt to the leſſer Axis, will have their angular Points farther removed, *&c.*

Fig. 183.

If M. *Caſſini*'s Meaſures of Terreſtrial Degrees, decreaſing from the Æquator towards the Pole, were grounded on Obſervations liable to no Error, he would have fully proved his Figure of the Earth. But ſince thoſe Meaſures (however accurately taken) are not built upon a mathematical Certainty, his Premiſes may be called in Queſtion, and his Concluſion, tho' mathematically drawn from theſe Premiſes, is only probable.

Now therefore, if I can ſhew from undoubted *Phænomena*, that his Concluſion will lead to an Abſurdity, his Meaſures muſt be falſe; becauſe his Reaſoning from them is juſt. This I ſhall endeavour to do firſt, which will diſprove his Figure of the Earth; and afterwards endeavour to point out ſome of the Errors which I ſuppoſe to have occaſioned the Miſtake in the Meaſures.

M. *Caſſini*, as well as the *Engliſh* Aſtronomers, believes that the Earth makes one Revolution about its Axis, once in 23 Hours 56′, becauſe in that Time, the Plane of any Meridian returns to the ſame fixed Star from which it had departed.

Let

to allow for the lengthening of the ſame Rod by the Summer Heat, when it is ſhut up in a Caſe, and carried towards the Æquator. For though the Experiments on Pendulums, made by ſeveral Perſons that travelled Southward, differ among themſelves, yet they all agree in this, that the Obſervers were obliged to ſhorten their Pendulums, in order to make them ſwing Seconds, as they went towards the Æquator. And when we come to compare them together, in order to have the exact Proportion of Length in different Latitudes, we muſt rely on the moſt exact Experimenter, which we may very well do on M. *Richer*; becauſe when he found a Difference, he was ſo careful to find out how much it was, that he cauſed a ſimple Pendulum to ſwing, and compared it with a good Pendulum Clock, which he did ſeveral Times every Week for 10 Months together; and when he returned to *France*, he compared it with the Length of the Pendulum at *Paris*; which is of 3 Feet $8\frac{2}{5}$ Lines (or 39,129 *Engliſh* Inches) and found it to be ſhorter by $1\frac{1}{4}$ Line.

Continued by the ſame, n. 388. p. 277.

3. Since the writing of the foregoing Papers, I met with a Diſſertation of M. *Mairan* (in the Memoirs of the Royal Academy of *Paris*, for the Year 1720.) wherein the learned and ingenious Author has taken a great deal of Pains to reconcile the Obſervations made on Pendulums (found to be ſhorter at the Æquator than at *Paris*, when they ſwing Seconds) with the oblong ſpheroidical Figure of the Earth, deduced from M. *Caſſini*'s Meaſures. And though upon a ſtrict Examination of his Conjectures, and what he gives for Demonſtrations, I do not find Reaſon to alter my Opinion concerning the oblate or flatted Spheroid, which Sir *Iſaac Newton* has ſhewn to be the Figure of the Earth; yet ſince it might be thought by ſome, who have read Monſ. *Mairan*'s Treatiſe, and afterwards may read mine, that I have not conſidered all the Circumſtances that He has done, and that I have not been exact enough in the Mathematical Part of my Diſſertation, becauſe I have drawn ſome Concluſions from ſuppoſing the Figure of the Earth ſpherical, when I ſhould have ſuppoſed it an oblong Spheroid; I beg Leave to ſhew here, wherein I think M. *Mairan* is miſtaken, and to give thoſe additional Proofs of my Aſſertions, which I promiſed the Society when I gave in my laſt Paper.

Firſt then I begin with the Conjectures.

Monſ. *Mairan* ſays, that it is as reaſonable to ſuppoſe the Earth (if it was once fluid) to have been an oblong Spheroid at firſt, as a Sphere; and that, in ſuch a Caſe, the Centrifugal Force of the ſeveral Parts of the Earth, ariſing from its Revolution about its Axis, which might convert a Sphere into an oblate Spheroid, would only change an oblong Spheroid into one leſs oblong.

If the Earth was at firſt a Fluid (ſuppoſed homogeneous, and of any given Form) and left to thoſe Laws, which we find to obtain at preſent; it muſt put on a ſpherical Figure, for the ſame Reaſon that

Fig. 183

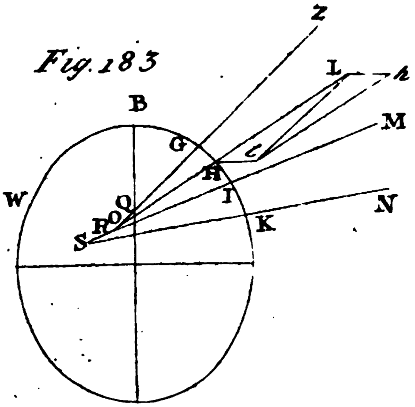


Fig. 184

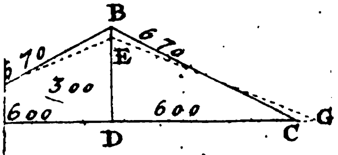


Fig. 186

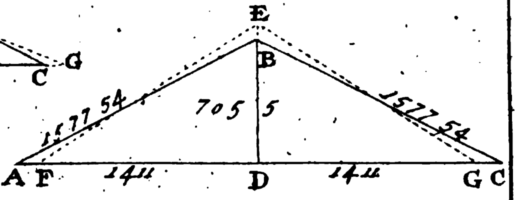


Fig. 187

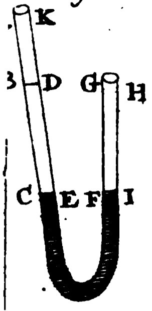


Fig. 188

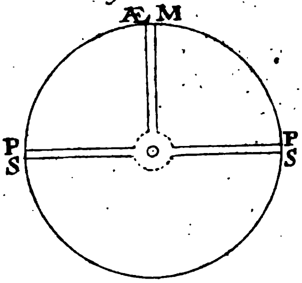
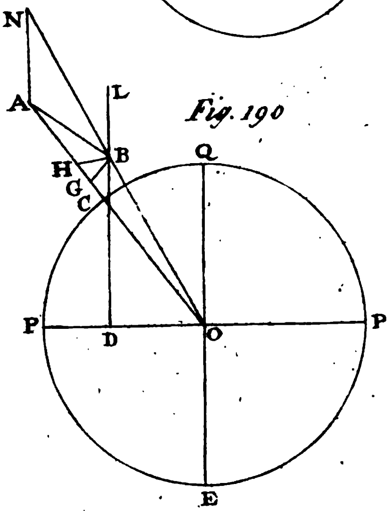


Fig. 190



Drops of Mercury, of Water, and other Fluids, put on ſuch a Figure. And to ſuppoſe any Change made in that Figure from the Preſſure of an external Fluid, filling up all Space, is contrary to what has been demonſtrated by Sir *Iſaac Newton* in his *Principia Lib.* 2. *Prop.* 19. where he ſhews, *That if any Portion of a Fluid be compreſs'd by the ſame or any other homogeneous Fluid, that Portion will not have its Figure altered by that Preſſure.*

And indeed we ſee, that in the Receiver of the Air-Pump, Lumps of Butter, coagulated Oil, or Honey, Drops of Quickſilver or Water, *&c.* have the ſame Figure, whether the Preſſure of the Air acts upon them, or be taken off by exhauſting the Receiver.

That a fluid Subſtance, of any Figure, will by the Gravity of its Parts become ſpherical, is plain by the following Demonſtration.

Let ABCDE be a Portion of a homogeneous Fluid, whoſe Parts tend towards one another, and whoſe Figure is not ſpherical. If in ſuch a Fluid we ſuppoſe a Syphon as ACE (or which is the ſame Thing, if all the Fluid ſhould be frozen, except the Canal ACE) whoſe Legs AC and CE are unequal, and meet at C, the Center of the Fluid, towards which there is the greateſt Tendency; the Fluid will run out at A in the Leg AC, till it be come down as far as *g* in the Leg CE, ſuppoſing C *g* equal to AC. But if the Leg AC be lengthened as far as *c*, then the Fluid will only come down as far as *e* in the Leg CE, and at the ſame time riſe up to *a* in the Leg C *a*, C *a* being equal to C *e*. Fig. 191.

If ſuch another Canal or Syphon be ſuppoſed at BCD, the Fluid in it will come down from D to *d*, and riſe from B to *b*. And ſince ſuch Syphons may be ſuppoſed all over the Fluid ABDE; that Fluid, by the mutual Tendency of its Parts towards one another, muſt be reduced to the ſpherical Figure *a b d e*. *Q. E. D.*

Now, without conſidering the Unreaſonableneſs of the Suppoſition, let us imagine the Earth to have been an oblong Spheroid at firſt, and then to have a diurnal Revolution given to it, which ſhould by Degrees ſhorten its Axis, to bring it to what Meſſieurs *Caſſini* and *Mairan* ſuppoſe it at preſent to be. If in ſuch a Caſe the Earth be ſuppoſed fluid enough to change its Figure, by the Revolution about its Axis, why ſhould it ſtop when the Æquatorial Diameter comes to want juſt $\frac{1}{96}$ Part of the Length of the Axis? ſince two Powers act upon it to ſhorten its Axis, *viz.* Gravity, and the Centrifugal Force; the firſt of which has already been ſhewn capable to reduce it to a Sphere, and the Centrifugal Force is acknowledged by M. *Mairan* to be (as Sir *Iſaac Newton* has proved it) at the Æquator equal to $\frac{1}{289}$ Part of the Gravity there. Certainly the Alteration of Figure would not have ſtopped, before the Earth came to be a Sphere; nay, and it muſt have riſen at the Æquator; and how much, I have already ſhewn in my former Papers.

Again, if we ſuppoſe the Earth of a heterogeneous Fluid, before the diurnal Revolution, the heavieſt Parts would go towards the Center, and the lighter towards the Surface; and that Way the Terraqueous Globe would alſo become a Sphere. Then if, when the Central Parts are fixed, and the ſuperficial *Strata* are ſtill fluid, the Earth receives a diurnal Motion; it will riſe at the Æquatorial Parts, and that to a greater Height than what I have ſhewn in my former Papers, where I ſuppoſed the Earth of uniform Matter. And that ſomething like this muſt be the Caſe, appears from what Sir *Iſaac Newton* has ſaid upon this Subject. For after having ſhewn, from ſuppoſing the Earth of uniform Matter, that the Centrifugal Force of all its Parts would bring it to be $17\frac{1}{6}$ *Engliſh* Miles higher at the Æquator than at the Poles, and after having given a Table of the proportionable Decreaſe of the Length of the Degrees of a Meridian of the Earth, going from the Poles to the Æquator, in ſuch a Figure of the Earth, with the Lengths that Pendulums muſt have to ſwing Seconds in ſeveral Latitudes; from a Compariſon of the Lengths of Pendulums (obſerved by different Perſons to be ſhorter towards the Æquator, than in greater Latitudes (when they ſwing Seconds) he ſhews that the Earth muſt be $31\frac{1}{12}$ Miles higher at the Æquator than at the Poles; and therefore that it muſt be denſer towards the Central than the Superficial Parts to produce a flatted Spheroid, where the Æquatorial Diameter muſt exceed the Axis ſo much more; that is, be longer ſomething more than $\frac{1}{125}$ Part.

I am very well aware, that it may be objected by ſuch as have read M. *Mairan*'s Diſſertation, and have not read Sir *Iſaac Newton*'s *Principia*, or have not read that Book with due Attention — "That I have "not argued fairly in drawing Conſequences from a greater Gravity "at the Æquator than at the Poles, in an oblong Spheroid; becauſe "M. *Mairan* has ſhewn, that, in ſuch a Figure of the Earth, the "Gravity is greater at the Poles than at the Æquator; and that I "ſhould have drawn my Conſequences from theſe Principles." To which I anſwer, that his Demonſtrations about Gravity are built upon wrong Suppoſitions, as I ſhall ſhew by and by. Nevertheleſs, ſuppoſing that Gravity was greater towards the Poles than towards the Æquator, in the Proportion that he aſſigns, namely of the Ray of Curvature drawn into the Perpendicular to the Curve, terminated at the Axis; let us conſider what will follow from his Principles.

Fig. 192. Let us then ſuppoſe the Earth at firſt in a fluid State; A A the Axis, *d* Æ the Æquatorial Diameter, *a b* a Ray of Curvature, *d n* another, *a c* and *d* C two Lines of Tendency or Perpendiculars to the Curve, intercepted by the Axis at *c* and C; and *d* C, AC, two Tubes or Canals of the Fluid, gravitating towards, and communicating at C. I ſay that, according to M. *Mairan*'s Principles of Gravity, the Earth cannot preſerve its oblong ſpheroidical Figure. For ſince the Gravity at *a*: Is to the Gravity at *d*:: As *dn* x *d* C: to *a b* x *a c*, it will follow (from the Nature of the Ellipſe) that the Gravity at A: will be to the Gravity at *d*:: As AC^4: to dC^4: and therefore the Forces, with which the Columns of Fluid AC and *d* C tend towards C, will be as

their Maſſes drawn into the Forces driving towards C, that is, as $AC \times AC^4$ to $dC \times dC^4$. Now by the Principles of Hydroſtaticks, it is evident that the Fluid, in the Canal AC, will cauſe the Fluid in the Canal dC to run out at d as long as $AC \times AC^4$ is greater than $dC \times dC^4$: And if the Canal Cd be continued quite to δ, the Surface of the Fluid in AC will ſink to α, whilſt the Surface of the Fluid in dC riſes up to δ, in which Caſe as $\alpha C = C\delta$, the Point A will come to α, and the Point d to δ, and the Curve Ad being changed into $\alpha\delta$, the oblong Spheroid will be changed into a Sphere, the only Figure conſiſtent with the *Æquilibrium* of the fluid Parts, according to Monſ. *Mairan*'s own Principles; becauſe then you will have $AC^4 = dC^4$, and $AC \times AC^4 = dC \times dC^4$. If we make uſe of Sir *Iſaac Newton*'s Principles in this Reaſoning, we ſhall alſo ſhew, that an oblong, ſpheroidical, fluid Earth will be changed into a Sphere; but not ſo faſt as it does by Monſ. *Mairan*'s Laws; for, according to Sir *Iſaac Newton*, the Gravity at A: Is to the Gravity at d:: As $\sqrt[5]{Cd} : \sqrt[5]{AC}$. *Q. E. D.*

N. B. *Here we have ſuppoſed no diurnal Revolution, for as ſoon as that begins, the Centrifugal Force will raiſe the Æquatorial Parts, and change the Sphere into a flatted Spheroid, as has been before ſhewn, and is allowed by M.* Mairan.

Now if we ſuppoſe the ſame Figure of the Earth, but the Land (at its firſt Creation) as firm as it is now; it will in that Caſe follow from M. *Mairan*'s Principles, that the Sea muſt riſe and overflow all the Æquatorial Regions, tho' the Earth had no diurnal Revolution; and much more ſo, when the Centrifugal Force, ariſing from the diurnal Motion, helps to carry the Water the ſame Way.

Let P æ P Æ repreſent the Plane of a Meridian, P P the Axis of *Demonſtration.* the Earth (ſuppoſed an oblong Spheroid) æ Æ the Diameter of the Æquator, $d\,e\,a$ æ Part of the Surface of the Earth, æ A and e B two Fig. 193. Perpendiculars to the Surface of the Earth (which are here two Rays of Curvature) fc the Surface of the Sea, and $fdeg$, baæc two Cylinders of Sea-Water of equal Baſes and equal Heights.

Since Gravity acts on the two equal Columns of Water bacæ, $fdeg$ in the reciprocal Ratio of the Ray of Curvature (at the reſpective Places of the Columns) drawn into that Part of it which Monſ. *Mairan* calls the Line of Tendency (that is, in the Ratio of $eB \times eZ$ to æ$A \times$ æC) the Weight of fe: will be to the Weight of bæ:: As æ$A \times$ æC: to $eB \times eZ$. Therefore if there be a Communication between the fluid Columns fe and bæ, there cannot be an *Æquilibrium*, till the Quantity of Matter in fe, becomes to the Quantity of Matter in bæ, reciprocally as the Gravity at the Place æ is to the Gravity at e; and in that Caſe the Height ge will be reduced to ke, if $ke : c$æ$ :: eB \times eZ :$ æ$A \times$ æC. And conſequently the Surface of the Sea will go thro' the Points *i k b c*, where *b c* under the Æquator is higher than *i k* towards the Poles. *Q. E. D.*

N. B. *That the Centrifugal Force will ſtill add to the Height of the Sea at* b c, *is plain from what we have ſaid before. And if we apply theſe Principles to determine the different Lengths of Pendulums, ſwinging Seconds at* Paris *and at the Æquator; from the Gravity at* Paris, *compar'd to the Gravity at the Æquator (in this Suppoſition of the Action of Gravity and Figure of the Earth) a Pendulum muſt be ſhorter at the Æquator by more than* 10 *Lines, without conſidering the Centrifugal Force; and if the Centrifugal Force be taken into Conſideration, the Pendulums muſt be ſhortened near a whole Inch. But this being about five Times more than agrees with Obſervation; what proves too much, proves nothing at all.*

Having thus ſhewn, that Monſ. *Mairan*'s Account of the Action of Gravity, on ſeveral Places upon the Earth's Surface, can be of no Service for reconciling the Experiments made on Pendulums, with the Figure of the Earth deduced from M. *Caſſini*'s Meaſures: I proceed to ſhew that his Demonſtrations are founded upon wrong Principles. And firſt, in relation to Gravity.

This Gentleman has followed Sir *Iſaac Newton*, in ſaying, that Gravity increaſes in a duplicate reciprocal Proportion of the diminiſhed Diſtance from the Center of the Force, and ſo *vice verſâ*; but he has followed Sir *Iſaac Newton* no farther than ſerved his preſent Purpoſe; otherwiſe he would have known.—That in reſpect to a Central Body (as a Planet) towards which others are (*attracted* or) impelled by Gravity, this Law obtains only, as Bodies attracted, are removed from the Surface of the Planet, to greater Diſtances from the Center compared with that Diſtance; or as from greater Diſtances they approach nearer to the Planet.—That the greateſt Action of Gravity is at the Surface of the Planet.—That afterwards in advancing towards the Center, the Force of Gravity, on the Body attracted, continually grows leſs, decreaſing directly as the Diſtance; and that this holds true in a Spheroid as well as a Sphere.—That on different Parts of the Surface of the Earth (in the Condition it is now) the Gravity on Bodies is reciprocally as their Diſtance from the Center of the Earth.— That though at a conſiderable Diſtance we look upon the Earth, or any Planet, or even the Sun, as a Point (in the Center of the Forces tending towards it) endued with an abſolute Force, proportional to its Quantity of Matter; yet when we come ſo near the Body as to conſider the Space it takes up, we are to take notice, that the whole Attraction or Gravity of the Body, is made up of the Sum of the Attraction of all its Parts properly combined; and therefore, that when a Corpuſcle, or Body attracted, comes to be within the Planet, or Body attracting, the Matter above it draws it back in ſuch a Manner, that it leaves it only a Force to go on towards the Center, which is directly as the Diſtance, as we have already ſaid; juſt as if a Body concentric to the Planet (whether ſpherical or ſpheroidical) had its Surface juſt where the Corpuſcle is, and all the exterior Cruſt or Shell was annihilated.

I do not doubt but M. *Mairan* will be of this Opinion, when he has carefully and impartially examined the 12th and 13th *Sections* of the Firſt Book of Sir *Iſaac Newton*'s *Principia*, and the 18th, 19th, and 20th *Prop.* of the Third Book. And if he will be at the Pains to compare the 38th and 39th *Propoſitions* of the Third Book with the 66th of the Firſt, he will find that the Preceſſion of the Æquinoxes is owing to the broad ſpheroidical Figure of the Earth; and that if it had M. *Caſſini*'s Figure, the Æquinoctial Points would move *in Conſequentia* faſter than they do now *in Antecedentia*.

Further M. *Mairan* demonſtrates, that in an oblong Spheroid, the Diminution of Gravity, by the Centrifugal Force, encreaſes faſter in going from the Poles to the Æquator, than it would do in a Sphere, and faſter in a Sphere than it would do in a broad Spheroid; and therefore would ſhew, "That notwithſtanding the Surface of the Earth is "nearer to the Center in M. *Caſſini*'s Figure than in Sir *Iſaac Newton*'s "yet the Centrifugal Force will diminiſh the Gravity ſo faſt in going "from *Paris* to the Æquator, that the ſhortening of Pendulums, to "make them ſwing Seconds at the Æquator, may very well be accounted for that Way".

Now let us examine into this Matter, to ſee whether the Cauſe is adequate to the Effect.

If the Diſtance from the Surface of the Earth at the Pole to the Center be 96, and the Diſtance of the Surface at the Æquator be 95, the Diſtance of the Surface at *Paris*, in the Latitude of 48° 50′, will be 95,562, *&c.* by the Property of the Ellipſe. Now ſince the Force of Gravity, in different Places on the Earth's Surface, is reciprocally as the Diſtance from the Center, and the Lengths of Pendulums, that perform their Vibrations in the ſame Time, are directly as the Force of Gravity; therefore the Length of Pendulums at *Paris*, will be to their Length at the Æquator, as 95 to 95,562, *&c.* that is, as 440,555, *&c.* to 443,165, *&c.* and conſequently they muſt be lengthened 2,61, *&c.* Lines. But as from M. *Mairan*'s Principles, the Diminution of Gravity by the Centrifugal Force, is greater at the Æquator than at *Paris*, hardly $\frac{1}{440}$ Part of the whole Gravity at the Æquator, the Pendulums muſt be ſhortened in that Proportion; ſo that then the Length of a Second-Pendulum, will be $440,555 + 2,61 - 1$ Lines. But as that Quantity is greater than 440,555, *&c.* therefore the Pendulums upon the Whole muſt be lengthened: Nay, though we ſhould allow a ſhortening of two Lines; ſince by Obſervation Pendulums are found to be about two Lines ſhorter at the Æquator, the oblong ſpheroidical Figure of the Earth cannot be conſiſtent with the Experiments on Pendulums.

I beg Leave to ſet down Monſ. *Mairan*'s aforeſaid Demonſtration here; that we may ſee whether he has aſſumed true Principles.

* XI.

Propoſition V. * XI. *The Centrifugal Force at any Degree of Latitude, taken upon the oblong Spheroid, between the Æquator and the Pole, is leſs in Compariſon to the Centrifugal Force at the Æquator, than it would be at the ſame Degree of Latitude taken upon a Sphere; or, which is the ſame thing, the Centrifugal Force encreaſes more, going from the Poles towards the Æquator, upon an oblong Spheroid, than upon a perfect Sphere; and conſequently Gravity diminiſhes more, and a Pendulum muſt be more ſhortened under the Æquator, in the Hypotheſis of the oblong Spheroid, than in that of a perfect Sphere.*

Fig. 194. ' Having deſcribed an oval Curve of any Kind, as for Example, the Ellipſe ADBE abovementioned, and inſcribed the Circle DHE, whoſe Radius is DC = half the ſhorter Axis DE; upon AD take any Point as R, between the Æquator and the Pole, and from that Point to the *Evoluta* OTX draw the *Ray of Curvature* RT, which gives the *Line of Tendency* RP (*Art.* IV.) Draw likewiſe from the common Center C, to the Circumference of the Circle DH, a Radius CV, parallel to PR, and meeting the Circle at V; then from the Points R, V, draw the Lines RN, VZ, perpendicular to the Axis AB.

' It muſt be obſerved, *Firſt*, That as the Ellipſe AD repreſents a Meridian of the oblong Spheroid, the Circle DH repreſents a Meridian of a Sphere in the ſame Plane.

' *Secondly*, That the Point V, on the Circular Meridian, anſwers to the ſame Degree of Latitude as the Point R, upon the elliptical Meridian; becauſe the Lines PR, CV, being parallel to each other, and perpendicular, the one to the Ellipſe and the other to the Circle (*by Conſtruction*) the touching Planes, or Horizons of the Points R, V, will alſo be parallel.

' *Thirdly*, Whence it follows that the Diminution of the Centrifugal Force (acting againſt Gravity) on account of its Obliquity to the Horizon (*Art.* X.) of the ſame Degree of Latitude on the Elliptical and on the Circular Meridian, is the ſame in both Caſes, and in the ſame Ratio as the abſolute Centrifugal Forces repreſented by the Perpendiculars RN, VZ, (*Art.* IX.) Therefore to know whether the Centrifugal Force (whether abſolute or relative) of the Point R, upon the oblong Spheroid ADBE, be leſs or greater in reſpect to, the Centrifugal Force under the common Æquator DE, than the Centrifugal Force (whether abſolute or relative) of the correſpondent Point V upon the Sphere; nothing more is required than to ſee which is the longeſt of the two Perpendiculars, namely, RN in the oblong Spheroid, or VZ in the Sphere; ſince theſe two Lines expreſs the Radii of the Circles of Revolution, and conſequently the abſolute Quantity of the Centrifugal Forces.

' *4thly and laſtly*, That the Ratio of the Centrifugal Forces of two correſpondent Points upon the oblong Spheroid ADBE, and the in-

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* *See Monſ.* Mairan's *Diſſertation, Article* XI. &c.

'ſcribed Sphere DHE, to the Centrifugal Force of their Æquators is the ſame, ſuppoſing the Sphere of any other Bigneſs; and that it has been determined here of the Diameter DE, only to render the Demonſtration eaſier, by giving the ſame Conſequent to the Antecedents RN and VZ. For if about the Center C and with the Radius C *d*, the Circle *d b e* be deſcribed equal (for Example) to a Meridian of a Sphere of the ſame Solidity as the oblong Spheroid ADBE; and the Radius CV be produced till it meet the Circle *d b* at the Point *u*, and *u z* be let fall perpendicular to the common Axis of Revolution, and parallel to VZ: It is plain, that we ſhall always have VZ: DC :: *u z* : *d* C, or $\frac{VZ}{DC} = \frac{uz}{dC}$, and conſequently $\frac{RN}{DC}$ will have the ſame Ratio to $\frac{VZ}{DC}$ as to $\frac{uz}{dC}$.

'Therefore, in order to demonſtrate that the Centrifugal Force of a Point, taken in any Latitude upon the oblong Spheroid, is leſs when compared to the Centrifugal Force of the like Point, taken upon a Sphere in reſpect to the Centrifugal Force at the Æquator; there is nothing more required than to ſhew that RN $<$ VZ, becauſe by that Means we ſhall have $\frac{RN}{DC} < \frac{VZ}{DC}$.

'This being obſerved; from the Point R, draw the Line RI, parallel to the Axis AB, and meeting the Circle DH at K, and the Diameter DE of the Æquator at the Point I. From the Point K having let fall the Perpendicular KL $=$ RN, upon the Axis AB, and drawn KC to the Center C; the Queſtion will be brought to this, *viz.* To know whether the Point V coincides with the Point K; or whether it is above it towards D, or below towards H.

'But CK $=$ CV $=$ CD $>$ PR (*Art* VIII.) therefore CK and PR being both between the Parallels AC, RI, the greateſt CK is more inclined to them than the leaſt PR, and the Angle KCA is leſs than the Angle RPA $=$ VCA. And ſince theſe two Angles have each of them one of their Sides coinciding with the Line AC, namely, the Side AP of the Angle RPA, and the Side AC of the Angle KCA; it follows that the Side VC of the Angle VCA $=$ RPA $>$ KCA, will go above CK between CK and CD, and meet the Line RI at the Point G, between K and I, and the Circle DH at the Point V, which conſequently will be above RI, between K and D. Therefore CV $=$ CG $+$ GV is $=$ PR $+$ GV, and conſequently VZ, which meets RI at the Point F, is $=$ ZF $+$ FV $=$ RN $+$ FV; and therefore RN $=$ VZ — FV. Therefore RN $<$ VZ.

'And becauſe the ſame Thing may be demonſtrated in reſpect of any other Point, taken between the Æquator and the Pole; and that

‘ Gravity, and conſequently the Length of a Pendulum diminiſhes, as the Centrifugal Force encreaſes. Therefore, *&c. Q. E. D.*

Corollary.

‘ XII. From what has been demonſtrated, and from *Prop.* 3. *Art.* VIII. it follows, that the Perpendicular which is drawn from any Point of an oval Meridian to the Axis, will be ſo much ſhorter, in Compariſon to the Perpendicular drawn from the correſpondent Point of an inſcribed circular Meridian, as the Latitude is greater; and conſequently (by *Art.* XI. *Num.* 3.) the Centrifugal Force will be ſo much the leſs, and Gravity ſo much the greater, upon the oblong Spheroid, in reſpect to the Centrifugal Force, and the Gravity under the Æquator.

‘ For as the Line RP does always decreaſe, as the Point R is taken nearer to the Pole A, it is evident, that the Angle VCK will continually encreaſe, in reſpect to the Angles VCA, KCA, as it is their Difference, and conſequently that the Perpendicular VZ will be ſo much greater than the Perpendicular KL=RN.

I paſs over the Demonſtration of the latter Part of his Propoſition abovementioned, which he deduces juſtly from his Conſtruction, if what he ſays (*Num.* 2.) be right; becauſe in ſuch a Caſe it cannot be called in Queſtion; and proceed to an Obſervation that he makes afterwards, *viz. We muſt take care to obſerve in the foregoing Propoſitions and Corollaries, that the Compariſon is always made between two ſimilar Points of Latitude, taken upon the two Spheroids, or upon one of the Spheroids and the Sphere, between the Æquator and the Poles, in reſpect to the Centrifugal Force upon the Æquator of any one of theſe Spheroids, or of the Sphere. For if we only compared abſolutely the Centrifugal Force of a Point of the Æquator of the one, to the Centrifugal Force of a correſpondent Point of the Æquator of the other, it is plain that it would be greater upon a flatted Spheroid, than upon a Sphere, or than upon an oblong Spheroid of the ſame Solidity, in the Ratio of the great Axis of the generating Ellipſe of the flatted Spheroid, to the Diameter of the Sphere, or to the ſhorter Axis of the generating Ellipſe of the oblong Spheroid. And in all Likelihood, this muſt be the Reaſon that has made others, who have treated of this Subject, to imagine the very contrary of what I have demonſtrated.*

As M. *Mairan* conſiders the Earth at Reſt, in the Conſtruction for his Demonſtration above quoted, and afterwards obſerves what Effect the Centrifugal Force will have upon Bodies on its Surface, to diminiſh the Gravity, with which they endeavour to deſcend in their Line of Tendency RP: He ſhould not only have taken Notice (as he has done) that the whole Centrifugal Force NR is not to be ſubſtracted from the Gravity at R, as the whole Centrifugal Force CD is to be ſubſtracted from the whole Gravity at D, becauſe of the Obliquity of RN to PR; but he ſhould have obſerved alſo, that the Obliquity of the Plane of the Parallel NR, in which the Centrifugal Force acts, muſt alter the Line of Tendency RP, and change the Direction RP into RW, ſomewhere between the Point P and the Center C; for if there be a heavy Body as a Plummet, hanging by a Thread in the Line SR, or SP, the Line of Tendency which has been ſuppoſed perpendicular to the Curve ARD, without taking in the Effect of the Centrifugal Force; as ſoon as the Spheroid revolves about its Axis, the Body which would fall in the Line SR, acted upon only by one Force, namely, that of Gravity, will now be acted upon by another Force, at the ſame Time puſhing it in the Line S *s* (which is the ſame as R *r*) and conſequently will move in the Line S *r*, diagonal of the Parallelogram *s* S R *r*; or, which is all one, a Body placed at R will have its Line of Tendency in RW, as I have already ſhewn in my firſt Diſſertation on this Subject; only I did not ſuppoſe the Earth a Spheroid before the diurnal Motion, and therefore made uſe of the Line ZV inſtead of the Line NR; ſo that it may be objected that the Angle *r* S R will not be ſo great in a Spheroid as in a Sphere, becauſe the Centrifugal Force which acts with the ſame Obliquity (ſince NRP=ZVC) is as much leſs in the Spheroid as NR is leſs than ZV: But I was aware of that, and therefore made the Angle R S *r* only of 5 Minutes, when it really appears to be of almoſt ſix Minutes, when the Earth is ſuppoſed ſpherical; and therefore, without coming to give the exact Quantity of the ſaid Angle, one may eaſily perceive, that Monſ. *Caſſini*'s Difference of the Axis and the Æquatorial Diameter will produce a Figure, in which the Angle R S *r*, will not be leſs than of 5 Minutes.

Such an Obliquity, cauſed in the Direction of Gravity, will render the oblong ſpheroidical Figure of the Earth impoſſible, becauſe then Fluids would not have the Lines of their Gravity perpendicular to the Horizons of the Places where they are (ſuppoſing the Horizons of Places to be Planes touching the Curve of the Earth in thoſe Places) and Plumb Lines would be ſo far out of the Perpendicular to Lines of Level, as to make an Angle eaſy to be obſerved, as I have ſhewn in my former Papers.

But if the ſame Cauſe be ſuppoſed to act upon the Sea to make it level, as makes heavy Bodies to fall (which certainly muſt) then indeed Lines of Level will be perpendicular to Plumb Lines, and the Level of the Sea, taken always for the Horizon of a Place, will not be a Plane touching the Earth, but cutting it towards the Poles, and conſequently the Water will be carried towards the Æquator, as was before ſhewn.

Beſides, the Difference of the Action of the Centrifugal Force would not be ſo great between correſpondent Points of the ſame Latitude in the Spheroid and in the Sphere; for when the Line of Tendency RP is by the Centrifugal Force changed into RW, the Point R upon the Spheroid does no longer correſpond in Latitude with the Point V upon the Sphere, but muſt be taken nearer to V, ſo that the Line RW may become parallel to VC, and RWA=VCA.

If it be alledged here, that M. *Mairan* ſuppoſes the Earth in Motion, and takes in the Effect of the Centrifugal Force, when he makes the Line of Tendency to be RP; I anſwer, that if he had conſidered the Earth as revolving upon its Axis, he would not have made VC the Line of Tendency of a ſpherical Earth in Motion, ſince it is the Line of Tendency of ſuch an Earth at Reſt.

In M. *Mairan*'s Obſervation abovementioned, he ſays, ' that we ' are not to compare the Centrifugal Force at the Æquator of an ob- ' long Spheroid, with the Centrifugal Force at the Æquator of a ' Sphere, or at the Æquator of a flatted Spheroid of the ſame Solidi- ' ty; allowing that then it would be greater in the Sphere, and ſtill ' greater in the flatted Spheroid: but only the Centrifugal Forces in ' ſeveral Latitudes upon the ſame Figure."—But I beg Leave to differ from him for the following Reaſons.

1. Becauſe the Force of Gravity is not the ſame at the Æquator of the flatted Spheroid, as it is at the Æquator of the Sphere, or as it is at the Æquator of the oblong Spheroid.

2. Becauſe it is not the ſame in different Latitudes, in either of the Spheroids. (See Sir *Iſaac Newton* Lib. 3. Prop. 19 and 20.) And Monſ. *Mairan*'s Way of arguing will only ſerve, in Caſe the Gravity ſhould be the ſame in all the Points of the Surface of the Earth in his Figure, and alſo in the two other Figures.

For Example, let the uniform Gravity be called g; and,

1. Let the Centrifugal Force at the Æquator of the flatted Spheroid be called $c+2$; and the Centrifugal Force in any Latitude, as for Example, the Latitude of *Paris* (as it is diminiſhed on Account of a ſhorter Coſine of Latitude, and likewiſe on Account of its Obliquity to the Line of Tendency) be called $c+2-l$; the Difference of the Diminution of Gravity at *Paris*, and at the Æquator will be $\overparen{g-\overline{c+2}}-\overparen{g-\overline{c+2-l}}=l$.

2. Let the Centrifugal Force at the Æquator of the Sphere be call'd $c+1$, and the Centrifugal Force at the Latitude of *Paris* be called $c+1-\overline{l+m}$; the Difference of the Diminution of Gravity at *Paris*, and at the Æquator in a ſpherical Earth, will be

$$\overparen{g-\overline{c+1}}-\overparen{g-\overparen{c+1-1-\overline{l+m}}}=l+m.$$

3. Let the Centrifugal Force at the Æquator of the oblong Spheroid be called c, and the Centrifugal Force at *Paris* be called $c-\overline{l+m+n}$; the Difference of the Diminution of Gravity at *Paris*, and at the Æquator, in an oblong ſpheroidical Earth, will be

$$g-c-\overparen{g-\overparen{c-\overline{l+m+n}}}=l+m+n.$$

Now, if Gravity ſhould in every Caſe be equal to g, it is evident, that the ſhortening of Pendulums, at the Æquator, would be greater in the oblong Spheroid, than in the Sphere, or in the flatted Spheroid; becauſe as the Lengths of Pendulums diminiſh with the Gravity, thoſe Lengths will be at *Paris* and at the Æquator, when compared, as $g-\overline{c+2-l}$ to $g-\overline{c+2}$ in the flatted Spheroid; as $g-\overparen{c+1-\overline{l+m}}$ to $g-\overline{c+1}$ in the Sphere, and as $g-\overparen{c-\overline{l+m+n}}$ to

$g-c$ in the oblong Spheroid; and conſequently from what M. *Mairan* has demonſtrated this Ratio of $g-\overline{c-\overline{l+m+n}}$ to $g-c$, being greater than either of the others, the Pendulums muſt be ſhortened in the oblong Spheroid.

But as the Force of Gravity is leſs at the Æquator of the flatted Spheroid, than at the Æquator of the Sphere, or of the oblong Spheroid of the ſame Solidity; let us expreſs its Quantity in the three Caſes by $g-s$, g, and $g+s$, and we ſhall then find the Lengths of the Pendulums, at the Æquator of the three Solids, as $g-s-\overline{c+2}$, $g-\overline{c+1}$, and $g+s-c$; and conſequently the Lengths of Pendulums will be greateſt at the Æquator of the oblong Spheroid, becauſe $g+s-c$ is the greateſt Quantity.

Laſtly, To compare the Lengths of Pendulums at the Æquator of the oblong Spheroid, thus found, with their Lengths at the Latitude of *Paris* upon the ſaid Spheroid—Let us expreſs the Exceſs of Gravity at the Æquator, whereby it is greater than at *Paris* (becauſe in this Figure, *Paris* is farther from the Center of the Earth, than the Æquator, by $\frac{1}{190}$ Part) by the Letter s, and the Exceſs of the Centrifugal Force at the Æquator, above that Part of it which acts directly againſt Gravity at *Paris*, by $l+m+n$, the Gravity at *Paris* by g, and the Centrifugal Force at the Æquator by c; then $g+s-c$ will ſtill repreſent the diminiſhed Gravity, and anſwer to the Length of Pendulums at the Æquator, whilſt $g-\overline{c-\overline{l+m+n}}$ or $g-c+l+m+n$ repreſents the diminiſhed Gravity, and conſequently the Length of Pendulums at *Paris*. If s be equal to $l+m+n$, Pendulums will be as long at the Æquator as at *Paris*; and if s be greater than $l+m+n$, Pendulums will be longer at the Æquator. But making all poſſible Allowance, in Favour of Monſ. *Mairan*'s Hypotheſis, no Calculation will bring $l+m+n$ to be greater than, or ever equal to s. Therefore Monſ. *Mairan*'s Demonſtrations, abovementioned, are of no Force to prove the Earth to be an oblong Spheroid.

And now, I think, I have anſwered all that relates to the Figure of the Earth in Monſ. Mairan'*s Diſſertation; in ſhewing, That his Conjectures can neither be ſupported by thoſe Phyſical Principles which Sir* Iſaac Newton *has Mathematically deduced from unqueſtioned Obſervations and Experiments accurately made; nor even by thoſe Principles which M.* Mairan *has aſſumed to ſerve his intended Purpoſe—That his Demonſtrations, relating to the Difference of the Action of the Centrifugal Force, are of no Service to him, for reconciling the Experiments made on Pendulums, with Monſ.* Caſſini'*s Meaſures;—becauſe, when applied to Sir* Iſaac Newton'*s Principles, they will make Pendulums longer at the Æquator than at* Paris, *and when applied to Monſ.* Mairan'*s own Principles, they will make them a whole Inch ſhorter at the Æquator than at* Paris, *contrary to all Obſervations, which, at a Medium, make Pendulums but about two Lines or* $\frac{166}{1000}$ *of an Inch longer at the Æquator than at* Paris.—

ris.—*That he has built his Demonſtrations upon a wrong Notion of Gravity—And that he has not conſidered what is moſt material in the Effect of the Centrifugal Force, acting on Bodies deſcending by their Gravity, between the Æquator and the Poles, namely, the Alteration of their Line of Direction, which would make them fall out of the Perpendicular towards the Æquator.*

I ſhall add one more Philoſophical Argument, given me by a Friend, to whom I communicated my Thoughts on this Subject; becauſe it is wholly independent on thoſe Principles of Philoſophy, concerning which, ſome of the Gentlemen that believe the oblong ſpheroidical Figure of the Earth, and the *Engliſh* Philoſophers, are not yet agreed; and it is this.

If the Earth was of an oblong ſpheroidical Figure, higher at the Poles than the Æquator; the Axis of its Revolution, would either go thro' one of its ſhort Diameters, or be continually changing unleſs the ſaid Axis did exactly coincide with the Axis of the Figure.

Demonſtration.

Fig. 195.

Suppoſe ſuch an oblong Figure as A *a* fixed to the Axis P *p* at the Center C, but capable of moving freely round it towards P or towards *p*, yet ſo as to be obliged to move with the Axis, when it is turned round. Suppoſe now the Poles P and *p* to be fixed, and the Body, thus conſtituted, to be turned ſwiftly round the Axis P *p*; then if the Angle ACP be oblique, and the Figure AD *a* E be oblong, the Parts AC and C *a* will acquire a Centrifugal Force, which will enlarge the Angle *p* C A, till it comes to be a right one. Beſides this, a Velocity will be generated in the Motion, while A is going towards the Perpendicular *a* C, which will make it go further on towards P, as to B, with a Motion which will after that be retarded, till the Centrifugal Force has Strength enough to ſend it back again the contrary Way; and ſo it will move continually with a reciprocal Motion, like the Oſcillation of a Pendulum; and if a little of this Motion be loſt at every Oſcillation, then the oblong Figure AD *a* E will at laſt move quietly about its leſſer Axis DE coinciding with P *p*.

If A a *did not at firſt exactly coincide with P* p, *the Centrifugal Force will have the abovementioned Effect; and that this is not the Caſe in the Earth is more than probable, becauſe the unequal Diſtribution of Sea and Land, beſides the* Phænomena *of the Tides muſt make the Axis of its Gravity, and conſequently the Axis of its Revolution, to differ from the Axis of the oblong Spheroid, if the Earth had ſuch a Figure; without conſidering that every Earthquake would alter ſo nice an* Æquilibrium, *which once loſt, would never be recovered again.*

To leave nothing unexamined, relating to the Controverſy, I have again conſidered the Meaſures and Obſervations, mentioned in the Account of the Meridian drawn thro' *France*, in the Memoirs of the *Royal Academy*, for the Year 1720; and I find them to want a great deal of the Accuracy required in ſo nice a Point, as determining the different Lengths of Degrees upon the Surface of the Earth. To prove my Aſſertion, I beg that the Reader will examine the following Tables, whereby it appears, that if any thing certain can be deduc'd from the ſaid Obſervations and Meaſures (either taken as they are, or reduc'd to the Level of the Sea, by the Rules given by Monſ. *Caſſini* *) it will be in Favour of Sir *Iſaac Newton*'s Figure of the Earth, rather than theirs.

In the following Table, the firſt Column gives the Names of Places; the ſecond, the Diſtances from *Paris*, according to the Meaſures taken by the *French* Gentlemen; the third, the Latitudes, ſuch as the meaſured Diſtances will give them, ſuppoſing the Earth ſpherical; the fifth, the Differences between theſe and the Latitudes obſerved, expreſſed in Seconds of a Degree, where when the Latitude computed, exceeds the Latitude obſerved, the Letter N (North) ſhews that Difference to be in Favour of M. *Caſſini*'s Figure, and the contrary Difference marked by the Letter S (South) is in Favour of Sir *Iſaac Newton*'s Figure.

Names of Places. I.	Diſtances from *Paris* meaſur'd. II. Toiſes.	Latitudes obſerved. III.	Latitudes in a ſpherical Earth computed from the meaſured Diſtances. IV.	Differences in Seconds. V.
Dunkirk.	125552	51° 2′ 25″½	51° 2′ 25″½	0″
Amiens.	60370	49 53 48	49 53 48	0″
Sourdon.	49970½	49 42 42	49 42 52,1	10,1 N
Paris.		48 50 10	48 50 20,3	10,3 N
Malvoiſine	18838	48 30 47	48 30 32,1	14,9 S
Vouſon.	67962	47 39 17	47 38 53,6	23,4 S
Bourges.	100192	47 4 31	47 04 58,7	27,7 N
S. Sauvier.	139934	46 23 24	46 23 12	12,0 S
Croc.	169540	45 51 43	45 52 4,6	21,6 N
Bort.	196484	45 23 27	45 23 45,2	18,2 N
Aurillac.	223606	44 55 13	44 55 14,5	1,5 N
Rodès.	256575	44 20 54	44 20 35,1	19,9 S
Alby.	280612	43 55 32	43 55 19	13,0 S
Carcaſſone.	321430	43 12 56	43 12 24,5	31,5 S
Collioure.	360604	42 31 13⅔	42 31 13,8	0.

In this Table it is to be obſerved that there is an equal Number of Differences marked N and S, and if the Differences on each Side be added together, there will be 89″,4 on the North Side, and 114″,7 on the South: This laſt agrees beſt with Sir *Iſaac Newton*'s Figure, which muſt be ſuppoſed for the Correction of ſo great a Difference.

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* *Memoirs for the Year* 1720. Vol. I. P. 1. Ch. 13.

In the next Table, the firſt Column gives the Names of Places; the ſecond, the Latitudes obſerved; the third, the Diſtances in the Meridian from *Paris*, reduced to the Level of the Sea; the fourth, the Differences of the ſecond Column expreſs'd in Seconds of a Degree; the fifth, the Differences of the Numbers in the third Column; and the ſixth, the Meaſure of a Degree by the fourth and fifth Columns compared.

I. Names of Places.	II.			III. Toiſes.	IV. Second of a Degree.	V. Toiſes.	VI. Toiſes.
Dunkirk.	51°	2′	19″	125454			
Amiens.	49	53	56	60444	4103″	65010	57040
Clermont.	49	22	57	31028	1859	29416	56965
The R. Obſervatory.	48	50	10	0	1967	31028	56787
Vouſon.	47	39	17	67959	4253	67959	57525
☞ S. Sauvier.	46	23	24	139237	4553	71978	56912
Croc.	45	51	43	169539	1901	29602	56058
Bort.	45	23	46	196480	1677	26941	57834
Aurillac.	44	55	13	223616	1713	27136	57028
Rodès.	44	20	53	256474	2060	32858	57422
Alby.	43	55	32	280612	1521	24138	57131
Carcaſſone.	43	12	55	321430	2557	40818	57468
Collioure.	42	31	13	360614	2502	39184	56380

In this Table in the third Column, over-againſt St. *Sauvier*, the Number which was 139944 is corrected to make it 139937, to the Advantage of the oblong Figure. In the ſixth Column, the Numbers appear ſo irregular, as to be unfit to decide this Controverſy. Then if a Compariſon be made between *Dunkirk*, St. *Sauvier* (which is very near the Middle of *France*, and almoſt in the Meridian of *Paris*) and *Collioure*, the Meaſurement is abſolutely in Favour of Sir *Iſaac Newton*'s Theory; the mean Degree between *Dunkirk* and St. *Sauvier* being larger by about 64 Toiſes, than between St. *Sauvier* and *Collioure*; and to reduce them even to an Equality, there muſt be a greater Alteration made in the Situation of thoſe three Places, than it is reaſonable to ſuppoſe their Obſervations to be capable of admitting. Here follows the Compariſon.

Dunkirk and *Collioure*	A mean Degree is	57061
Dunkirk and *Paris*		56960
Paris and *Collioure*		57097
Dunkirk and *S. Sauvier*		57090,4
S Sauvier and *Collioure*		57026,5
According to M. *Picard*,		57060

To conclude, I will propoſe a Method of obſerving the Figure of the Shadow of the Earth in Lunar Eclipſes, whereby the Differences between the Diameters in the oblong ſpheroidical Figure, if there be ſuch an one as M. *Caſſini* affirms (*viz.* of 96 to 95) may be diſcovered.

Let PÆPÆ repreſent the Earth, ſeen from the Sun at the Time of the Summer Solſtice; it is evident, that the ſame Figure will expreſs the Section of the Earth's Shadow at the Moon's Diſtance, as ſeen from the Earth. If EE repreſents the Ecliptick, Æ Æ will be the ſhorteſt Diameter of the Section; and if L L be taken for the Moon's Way, in a total and central Eclipſe of the Moon, by obſerving the Time which is ſpent in the Paſſage of the Center of the Moon, thro' the Shadow, and reducing that Time to Seconds of a Degree of a great Circle of the Heavens, we ſhall have the leaſt Diameter of the Shadow. Fig. 196.

Again, let the ſame Letters repreſent the ſame Things, only here the Section of the Shadow is ſuch, as the Earth will caſt at the Æquinox, and the Eclipſe of the Moon is here ſuppoſed partial, its Center juſt touching the Shadow. When the Moon's Center is got to *c*, if the Latitude of its Center or its Diſtance from the Ecliptick be obſerved, we ſhall have the Length *c* C nearly equal to the longeſt Semi-diameter of the Shadow. Fig. 197.

Now, comparing *c* C in this Figure to LC in the former (the Difference between *c* C and CP (Fig. 197.) and between CL and CÆ (Fig. 196.) not being worth Notice) they ought to be to one another, as 96 to 95, which in ſuch a Shadow will give a Difference of about 25″ at a Medium, ſenſible enough to be obſerved, notwithſtanding the *Penumbra.* If therefore thoſe Aſtronomers who have Inſtruments nice enough, and ſufficient Skill in the Management of them, to take Angles to 3 or 4 Seconds of a Degree, will obſerve what I have been mentioning in total and partial Eclipſes of the Moon; by ſuch Obſervations they will eaſily convince us, that the Figure of the Earth is ſuch as Monſ. *Caſſini* ſuppoſes it, or convince him that he has been miſtaken.

The Semi-diameter of the Earth's Shadow, when the Earth is in Perihelio, *and the Moon in* Apogæo *is* 38′, *or* 2280″, *without conſidering the Encreaſe of the Shadow, on account of the Atmoſphere of the Earth, which would make it* 39′ or 2340″ *(allowing one Second for a Mile;) and the Semi-diameter of the Shadow, when the Earth is in* Aphelio, *and the Moon in* Perigæo *is* 46′, 20″, *or* 2780″, *which encreaſed on account* *of the Atmoſphere of the Earth, will bring it to* 47′, 20″ *or* 2840″. *Now if the Proportion of* 95 *to* 96 *be taken in both Caſes you will have theſe Analogies,* $\left\{\begin{array}{l}95:96::2340'':2364''\ 6\\ 95:96::2840'':2869'',8\end{array}\right\}$ *So that* 2364″,6—2340″ =24″,6 *will be the Difference of the Semi-diameters, when the Section of the Shadow is the leaſt, and* 2869″,8—2840″=29″,8 *will be the Difference of Semi-diameters, when the Section of the Shadow is the greateſt; the Sum of thoſe Differences* 24″,6+29″,8 *halved, will give the Difference, when the Section of the Shadow is at a Medium* =27″,4; *from which if we take* 2″,4 *becauſe in* Fig. 197. *C c is a little leſs than* C P, *and in* Fig. 196. LC *is ſomething greater than* ÆC, *we ſhall have* C*c in* Fig. 197. *to compare with* LC *in* Fig. 196. *which will exceed it by* 25″, *if Monſ.* Caſſini'*s Figure of the Earth be the true one.*

An Experiment to illuſtrate what has been ſaid in the foregoing Papers, by the ſame n. 389. 344.

4. Upon an Axis of Iron, that could be made to turn ſwiftly (by means of a Wheel, whoſe String went round a Pulley fixed to the ſaid Axis) I ſlipped on two Iron Hoops, whoſe Planes interſected at right Angles, repreſenting two Colures, which, being of a ſpring Temper, ſprung in ſuch Manner as to be $\frac{1}{96}$ Part longer in that Diameter that coincided with the Axis, than in their Æquatorial Diameter; this Proportion being the ſame that M. *Caſſini* ſuppoſes to be between the Axis and Æquatorial Diameter of the Earth. Two circular Plates, to which the ſaid Hoops were riveted, had ſquare Holes, thro' which the Axis paſs'd, ſo that the two Poles of the oblong Spheroid, which the Hoops deſcribe in their Revolution, might approach together in ſuch Manner, as to let them put on the Form of a true Sphere, when, by the Whirling, the Æquatorial Diameter of the Machine ſwelled and over-powered the Elaſticity of the Hoops. A greater Degree of Swiftneſs turned the Sphere into an oblate Spheroid of Sir *Iſaac Newton*'s Figure. A Velocity ſtill greater makes the Diſproportion of the Diameters, ſuch as thoſe of *Jupiter*; and ſtill the Æquatorial Diameter encreaſes with the Centrifugal Force.

Another Hoop with a Catch, repreſenting the Æquator, ſhews (in the Experiment) the Increaſe of the Æquatorial Circumference, and an Index applied to the Frame, ſhews the Increaſe of the Diameter.

A Method for determining the Geographical Longitude of Places, from the Appearance of Falling Stars, *by* George Lynn, *Eſq;* n. 400. p. 351.

II. Upon peruſing the Account which Dr. *Halley* has given in the *Tranſactions*, N° 360, of that extraordinary Meteor which appeared all over *England*, *March* 19, $171\frac{8}{9}$. I obſerve one very great Uſe he ſuggeſts might be made of thoſe momentaneous *Phænomena*, in determining the Geographical Longitude of Places, if we could but have the leaſt Notice of their appearing, *&c.*

I cannot but think, that ſome other Meteors which are very frequent, tho' little taken Notice of, might ſerve very well for the ſame Purpoſe. I mean thoſe which are vulgarly called *Stars ſhooting*, or *falling*, being a Sort of natural *Sky-rockets* diſcharged at a very great Height, as I cannot but imagine from this Circumſtance, that they never appear, any of them, according to the beſt of my Obſervation, where the Sky is cloudy; and therefore, in all Probability, their Exploſion is in the Regions far above the Clouds, and they themſelves of the ſame Nature with (tho' perhaps leſs, and much lower than) that great Meteor above-mentioned, whoſe Height Dr. *Halley* computes to have been above 60 Geographical Miles, *viz.* much above the reputed Limits of our Atmoſphere. But ſuppoſing theſe I mention to be diſcharged only at 20 or 30 Miles high, they may be ſeen by different Obſervers at the ſame Moment of abſolute Time, in very diſtant Places from one another, which is the Thing required. For, if in any two Places, as the Doctor takes Notice, any two Obſervers, by Help of Pendulum Clocks duly corrected by celeſtial Obſervations, do exactly note at what Hour, Minute, and Second, ſuch a Meteor is diſcharged, the Difference of thoſe Times will be the Difference of Longitude of the two Places; nor does it require ſo much as the Uſe of a Teleſcope, as in the Methods hitherto put in Practice for that Purpoſe. Now theſe natural *Rockets* I have found to be very frequent in every Star-light Night; but eſpecially after a ſtormy Day, or in a ſtormy Night. If, therefore, Perſons who are prepared, as above, to be exact in their Time, and alſo have a moderate Knowledge of the ſeveral Conſtellations, ſo as to deſcribe the Track of any of thoſe Meteors amongſt the Stars, would but beſtow any determinate Hour to be agreed amongſt them, as for Inſtance, from 8 to 9 each ſuch Night, to watch and obſerve their Exploſions, noting down immediately the Time and Track of them, it would be eaſy to determine, upon comparing their Obſervations, which of theſe Exploſions each of them ſee at the ſame Time; and thereby the Difference in Longitude of thoſe Places would be exactly had, as above. It would however be worth the while, this Way to try whether ſuch common Meteors are diſcharged, at any conſiderable Height above the Clouds, and how far, and whether they differ much from one another in their Heights.

A Propoſal of a Method for finding the Longitude *at* Sea *within a Degree, or twenty Leagues. By* Edmund Halley, L.L.D V. P. R. S. n. 421. p. 185.

III. It is now above twenty Years ſince I added an *Appendix* to the ſecond Edition of Mr. *Street*'s *Caroline Tables*, containing a Set of Obſervations I had made in the Years 1683 and 1684, for aſcertaining the Moon's Motion; and giving a *Specimen* of what I thought, at that Time, might be the only practicable Method of attaining the Longitude at Sea. What I printed ſo long ago, is as follows:

‘The Advantages of the Art of finding the Longitude at Sea, are too evident to need any Arguments to prove them. And having by my own Experience found the Impracticability of all other Methods propoſed for that Purpoſe, but that derived from a perfect Knowledge of the Moon's Motion; I was ambitious, if poſſible, to overcome the Difficulties that attend the Diſcovery thereof.

‘And firſt, I had found it only needed a little Practice to be able to manage a five or ſix Foot Teleſcope, capable of ſhewing the Appulſes or Occultations of the *Fixed Stars* by the *Moon*, on Ship-board, in moderate Weather; eſpecially in the firſt and laſt *Quarters* of the *Moon's Age*, when her weaker Light does not ſo much efface that of the *Stars*. Whereas the Eclipſes of the *Satellites* of *Jupiter*, how proper ſoever for *Geographical* Purpoſes, were abſolutely unfit at Sea, as requiring Teleſcopes of a greater Length than can well be directed in the rolling Motion of a Ship in the Ocean.

‘Now the Motion of the Moon being ſo ſwift, as to afford us ſcarce ever leſs than two Minutes for each Degree of Longitude, and ſometimes two and a half; it is evident, that were we able perfectly to predict the true Time of the Appulſe or Occultation of a Fix'd Star, in any known Meridian, we might, by comparing therewith the Time obſerved on Board a Ship at Sea, conclude ſafely how much the Ship is to the Eaſtward or Weſtward of the *Meridian* of our *Calculus*.

‘But after much Examination, and carefully collating the *Caroline Tables of Mr*. T. Street (though generally better than thoſe that went before him) as likewiſe thoſe of *Tycho*, *Kepler*, *Bullialdus*, and our *Horrox*, with many accurate Obſervations of the *Moon*, carefully made on Land; it does not appear that any of theſe *Tables* do repreſent the Motions with the Certainty required; and tho' many times the Agreement ſeems ſurprizing, when the Errors of the ſeveral *Equations* compenſate one another; yet in thoſe Parts of the *Orb* where they all fall the ſame Way, the Fault is intolerable, and the Reſult many times not to be depended on, to more than one hundred Leagues; that is to ſay, it is wholly inſufficient.

‘Yet ſtill this Fault is *Artificis*, not *Artis:* For obſerving the *Period* of the *Lunar Inequalities*, which is performed in eighteen Years and eleven Days, or two hundred and twenty-three *Lunations*; it is found that the Returns of the Eclipſes, and other Phænomena of the Moon's Motion, are very regularly performed; ſo that whatever Error you found in a former Period, the ſame is again repeated in a ſecond, under the like Circumſtances of the ſame Diſtance of the Moon from the Sun and *Apogæon*.

‘Thus, from the Obſervation made of the Eclipſe of the Sun, which was *June* 22, 1666, in the Morning, ſeen at *London* and *Dantzick*, I was enabled to predict, with great Certainty, that other, which I obſerved *July* 2, 1684, by allowing the ſame Error I found in the *Calculus* of the former. And the like with equal Certainty will do, in the Caſes *extra Syzygias*, when the Mean and Synodical Anomalies are nearly the ſame, about the ſame Time of the Year.

‘Being thus aſſured, from the Certainty of theſe Revolutions, that all the intermediate Errors of our *Tables* were not uncertain Wandrings, but regular Faults of the *Theories*; I next thought how I might beſt be informed of the Quantity and Places of theſe Defects: That being apprized how much, and which Way my Numbers erred, I might apply the Difference, ſo as at all times to repreſent the true Motion of the *Moon.* Nor was there any other Way, but from the Heavens themſelves, to derive this Correction, by a ſedulous and continued *Series* of *Obſervations*, to be collated with the *Calculus*, and the Errors noted in an *Abacus:* From whence, at all Times, under the like Situation of the *Sun* and *Moon*, I might take out the Correction to be allowed.

'And having by me the *Sextant* I made to obſerve the Southern *Stars* at St. *Helena*, in the Year 1677, I fixed it for this Purpoſe; reſolving to have continued to obſerve, till I had filled my *Abacus*, ſo as it might have the Effect of exact *Lunar Tables*, capable to ſerve at Sea, for finding the *Longitude* with the deſired *Certainty*.

'With this Deſign, I applied the Leiſure I had procured myſelf about the Year 1683, to obſerve diligently, as often as the Heavens would permit, the true Place of the Moon, eſpecially as to Longitude; and in the Space of about ſixteen Months I had gotten near two hundred ſeveral Days Obſervations, moſt of which I collated with the *Horroxian Theory* (whoſe *Calculus* is ſomething more compendious than that of Mr. *Street*) and having placed the Errors in an *Abacus*, I perceived how regular the Irregularities were, and that where the Moon had been exactly obſerved formerly, at the Diſtance of one or more Periods of two hundred twenty three Months, I could even predict the Error of the *Tables*, with a Certainty not much inferior to that of the Obſervations themſelves. But this Deſign of mine was ſoon interrupted by unforeſeen domeſtick Occaſions, which obliged me to poſtpone all other Conſiderations to that of the Defence of my Patrimony: And, ſince then, my frequent Avocations have not permitted me to re-aſſume theſe Thoughts.

'In the mean time I have taken Care to preſent my Obſervations, ſuch as they are, to the Publick, in order to preſerve them; aſſuring, that as on the one Hand they were made with a very ſufficient *Inſtrument*, with all the Care and Diligence requiſite; ſo in the remote Voyages I have ſince taken to aſcertain the *Magnetick Variations*, they have been of ſignal Uſe to me, in determining the *Longitude* of my *Ship*, as often as I could get Sight of a near *Tranſit* of the *Moon* by a known *Fixed Star:* And thereby I have frequently corrected my *Journal* from thoſe Errors which are unavoidable in long Sea-Reckonings.

'If therefore you happen at *Sea* to obſerve nicely the Time of an *Occultation* or cloſe *Application* of a *Star* to the *Moon*; and can find a correſpondent Obſervation, about the ſame mean *Anomaly* and Diſtance of the *Moon* from the *Sun* (either among theſe of mine, or in any other Collection of Obſervations accurately made) eſpecially near 'the ſame Time of the Year; and, above all, after the aforeſaid '*Period* of eighteen Years and eleven Days, you may, without ſenſi-'ble Error, from thence pronounce in what *Meridian* your *Ship* is; 'taking Care in ſo operoſe a Calculation, to commit no Miſtake; and, 'notwithſtanding the Direction the Moon gives you, not confiding 'ſo much therein as to omit any of the uſual Precautions to preſerve 'a Ship when ſhe approaches the Land.

'I had intended to inſiſt more largely upon this Method of obtain-'ing the *Moon's Place*, and, by Conſequence, the *Longitude* at *Sea*, 'but that I find, that it requires a juſt Treatiſe, too long to be here 'ſubjoined: And, more eſpecially, that the great Sir *Iſaac Newton* '(to whom no Mathematical Difficulty is inſuperable) has been pleaſ-'ed to give us a *True* and *Phyſical Theory* of the *Moon*'s Motions, 'whereby the Defects of all former *Tables* are ſo far amended, that it 'is hoped the Error may ſcarce ever exceed three Minutes of Motion, 'or ſo little in *Longitude*, that, perhaps, it may be thought a ſuffici-'ent Exactneſs for all the Uſes of *Navigation*. If therefore what is here 'offered find a kind Acceptance from thoſe that it chiefly concerns, I 'ſhall be encouraged to proceed on a Work I have long meditated, 'to improve the abovementioned *Period*, as to the abbreviating the 'Computation of *Eclipſes*, and, in general, to facilitate the too labo-'rious *Calculation* of the *Moon*'s *Place extra Syzygias*.

Not long after her late Majeſty Queen *Anne* was pleas'd to beſtow upon the Publick, an Edition of the much greater, and moſt valuable Part of Mr. *Flamſteed*'s Obſervations; by Help of which the great Sir *Iſaac Newton* had formed his curious Theory of the *Moon*, a firſt Sketch of which was inſerted by Dr. *David Gregory* in his *Aſtronomiæ Phyſicæ & Geometricæ Elementa*, publiſhed at *Oxford*, 1702; and again, in the ſecond Edition of Sir *Iſaac*'s *Principia*, which came out in 1713, we have the ſame reviſed and amended by himſelf, to that Degree of Exactneſs, that the Faults of the *Computus* formed therefrom rarely exceed a quarter Part of what is found in the beſt *Lunar Tables* before that Time extant.

Being thus provided with proper Materials, *viz.* a large Set of Obſervations, and a *Theory* of the *Motions* ſo very near the Truth, I reſumed my former Deſign of filling up my *Abacus* or *Synopſis* of the Defects of this *Lunar Theory*, and made Tables to expedite the *Calculus* according thereto, and compared the Numbers thereof with many of the moſt certain of Mr. *Flamſteed*'s Places obſerved. By this it was evident that Sir *Iſaac* had ſpared no Part of that Sagacity and Induſtry ſo peculiar to himſelf, in ſettling the *Epoches*, and other *Elements* of the *Lunar Aſtronomy*, the Reſult many Times, for whole Months together, rarely differing two Minutes of Motion from the Obſervations themſelves; nor is it unlikely but good Part of that Difference may have been the Fault of the Obſerver. And where the Errors were found greater, it was in thoſe Parts of the *Lunar Orb* where Mr. *Flamſteed* had very rarely given himſelf the Trouble of obſerving; *viz.* in the third and fourth Quarter of the *Moon*'s Age, where ſometimes theſe Differences would amount to at leaſt five Minutes.

Mr. *Flamſteed* was long enough poſſeſſed of the *Royal Obſervatory* to have had a continued Series of Obſervations for more than two *Periods* of eighteen Years; by which he had it in his Power to have done all that could be expected from Obſervation, towards diſcovering the *Law* of the *Lunar Motion.* But he contented himſelf with ſparſe Obſervations, leaving wide Gaps between, ſo as to omit frequently whole Months together; and in one Caſe the whole Year 1716. So that notwithſtanding what he has left us muſt be acknowledged more than equal to all that was done before him, both as to the Number and Accuracy of his Accounts; yet for want of an uninterrupted Succeſſion of them, they are not capable of diſcovering, in the ſeveral Situations of the *Lunar Orbit*, what Corrections are neceſſary to be allowed, to ſupply the Deficiencies of our *Computus.*

On Mr. *Flamſteed*'s Deceaſe, about the Beginning of the Year 1720, his late Majeſty King *George* I. was graciouſly pleaſed to beſtow upon me the agreeable Poſt of his *Aſtronomical Obſerver*, expreſly commanding me *to apply myſelf with the utmoſt Care and Diligence to the rectifying the Tables of the Motions of the Heavens, and the Places of the Fixed Stars, in order to find out the ſo much deſired Longitude at Sea, for the perfecting the Art of Navigation.* Theſe are the Words of my *Commiſſion*; and here I might have thought myſelf in a Condition to put in Execution my long projected Deſign of compleating my *Abacus*, or Table of the Defects of our *Lunar Numbers*; but on taking Poſſeſſion, I found the *Obſervatory* wholly unprovided of *Inſtruments*, and indeed of every thing elſe that was moveable, which poſtponed my Endeavours till ſuch Time as I could furniſh myſelf with an *Apparatus* capable of the Exactneſs requiſite. And this was the more grievous to me, on account of my advanced Age, being then in my ſixty fourth Year, which put me paſt all Hopes of ever living to ſee a compleat *Period* of eighteen Years Obſervation.

But, Thanks to God, he has been pleaſed hitherto to afford me ſufficient Health and Vigour to execute my Office in all its Parts with my own Hands and Eyes, without any Aſſiſtance or Interruption, during one whole *Period* of the *Moon*'s *Apogee*; which *Period* is performed in ſomewhat leſs than nine Years. In this Time I have been able to obſerve the *Right Aſcenſion* of the *Moon* at her *Tranſit* over the *Meridian*, near fifteen hundred times (and with an Exactneſs, I am bold to ſay, preferable to any thing done before) a Number not leſs than thoſe of the noble *Tycho Brahe*, *Hevelius* and *Flamſteed*, taken in one Sum, there being near four of my Lunar Obſervations for each Degree of the Zodiack, as alſo for each Degree of the *Argumentum annuum*, or Diſtance of the *Sun* from the *Moon*'s *Apogee.* And that theſe might be duly applied to rectify the Defects of our *Computations*, I have myſelf compared with the aforementioned Tables, made according to Sir *Iſaac*'s Principles, not only my own Obſervations, but alſo above eight hundred of Mr. *Flamſteed*'s.

This *Compariſon* of my own *Obſervations* (from the Time I eſteem them compleat) with the *Computus* by the ſaid Tables, being now continued for above nine Years, I deſign ſpeedily to communicate it to the Publick, together with the Tables themſelves, which have been printed, and ſhould long ſince have been publiſhed, had not my Poſt at *Greenwich* given me an Opportunity to examine, with proper Nicety, in what Parts of the *Lunar Orb*, and how much, our Numbers erred. So uſeful an Addition as this, it is hoped may fully anſwer the long delayed Expectation ſome Perſons may have had of ſeeing the ſaid Tables ſooner. By Means thereof, thoſe that are qualified may, if they pleaſe, examine by their own Obſervation the Truth of what is here aſſerted.

Comparing likewiſe many of the moſt accurate of Mr. *Flamſteed*, made eighteen or thirty ſix Years before (that is one or two *Periods* before mine) with thoſe of mine which tallied with them, I had the Satisfaction to find that what I had propoſed in 1710 was fully verified; and that the Errors of the *Calculus* in 1690 and 1708, for Example, differed inſenſibly from what I found in the like Situation of the Sun and Apogee, in the Year 1726. The great Agreement of the *Theory* with the *Heavens* compenſating the Differences that might otherwiſe ariſe from the Incommenſurability and Excentricity of the Motions of the Sun, Moon and Apogee.

Encouraged by this Event, I next examined what Differences might ariſe from the Period of nine Years wanting nine Days, in which Time there are performed very nearly one hundred and eleven *Lunations*, or Returns of the *Moon* to the *Sun*; but the Return of the *Sun* to the *Apogee* in that Time differing above four times as much from an exact Revolution as in the Period of eighteen Years, I could not expect the like Agreement in that. However, having now entered upon the tenth Year, I compared what I had obſerved in the Years 1721 and 1722, with my late Obſervations of 1730 and 1731, and have rarely found a Difference of more than one ſingle Minute of Motion (Part of which may probably ariſe from the ſmall Uncertainty that always attends Aſtronomical Obſervation) but moſt commonly this Difference was wholly inſenſible; ſo that by the Help of what I obſerved in 1722, I preſume I am able to compute the true Place of the Moon with Certainty, within the Compaſs of two Minutes of her Motion, during this preſent Year 1731, and ſo for the future. This is the Exactneſs requiſite to determine the Longitude at Sea to twenty Leagues under the Equator, and to leſs than fifteen Leagues in the *Britiſh* Channel.

It remains therefore to conſider after what Manner Obſervations of the Moon may be made at Sea with the ſame Degree of Exactneſs:

But ſince our worthy Vice-Preſident *John Hadley*, Eſq; (to whom we are highly obliged for his having perfected and brought into common Uſe the *Reflecting Teleſcope*) has been pleaſed to communicate his moſt ingenious Invention of an Inſtrument for taking the Angles with great Certainty by Reflection (*Vid. Tranſact.* N° 420) it is more than probable that the ſame may be applied to taking Angles at Sea with the deſired Accuracy.

The Longitude of Buenos Aires, *determined from an Obſervation made there by* Pere Feuillée. *By the ſame.* N° 370 *p.* 364.

IV. 1. I have as Occaſion offered, made it my Buſineſs to collect ſuch Celeſtial Obſervations as might be of Uſe to determine the Longitudes of Places on the Sea-coaſt of the World; in order to get as near as poſſible the Out-line, or true Figure of the Earth, without which our Geography of the Inlands muſt neceſſarily be very inſufficient. The Memoirs of the Royal Academy of *Paris*, afford a good Number of Obſervations of this Kind, and among the reſt, there is one made at *Buenos Aires* on the River of *Plate*, in the South *America*, by *Pere Feuillée* in his Voyage to *Peru*: Who, in the Memoirs for the Year 1711, is ſaid to have obſerved at that Place on the 19th of *Auguſt*, 1708, the Immerſion of the Star in the Southern Foot of *Virgo* (marked by *Bayer* with λ) behind the obſcure Limb of the Moon. Being deſirous to ſee what Longitude might be deduced from this Obſervation, I ſoon found that there was a Fault in the Day, and likewiſe in the Star; for that λ of *Virgo* was then nearly in 3 Degrees of *Scorpio*, and the Moon would not be there till the next Day, *Monday* the 20th of *Auguſt*; and the Latitude of λ being about half a Degree North, the Moon at that Longitude would be about 3 Degrees more Southern than the Star, and conſequently far from Eclipſing it; for that at that Time the deſcending Node was in the very Beginning of *Libra*. Hence I concluded it muſt be ſome other Star, that *Pere Feuillée* obſerved Eclipſed by the Moon: The Day was certainly the 20th and not the 19th of *Auguſt*, as was evident by the Place of the Moon; but as to the Star, it was neither in the *Tychonick* Catalogue, nor yet in that more copious *Britiſh* Catalogue of Mr. *Flamſteed*; but turning over that of *Hevelius*, I found a Star whoſe Situation agreed well with the Obſervation, and was undoubtedly the Star that was ſeen to immerge behind the Moon: The Place Mr. *Hevelius* gives it, allowing the Preceſſion of the Equinox, was then ♏ 1° 56′$\frac{1}{4}$ with South Latitude 2° 51′$\frac{1}{4}$. It remained then for me to be aſſured of the Place of this Star, and accordingly on the 21ſt and 24th of *December* laſt, I got ſuch Obſervations by Help of the circumjacent Stars, that I was aſſured the Place of the Star (which is a fair Star, of the 5th Magnitude) was at that time, ♏ 1° 58′ 40″ with South Latitude 2° 54′$\frac{1}{3}$, being above 2′ in Longitude, and 3′ in Latitude, more than *Hevelius* gives it. The Hour of this Occultation is ſet down preciſely, 7^h 5′ 38″ at *Buenos Aires*, the Latitude of the Place being 34° 35′ South. Whence the Altitude of the Moon there was then 42° 48′, and the

Parallactick Angle 76° 38′, and the Parallax in Longitude 40′ 11″ to the Weſt, and in Latitude 9′ 33″ to the North. So the Moon's obſerved Place corrected by Parallax was ♏ 2° 28′ 4″ with South Latitude 2° 52′$\frac{1}{2}$. To this Place, by the *Calculus* of thoſe Numbers I have fitted to our Preſident's Theory of the Moon (but which would be improper and too long to be here recited). the Moon will be found to have arrived *Auguſt* the $\frac{9}{20}$ at 10^h 57′ 36″ apparent Time at *London*. But at *Buenos Aires* it was then computed but 7^h 5′ 38″, whence the Difference of Longitude reſulting from this Obſervation is 3^h 52′ or 58 Degrees, by how much *Buenos Aires* is more Weſterly than *London*. I have twice repeated the Calculation to be ſure to avoid Error, and by comparing my Chart of the Variation with the Longitude thus found, it appears that in this Caſe a Ship at Sea uſing thoſe Tables and that Chart, would by an Obſervation of this Occultation have fallen with greater Exactneſs on the Coaſt of *America*, than by any Reckoning can be pretended to be done.

The Longitude of Carthagena *in* America. *By the ſame.* N° 375. p. 237.

2. Having lately, by the Favour of Sir *Hans Sloane*, received a Packet of Obſervations from *Cartagene* in *America*, made by Colonel *Don Juan de Herrera*, Chief Engineer of that City, I find among them one Immerſion of the firſt Satellite of *Jupiter* into his Shadow, obſerved there by a Teleſcope of 17 $\frac{1}{2}$ Feet, on *April* 9. *Stilo vet.* 1722. at 15 *h.* 58′ 44″ apparent Time; and two Emerſions of the ſame, *viz.* *July* 5. 11 *h.* 23′ 41″ and *July* 21. 9 *h.* 42′ 17″ *Stil. vet.* all which tally with Obſervations made at *Wanſted*, by the Reverend Dr. *Pound* and Mr. *Bradley*, who obſerved there the very next Eclipſes to all the three; that is to ſay, the Immerſion by a fifteen Foot Tube, on *April* 11. 15 *h.* 28′ 40″ *Temp. æqu.* or 15 *h.* 30′ 25″ *Temp. app.* And the firſt Emerſion, *July* 7. 10 *h.* 59′ 28″ *Temp. æq.* by the Reflecter, and 18″ after, or 10 *h.* 59′ 46″ by the 15 Foot Glaſs, that is, 10 *h.* 54′ 12″ apparent Time. The other was obſerved at *Wanſted*, *July* 23. 9 *h.* 19′ 10″ *Temp. æq.* both by the Reflecter and 15 Foot Glaſs; that is to ſay, at 9 *h.* 13′ 35″ apparent Time. Subſtract from each of theſe one Period of this Satellite, or 1 *d.* 18 *h.* 28′ 36″ and *April* 9. 15 *h.* 58′ 44″ at *Cartagene* will be 21 *h.* 1′ 49″ of the ſame Day at *Wanſted*, and the Difference of Meridians 5 *h.* 3′ 5″. Likewiſe by the firſt Emerſion, *July* 5. 11 *h.* 23′ 41″ at *Cartagene*, was at *Wanſted* 16 *h.* 25′ 36″ of that Day, whence the Difference of Meridians 5 *h.* 1′ 55″. But by the laſt Emerſion, *July* 21. 9 *h.* 42′ 17″ at *Cartagene* was 14 *h.* 44′ 59″ at *Wanſted*; whence *Wanſted* is 5 *h.* 2′ 42″ more eaſterly than *Cartagene*: and taking the Medium of all three, 5 *h.* 2′ 34″ or 75° 38′ may be taken for the true Difference of Longitude, that is, 75 $\frac{1}{2}$ from *London*, which compared with Capt. *Candler*'s Obſervation of the late Lunar Eclipſe, ſhews *Cartagene* to be about 20 Leagues to the Eaſtwards of *Port Royal* in *Jamaica*.

3. The

—— *Of the Fort of* New-York, *from Eclipſes of the firſt Satellite of* Jupiter *communicated by* William Burnet, *Eſq; Governor of* New York, F. R. S. n. 385. p. 162.

3. The Latitude of the Fort, was formerly determined to be 40° 40′.

	H.	′	″
Aug. 9. 1723. Time of Emerſion at *London*, according to Mr. *Pound*'s Tables, reduced to apparent Time	16	09	25
Time as it was ſeen at *New York*	11	10	43
Difference of Meridians	4	58	42

I neglected to write down the Altitudes which were taken of the Sun, for correcting the Clock.

Aug. 25.	Alt. of the Sun's Upper Limb.			Time by the Clock.			Time by Calculat.		
	°	′	″	H.	′	″	H.	′	″
Sun's Declin.	49	30	00	10	17	52	10	17	28
6° 55′	51	13	30	10	33	10	10	32	8
Aug. 26.									
Sun's Declin.	46	24	00	9	57	40	9	56	25
6° 33′	47	50	00	10	8	22	10	6	57

	H.	′	″
Time of Emerſion by Mr. *Pound*'s Tables	14	31	25
Equation of Time to be added	00	01	22
	14	33	47
Time obſerved by the Clock	09	35	14
The ſame corrected	09	34	14
The Difference of Meridians	04	58	33

This I look upon as the moſt diſtinct and beſt Obſervation.

Sept. 10.	Alt. of the Sun's Upper Limb.		Time by the Clock.			Time by Calculat.		
	°	′	H.	′	″	H.	′	″
Sun's Declin.	33	21	09	01	00	09	00	16
49′	34	06	09	06	01	09	04	49
Sept. 17th								
Sun's Declin.	17	17	04	21	40	04	21	44
1° 54′	15	15	04	33	05	04	32	47

	H.	′	″
Time of Emerſion by the Clock *Sept.* 10.	08	00	10
Time of Emerſion by Mr. *Pound*'s Tables	12	50	36
Equation of Time to be added	00	06	54
	12	57	30
Corrected Time at *New-York.*	07	59	08
Difference of Meridians	04	58	22

June, 26. 1724.

	Altitude of the Sun's Upper Limb.		Time by the Clock.			Time by Calculat.		
	°	′	*H.*	′	″	*H.*	′	″
June 20th, Sun's Declin. 23 . 7	56	44	09	48	03	09	43	37
	60	27	10	09	40	10	05	05
June 27th, Sun's Declin. 22 , 26.	63	31	10	27	43	10	27	05
	65	21	10	40	00	10	39	27

June 26.

	H.	′	″
Time of Immerſion by the Clock	11	41	12
Time of Immerſion by Mr. *Pound*'s Tables	16	43	02
Equation of Time to be ſubſtracted	00	04	26
	16	38	36
Time at *New-York* corrected	11	40	15
Difference of Meridians	04	58	21

The Mean of all theſe Obſervations is 4h 58′ 30″, which agrees to 3″ with that Obſervation, which I thought the moſt exact, and therefore the Longitude of *New York*, is nearly 74° 57′ 30″ Weſt from *London*.

Variation of the Needle.

The Variation of the Magnetick Needle was obſerved, this Year, to be 7° 20′ Weſt. *Philip Wells*, Surveyor General of this Province, in the Year 1686, obſerved it to be 8° 45′; by which, it appears to decreaſe about 1° 25′ in 38 Years, or a little more than two Minutes in a Year.

—— *Of* Liſbon, Paris, *and* London, *from Eclipſes of the Moon, and of the Satellites of* Jupiter, *by* F. J. Bapt. Carbone, n. 385. p. 186.

4. Illud advertere juvat, differentiam inter hunc meridianum, ac Pariſienſem, minorem jam inventam, ac nos antea putabamus; non quidem novis obſervationibus hic habitis, ſed iiſdem cum Pariſienſibus, quas modo accepimus, comparatis. Nullas antehac acceperamus, unde dictam differentiam deprehendere certo poſſemus; ſed noſtras obſervationes unice contuleramus cum ſupputationibus domini *Lieutaud*, meridiano Pariſienſi accommodatis, in ſuo libello, quem *Connoiſſance des Temps* inſcribit, & quotannis, Academiæ regiæ juſſu, in lucem edit. At nimium a vero aberrare illas dignovimus, præcipue, quæ ad immerſiones, atque emerſiones intimi Jovis ſatellitis ſpectant. Ipſæ enim obſervationes habitæ in Obſervatorio regio, modo duobus, modo tribus, modo etiam 4 minutis, ab illis diſſentiunt. En tibi comparationem noſtræ obſervationis lunaris Eclipſis, cum obſervatione Pariſienſi, ab ipſo *Maraldo*, obſervatore, & regio aſtronomo facta; cui etiam apponam comparationes immerſionum atque emerſionum, quæ utrobique fuerunt obſervatæ: omnia genuino idiomate, quo fuerunt ad me miſſa, Pariſiis.

Comparaiſon des Obſervations de l'Eclipſe de Lune du 1 Novembre 1724, faites a Lisbone, & a Paris.

Quoyque dans cette eclipſe, l'ombre de la Lune n'ait point paru terminée a Lisbone, ny a Paris, ce qui a rendu la determination de ces phaſes plus difficiles, cependant la plus part des obſervationes s' accor-

dent ſi bien enſemble, que nous avous cru devoir faire la comparaiſon des phaſes principales, qui paroiſſent avoir eté obſerveés avec le plus d' exactitude, pour en determiner la difference des meridiens entre Liſbone, & Paris.

H.	′	″	
1	47	45	Commencement a Lisbone
2	33	30	a Paris
	45	45	Difference des meridiens entre Lisbone & Paris
2	0	16	L'ombre a Ariſtarque a Lisbone
2	46	15	a Paris
	45	59	Difference
2	11	28	a Lisbone l'ombre a Galilée
2	56	20	a Paris
	44	52	Difference
2	34	37	a Lisbone l'ombre au bord ſeptentrional de la mer Caſpiene
3	20	30	a Paris
	45	53	Difference
2	37	17	a Lisbone l'ombre a Proclus
3	23	30	a Paris
	46	13	Difference
3	29	2	a Lisbone Ariſtarque ſort de l'ombre
4	14	30	a Paris
	45	28	Difference
3	31	34	Tout Copernic eſt hors de l'ombre a Lisbone
4	17	50	a Paris
	46	16	Difference
3	47	46	Timocharis eſt ſorti de l'ombre a Lisbone
4	33	34	a Paris
	45	48	Difference
3	58	59	Platon eſt entierement hors de l'ombre a Lisbone
4	44	23	a Paris
	45	24	Difference
4	20	36	Fin de l' eclipſe a Lisbone
1	6	30	a Paris
	45	54	Difference

Suivant ces obſervations la durée de l' eclipſe a Liſbone a eté de 2h 32′ 51″ plus petite ſeulement de 9 ſecondes qu'elle n'a eté obſervée a Paris, & la difference des meridiens, qui reſulte des obſervations du commencement & de la fin eſt de 45′ 50″ ce qui s'approche beaucoup de ce qui reſulte de la comparaiſon des autres taches obſervées a Liſbone, & a Paris.

Comparaiſon de quelques Obſervations des Satellites de Jupiter, faites a Liſbone, & a Paris.

Nous avons fait a l'obſervatoiree royal de Paris pluſieurs obſervations correſpondantes a celles qui nous ont eté envoyées de Liſbone, en voici la comparaiſon. Le

		H.	′	″	
Le 30 Juin. 1724	a	2	08	51	Immerſion a Lisbone
		2	54	41	a Paris
			45	50	Difference
Le 2 Sept. 1724	a	9	36	57	Emerſion a Lisbone
		10	22	46	a Paris
			45	49	Difference
Le 25 Sept. 1724	a	9	59	21	Emerſion a Lisbone
		10	45	05	a Paris
			45	44	Difference
Le 4 Octobre	a	6	26	44	Emerſion a Lisbone
		7	11	58	a Paris
			45	14	Difference

La plus part de ces obſervations, s' accordent a donner la difference des meridiens, entre Lisbone & Paris de 45′ 48″ d'heure, ce qui s'accorde avec toute l'exactitude que l'on peut eſperer, a celle que l'on a determinée par l'obſervation derniere de l'eclipſe de Lune, &c. Hactenus *Maraldus*, cujus obſervationem lunaris eclipſeos, ſeorſim tranſcribere non vacat; pluribus enim curis ſum diſtentus.

Si vera eſt prædicta differentia, nempe 45′ 48″, erit differentia inter hunc meridianum Ulyſſiponenſem, & Londini, 36′ 7″, quam mox collatis obſervationibus in utroque meridiano faciendis, melius examinabimus, certiuſque deprehendemus.

—— *Of* Liſbon, *and the Fort of* New York, *from* Wanſted *and* London, *determined by Eclipſes of the Firſt Satellite of* Jupiter. *By the Rev. Mr.* James Bradley, M. A. Aſtron. Prof. Savil. F. R. S. N° 394. p. 85.

5. Some curious Aſtronomical Obſervations having lately been communicated to this Society from *Liſbon*, among which were ſeveral Eclipſes of the firſt Satellite of *Jupiter*; I was willing to examine whether I had made any at *Wanſted* which tallied with them, that by comparing ſuch together, the true Diffence of Longitude between thoſe Places might be found. But looking over my Obſervations of the firſt Satellite, made laſt Year and the beginning of this, I meet only with Two Emerſions that were obſerved the ſame Night both at *Liſbon* and *Wanſted*. There are others, indeed, made within a few Days of each other, which may likewiſe be made uſe of to determine the Difference of Longitude; but not with the ſame Degree of Certainty, by reaſon of the irregular Motion of the Satellite; which I preſume, chiefly ariſes from the Gravity of the other Satellites towards it. For although the Effect of the Influence that the Satellites have on each other, is moſt remarkable in the Second, whoſe Motion will ſometimes be accelerated or retarded thereby, as much as amounts to 30 or 40 Minutes in time, in the ſpace of about ſeven Months, or in half the Period in which the three innermoſt Satellites return, to have nearly the ſame Poſition with reſpect to themſelves, and the Shadow of *Jupiter*; yet the firſt ſeems alſo liable to Inequalities that cannot well be accounted for, but from ſome ſuch Cauſe as

is before-mentioned, the effect of which will not eaſily be reduced to any Rule, but from a long and exact Series of Obſervations. And till ſome better and more certain Rule can be found out, we may ſuppoſe, that the Effect produced by this Cauſe, is, during ſmall Intervals, proportionable to the Time. On this Suppoſition I have compared ſome Obſervations with others not made the ſame Nights; and the reſult is nearly the ſame as in thoſe which were obſerved at the ſame time in both Places, as will appear by the following Particulars.

The Immerſion of the Firſt Satellite was obſerved at *Wanſted* with Mr. *Hadley*'s reflecting Teleſcope on *Auguſt* 4, N. S. 1725, about 45″ after the time of the Immerſion, as calculated from my Tables. By another Obſervation made *Auguſt* 29, N. S. the true Immerſion preceded the Calculation from the ſame Tables 1′ 10″. So that in 25 Days the Satellites Motion was accelerated as much as anſwered to 1′ 55″ in time. Suppoſing therefore the Acceleration to have been in the ſame proportion between *July* 28, and *Auguſt* 4, N. S. then the true Immerſion *July* 28, N. S. would have happened at *Wanſted* about 1′ 15″ after the time by the Tables, which make the Immerſion at 12^{h} 48′ 45″ App. Time. The true Immerſion therefore was at *Wanſted July* 28, N. S. 12^{h} 50′ 0″ App. Time; and at *Liſbon* 'twas obſerved at 12^{h} 12′ 26″ App. Time, the Difference being 37′ 34″.

September 28, N. S. the Firſt Satellite was ſeen emerging in the Reflecter at *Wanſted* 3′ 50″ ſooner than the Tables make the Emerſion; and by the Mean of two more Obſervations made at the ſame Place, and with the ſame Teleſcope, on the 14th and 16th of *October*, N. S. the true Emerſion preceded the Calculation 4′ 30″. We may therefore from hence conclude, that on *Sept.* 21, N. S. the true Emerſion at *Wanſted* preceded the Calculation by the Tables about 3′ 35″, and that the true Emerſion there was at 12^{h} 1′ 15″ *Apr.* 1; but this Emerſion was obſerved at *Liſbon* at 11^{h} 24′ 55″, the Difference being 36′ 20″.

The Obſervations at *Wanſted* being made with Mr. *Hadley*'s Reflecting Teleſcope (by which one may ſee the Firſt Satellite near $\frac{1}{4}$ of a Minute ſooner when 'tis Emerging, than in a Refracting Teleſcope of 15 Feet, and the contrary when 'tis Immerging) there ought to be ſome Allowance made on account of different Teleſcopes made uſe of at *Liſbon* and *Wanſted*, by deducting 10 or 15″ from the Difference of Time collected from the Immerſions, and adding as much to the Difference deduced from the Emerſions. Such Correction being made, the Difference of Meridians by the Immerſion obſerved *July* 28, will be 37′ 20″, and by the Emerſion *Sept.* 21, 36′ 35″.

The Emerſion obſerved at *Liſbon December* 8, N. S. at 8^{h} 32′ 40″ Apparent Time, was likewiſe ſeen at *Wanſted* in a 15 Foot Tube at 9^{h} 10′ 5″ Apparent Time, the Air being a little hazy, which may probably make the Difference 37′ 25″ a little too great.

The Emerſion ſeen at *Liſbon Jan.* 16, 1726. N. S. at 6ʰ 51′ 10″, which ſeems accompanied with Circumſtances that argue its Exactneſs, was likewiſe very well obſerved at *Wanſted* in a 15 Foot Tube at 7ʰ 28′ 22″ Apparent Time, the Difference being 37′ 12″.

Theſe are the only Obſervations among thoſe which were laſt communicated, that I could compare with any Degree of Certainty with my own: But I find others printed in the *Philoſoph. Tranſact.* N°. 385, which were likewiſe made by the ſame curious Perſons, who obſerved an Emerſion of the Firſt Satellite at *Liſbon September* 2, 1724, N. S. at 9ʰ 36′ 57″. This was ſeen alſo at *Wanſted* in the Reflecter at 10ʰ 13′ 28″ Apparent Time. Hence, allowing for the different Teleſcopes, the Difference of Meridians is 36′ 45″.

This Emerſion at *Wanſted* preceded the Calculation by the Tables 4′ 40″: And another Emerſion obſerved with the ſame Teleſcope on *Sept.* 18, N. S. preceded the Calculation 5′ 10″. We may therefore ſuppoſe, that on *Sept.* 9, N. S. the true Emerſion at *Wanſted* preceded the computed about 4′ 52″. The Emerſion that Day by the Tables was at 12ʰ 15′ 34″ App. Time; therefore the true Emerſion at *Wanſted* was at 12ʰ 10′ 42″. At *Liſbon* was obſerved at 11ʰ 34′ 26″. So that allowing for the Difference of Teleſcopes, the Difference of Meridians by this Obſervation is 36′ 30″.

The Mean of all theſe Differences is about 36′ 58″, from which ſubſtracting 28″ for the Difference of Meridians between *London* and *Wanſted*, the remainder will be the Difference of Meridians between *London* and *Liſbon*, viz. 36′ ½ = 9° 7′ ½, *Liſbon* being ſo much to the Weſtward of *London*. This Difference of Longitude is about 5′ ½ greater than what is determined in the forementioned *Tranſaction*: But as the Gentlemen to whom we are indebted for theſe Obſervations, have given us hopes that they will continue to make and communicate more, we need not doubt but their exact Care and Diligence will ſoon enable us to judge yet more nicely of the true Situation of thoſe Cities with reſpect to each other.

New York. The ſame *Tranſaction* containing ſome Obſervations of Eclipſes of the ſame Satellite made in the Fort of *New York*, communicated by his Excellency *William Burnet*, Eſq; Governor of *New York*, I ſhall take this Opportunity of determining the Longitude of that Fort more exactly than it can be ſuppoſed to be there done, by the bare Compariſon of the Obſervations with the Tables; having two Obſervations made at *Wanſted*, which tally with two made at *New York*, on *Aug.* 25, and *Sept.* 10.

By the Obſervation made *Aug.* 25, 1723, O. S. which is eſteemed the moſt diſtinct and beſt, the Satellite Emerged at 9ʰ 35′ 14″ by the Clock, which went about 1′ ¼ too faſt for the Apparent Time at the Emerſion, as appears by the Altitudes of the Sun's Limb taken the Morning paged wrongly from p. 414 to end of the book.

Morning before and after the Obſervation ; ſo that the Emerſion at *New York* was at $9^h\ 34'$ Apparent Time ; that is, $9^h\ 32'\ 20''$ Mean Time.

Auguſt 27, $8^h\ 57'\ 40''$ Mean Time, the Satellite was ſeen emerging at *Wanſted* in the Reflecter ; and *Sept.* 12. $7^h\ 17'\ 15''$ M. T. 'twas ſeen emerging again in the ſame Teleſcope : So that in $15^d\ 22^h\ 19'\ 35''$ there were 9 Emerſions ; and the Interval between each was about $1^d\ 18^h\ 28'\ 50''$. This ſubſtracted from the Time of the Emerſion obſerved at *Wanſted Auguſt* 27, will give the true Emerſion at *Wanſted* on *Auguſt* 25, $14^h\ 28'\ 50''$ M. T. that is, $4^h\ 56'\ 30''$ later than it was obſerved at *New York*.

September 10, $8^h\ 0'\ 10''$ by the Clock, another Emerſion was obſerved at *New York*. From the Altitudes of the Sun's Limb taken the Morning before, I compute the Error of the Clock at the Time of the Emerſion to be $1'\ 10''$, and that the Emerſion happened at $7^h\ 59'$ App. T. that is, $7^h\ 51'\ 52''$ Mean Time at *New York*. But ſubſtracting the forementioned Interval of $1^d\ 18^h\ 28'\ 50''$ from the Time of the Emerſion obſerved at *Wanſted September* 12, $7^h\ 17'\ 15''$ M. T. we ſhall have the Time of the true Emerſion at *Wanſted* on *Sept.* 10, at $12^h\ 48'\ 25''$ M. T. which is $4^h\ 56'\ 33''$ later than 'twas obſerved at *New York*. The Difference therefore of Meridians between *Wanſted* and *New York*, allowing about $15''$ for the Difference of Teleſcopes, is about $4^h\ 56'\ 45''$, and between *London* and *New York*, $4^h\ 56'\ \frac{1}{4}$. So that the true Longitude of *New York* from *London* is 74° 4′ Weſt.

6. Differentia Meridianorum Ulyſſiponem inter ac Telonem Martium ex pluribus obſervationibus eruta eſt, $1^h\ 0'\ 9''$, ſeu $15^\circ\ 2'\ 15''$, quibus Ulyſſipo occidentalior eſt Telone Martio.

—*Of* Toulon *and* Liſbon. *By* F. Ant. Laval. N°. 394. *p.* 101.

7. Ex pluribus obſervationibus Romæ habitis, & Ulyſſipone, eritur Meridianorum differentia $1^h.\ 28'\ 0''$, ſeu 22°, quæ ſane differentia aut omnino vera aut quam proxime veritati accedere videtur.

—*Of* Liſbon, *By Sig.* Francis Bianchini. N°. 396. *p.* 178.

——*Of diverſe Places computed from Obſervations of the* Eclipſes *of* Jupiter's Satellites, *by the Rev.* Mr. Derham. N°. 407. *p.* 34.

8. *Rome* and *Liſbon.*

H.	′	″
1	24	46
1	25	34
1	26	34
1	29	0
1	26	44
1	26	54
1	28	11
Rome and *Paris.*		
0	39	48
0	40	50
0	36	16
0	38	56
0	40	17
Rome and *Ingolſtad.*		
0	2	51
0	4	1
Rome and *Bologne.*		
0	3	45
	2	16
0	4	45
0	4	14

Rome and *Kew.*

H.	′	″
0	45	47
Rome and *Wanſted.*		
0	49	10
Rome and *Upminſter.*		
0	47	28
Rome and *Southwick Northampton-ſhire.*		
0	47	58
Urbino and *Liſbon.*		
1	28	57
Paris and *Liſbon.*		
0	45	46
0	45	44
Paris and *Bologne.*		
0	34	30
0	34	0
0	38	32

Ingolſtad and *Liſbon.*

H.	′	″
1	22	53
1	23	21
Ingolſtad and St. *Quirico.*		
0	1	20
0	1	40
Ingolſtad and *Bologne.*		
0	1	53
Ingolſtad and *Paris.*		
0	36	23
0	36	00
Ingolſtad and *Upminſter.*		
0	46	10
St. *Quirico* & *Liſbon.*		
1	22	30
St. *Quirico* & *Paris.*		
0	37	40

St. *Quirico* & *Upminſter.*

H.	′	″
0	47	50
Florence and *Liſbon.*		
1	19	43
Florence and *Bologne.*		
0	0	31
Florence and *Upminſter.*		
0	42	1
Upminſter & *Bologne.*		
0	43	43
Upminſter & *Liſbon.*		
0	37	42
Bologne and *Liſbon.*		
1	21	28
Bologne and *Albano.*		
0	3	43

Latitude of Yale *College, by Mr* Tho. *Robie.* N°. 382. p. 67.

V. 1. *Yale* College in *Connecticut* Colony in *New England* lies about 8′ or 10′ Weſt from *Cambridge*, in Latitude about 41° ½ *North.*

——*Of* New York, *communicated by* Will. Burnet, *Eſq;*. N°. 385. p. 162.

2. The Latitude of the Fort of *New-York* was formerly determined to be 40° 40′.

——*Of* Liſbon *by* F. John Baptiſt Carbone. N°. 394. p. 93.

3. Cum innumeræ circa hujus urbis Latitudinem, inſtitui poſſint interdiu, noctuque obſervationes, non adeò nobis opportunum fuit illam citiùs explorare; inſtrumenta ſiquidem, quæ hic invenimus, quæque etiam nobiſcum tulimus, etſi ſatis apta ad gradus ac minuta prima præter propter dignoſcenda, non tamen ad minuta eadem certiùs exploranda, multoque minùs ad ſecunda inveſtiganda (quæ ſanè

Aſtronomis contemnenda non ſunt) opportuna eſſe videbantur. Atque hoc magis, quod non una erat de hujuſce urbis Latitudine ſententia, quoad minuta prima; quarum quidem nonnullæ propius veritati accedebant, ut ex noſtris quoque obſervationibus inferre licebat; verum nulli fidere certò poteramus. Duas tantùm ſententias in medium profero, quarum ſingulæ plurimum ponderis apud prudentes habere poſſent; ni diſcordia inter utramque earum fidem imminueret. Altera igitur ſententia eſt Emanuelis Pimentel Regii Coſmographi, in mathematicis apprimè verſati, qui multis ac repetitis obſervationibus per umbram rectam Gnomonis, cujus altitudo pedum 16, ſe inveniſſe teſtatur hujus Poli altitudinem 38° 48′ 20″, quod in libello quodam MS. ejuſdem authoris legi, in quo & ipſas obſervationes fuſè adnotaverat. Altera verò ſententia eſt Regiæ Pariſienſis Academiæ ex obſervationibus domini Couplet, qui Ulyſſiponem venit Anno 1697, ubi aliquot inſtituit obſervationes ad Meridianorum differentiam Ulyſſiponenſis ſcilicet ac Regii Obſervatorii, necnon hujuſce urbis latitudinem inveniendam. Hanc autem ſe inveniſſe fatetur 38° 45′ 25″. Ut igitur certi aliquid in hac re deprehendere poſſemus, expectandum tantiſper exiſtimavimus, dum aptiora Pariſiis inſtrumenta reciperemus; ubi juſſu ac munificentia ſereniſſimi Regis noſtri tum multa alia conficiebantur à peritiſſimis artificibus inſtrumenta, tum in primis Quadrantes duo Aſtronomici, quorum alter 5, alter 3 pedum Pariſinorum; nec non Sextans totidem pedum. Hæc jam inde recepimus, & multiplici experimento ad trutinam revocavimus; nec ſanè quidquam ſenſibile in iis corrigendum invenimus, niſi quod facillimè corrigi poteſt, & plerumque obſervatorum curæ corrigendum relinquitur, dioptras nempe Teleſcopicas ad rectum ſitum reducere. Id facilè præſtitimus, ſtatimque prædictis inſtrumentis uti cepimus, ad prædictam Poli altitudinem inquirendam.

Plurimas ſanè inſtituimus obſervationes; quarum tamen aliquot hîc ſubnecto circa Solis altitudines, præſertim Meridianas, vel ſextante, vel quadrante Aſtronomico trium pedum habitas, poſtmodùm miſſurus alias tum prædictis, tum etiam quadrante murali quinque pedum habendas vel circa Solem, vel circa reliqua Aſtra. Juvat verò tum obſervatas altitudines Solis ſubnectere, tum integras etiam ſupputationes, unde Poli altitudo deducta eſt.

Obſervationes
Altitudinum
Solis Meridianarum ad Poli
Elevationem
inveſtigandam
Ulyſſip.

Sequentium obſervationum aliæ habitæ ſunt in Collegio divi Antonii Magni, aliæ in Obſervatorio Palatii Regii; quæ ſanè loca, cùm in eodem ſint Meridiano, quidquid inter ſe differunt, in ſola differunt Latitudine: At verò tanta non eſt differentia, ut ejus habenda ſit ratio in his obſervationibus, quibus hujuſce urbis Elevationem non adeò exactè exploratam volo, ut ſecunda quoque Graduum inventa affirmem.

		°	′	″
Nov. 24. 1725.	Altitudo Limbi ſuperioris Solis in Merid. quadrante Aſtronomico obſervata	30	56	20
	Refractio propria hujus altitudinis ex tabulis Halleii		1	28
	Altitudo correcta hujuſdem Limbi	30	54	52
	Parallaxis Solis			4
	Altitudo vera Limbi ſuper Solem	30	54	56
	Semidiameter Solis Appar.		16	18
	Altitudo vera centri Solis	30	38	38
	Declinatio Solis Auſtral.	20	38	59
	Altitudo Æquatoris	51	17	37
	Complementum, ſeu Latitudo Ulyſſiponis	38	42	23
Decemb. 5.	Altitudo Meridiana Limbi ſuperioris Solis, ſextante obſervata	29	8	10
	Refractio propria hujus altitudinis		1	35
	Altitudo ejuſdem Limbi correcta	29	6	35
	Parallaxis Solis			4
	Altitudo vera ſuperioris Solis	29	6	39
	Semidiameter Solis		16	20
	Altitudo vera Centri Solis	28	50	19
	Declinatio Solis Auſtralis	22	27	7
	Altitudo Æquatoris	51	17	26
	Complementum, ſeu Poli elevatio	38	42	34
Decemb. 6.	Altitudo Limbi ſuperioris Solis ſextante obſervata	29	1	0
	Refractio propria hujus altitudinis		1	36
	Altitudo correcta ejuſdem Limbi	28	59	24
	Parallaxis Solis			4
	Altitudo vera Limbi ſuperioris	28	59	28
	Semidiameter Solis		16	21
	Altitudo vera Centri Solis	28	43	7
	Declinatio Solis Auſtralis	22	34	24

Alti-

	°	′	″	
Altitudo Æquatoris	51	17	31	
Complementum, ſeu Latitudo Ulyſſip.	38	42	29	
Altitudo Limbi ſuperioris Solis quadrante obſervata	28	20	22	*Decemb.* 29.
Refractio propria hujus Altitudinis		1	39	
Altitudo correcta Limbi ſuperioris Solis	28	18	43	
Parallaxis Solis			4	
Altitudo vera Limbi ſuperioris Solis	28	18	47	
Semidiameter Solis		16	21	
Altitudo vera Centri Solis	28	2	26	
Declinatio Auſtralis	23	14	57	
Altitudo Æquatoris	51	17	23	
Complementum, ſeu Latitudo Ulyſſip.	38	42	37	
Altitudo Limbi inferioris Solis ſextante obſervata	28	47	10	*Jan.* 8. 1726.
Refractio propria hujus altitudinis		1	37	
Altitudo correcta Limbi inferioris Solis	28	45	33	
Parallaxis Solis			4	
Altitudo vera Limbi inferioris Solis	28	45	37	
Semidiameter apparens Solis		16	21	
Altitudo vera Centri Solis	29	1	58	
Declinatio Auſtralis Solis	22	15	42	
Altitudo Æquatoris	51	17	40	
Complementum, ſeu Latitudo Ulyſſip.	38	42	20	

Jan. 9.

Duas Solis altitudines ſextante Aſtronomico obſervavimus ante Meridiem, totidemque poſt Meridiem, alteras alteris reſpondentes, in verticalibus ſcilicet à Meridiano æquidiſtantibus; quod ut accuratiùs fieret, addita ſunt altitudinibus pomeridianis ſcrupula convenientia, quæ nimirum ex declinatione Solis, utcunque minori, refundi debebant in ipſam Solis verticalem altitudinem. Has quidem obſervationes eatenùs inſtituimus, ut noſtri horologii pendulo inſtructi vel minimam à tempore vero diſcordiam deprehenderemus, ſimulque meridianas lineas, quibus non unis utimur in hoc noſtro Collegio, iterùm atque iterùm ad trutinam revocaremus. Utræque verò obſervationes inter ſe collatæ, adeò exactè conſenſere in eadem differentia oſtendenda, ut de illarum rectitudine non niſi temerè dubitarem. Opportunè igitur his ipſis obſervationibus uſuros nos duxi ad Poli quoque altitudinem explorandam; his tribus nempe cognitis, altitudine Solis, ejuſdem declinatione, & hora diei.

	°	′	″
Altitudo Solis vera	20	36	18
Ejuſdem declinatio Auſtralis	22	8	11
Tempus verum obſervatæ altitudinis	9ʰ.	37	26

Ex his igitur, per calculos Trigonometricos, quos inivimus (nec tamen hic apponere opus eſt) reſultat Poli elevatio Ulyſſipone, 38° 42′ 24″.

Iterum, ex ſecunda obſervatione matutina,

Altitudo vera Solis	23.	25	47
Ejuſdem declinatio Auſtralis	22	8	10
Tempus verum obſervatæ altitudinis	10ʰ.	4	41

Ex quibus pariter per Trigonometriam reſultat Poli elevatio Ulyſſip. 38° 42′ 25″.

Ex obſervationibus pomeridianis, in quibus penè omnia ſunt eadem atque in obſervationibus matutinis, eadem quoque inferri debebat altitudo Poli, ac proinde novis calculis non fuit opus.

Jam verò ex omnium obſervationum complexu, inferre hactenus licet Latitudinem Ulyſſiponenſem in hoc Collegio divi Antonii, aut etiam in Palatio Regio obſervatam non excedere 31° 43′, nec minorem eſſe 38° 42′; propiùs verò accedere ad 38° 42′ 30″. Quod ſanè quamprimum novis atque iteratis obſervationibus certiùs innoteſcet.

Aliorum diſſidium in hac Elevatione aſſignanda, vel ex aliquo inſtrumentorum vitio (quod non facilè noſtris contingere potuit, cum & plura ſint, & grandiuſcula, nec uno ab artifice elaborata, ac tandem ſæpiùs adhibita in idem ſemper conſpiraverint) vel ex locorum diverſitate, in quibus obſervationes habitæ ſunt oriri putaverim. Eſt enim Ulyſſipo ſatis ampla; & à Borea ad Meridiem plus una extenditur Leuca, quæ quidem diſtantia trium, vel quatuor minutorum parere poſſet diſcordiam. Accedit ad hæc non exigua difficultas, quam quiſque, vel peritiſſimus, experiri ſolet in determinandâ extremitate umbræ veræ, eâdemque à penumbrâ ſecernendâ; quam ſanè difficultatem non facilè declinaverit clariſſimus Vir, de quo ſuperiùs memini, Emanuel Pimentel, in ſuis obſervationibus per umbram rectam Gnomonis. Ac proinde nec mirum ſi qua intercedat diverſitas, cùm non una ſit inſtrumentorum conditio.

	°	′	″	
4. Altitudo Meridiana limbi ſuper. Solis, *Quadrante Murali quinque pedum*	67	5	25	*Continued by the ſame.* N°. 401. p. 409. *Maii* 3.
Correctio additiva Quadrantis		10	15	
Altitudo apparens ejuſdem limbi	67	15	40	
Refractio			23	
Altitudo correcta, & vera limbi ſuperioris	67	14	17	
Semidiameter Solis apparens		15	56	
Altitudo vera centri	66	58	21	
Declinatio Borealis	15	40	36	
Altitudo Æquatoris	51	17	45	
Elevatio Poli	38	42	15	

	°	′	″	
Altitudo Meridiana limbi ſuper. Solis, *Quadrante aſtronomico trium pedum*	75	5	20	*Jun.* 22.
Correctio Quadrantis ſubtrahenda		2	37	
Altitudo apparens prædicti limbi	75	2	43	
Refractio			15	
Altitudo vera limbi ſuperioris	75	2	28	
Semidiameter Solis apparens		15	53	
Altitudo vera centri Solis	74	46	35	
Ejuſdem Declinatio Borealis	23	28	50	
Altitudo Æquatoris	51	17	45	
Elevatio Poli	31	42	15	

	°	′	″	
Altitudo Meridiana Lucidæ Lyræ, *Quadrante Murali quinque pedum*	89	40	15	*Aug.* 11.
Correctio Quadrantis, addit.		10	15	
Altitudo vera ſideris	89	50	30	
Declinatio Borealis, ex tabulis Flamſteadi	38	32	55	
Altitudo Æquatoris	51	17	35	
Elevatio Poli	38	42	25	

	°	′	″	
Altitudo Meridiana limbi ſup. Solis, *eodem Quadrante Murali*	65	49	13	*Aug.* 14.
Correctio Quadr. add.		10	15	
Altitudo apparens ejuſdem limbi	65	59	28	
Refractio			24	
Altitudo vera limbi ſuperioris	65	59	4	
Semidiameter apparens Solis		15	53	
Altitudo vera centri	65	43	11	

		°	′	″
	Declinatio Bor.	14	25	30
	Altitudo Æquatoris	51	17	41
	Elevatio Poli	38	42	19
Sept. 24.	Altitudo Meridiana limbi ſup. Solis, *Quadrante aſtronomico trium pedum*	50	47	11
	Correctio Quadrantis ſubtr.		2	37
	Altitudo apparens ejuſdem limbi	50	44	34
	Refractio			44
	Altitudo apparens correcta	50	43	50
	Parallaxis			2
	Altitudo vera limbi ſuperioris	50	43	52
	Semidiameter Solis apparens		16	4
	Altitudo vera centri Solis	50	27	48
	Ejuſdem Declinatio Auſtralis		50	12
	Altitudo Æquatoris	51	18	0
	Elevatio Poli	38	42	0
Octob. 27.	Altitudo Meridiana limbi ſup. Solis, *Sextante*	38	40	15
	Correctio Inſtrumenti additiva		6	5
	Altitudo apparens ejuſdem limbi	38	46	20
	Refractio		1	6
	Altitudo limbi ſuperioris corr.	38	45	14
	Parallaxis			3
	Altitudo vera ejuſdem limbi	38	45	17
	Semidiameter ſolis apparens		16	12
	Altitudo vera centri	38	29	5
	Ejuſdem Declinatio Auſtralis	12	48	34
	Altitudo Æquatoris	51	17	39
	Elevatio Poli	38	42	21
Octob. 27.	Altitudo Meridiana *Fomahantis Aquarii* ſeu Lucidæ in ore Piſcis Auſtrini, *eodem Sextante*	20	9	55
	Correctio Inſtrumenti additiva		6	5
	Altitudo Apparens	20	16	0
	Refractio		2	26
	Altitudo vera ſideris	20	13	34
	Declinatio Auſtralis ex *De La Hire*	31	3	59
	Altitudo Æquatoris	51	17	33
	Elevatio Poli	38	42	17

Ex prædictis Obſervationibus, non imprudenter ſtatui poteſt *latitudo Obſervatorii Regii in aulâ* 38° 42′ 20″, *noſtri verò Collegii D. Antonii Magni*, 38° 42′ 30″.

5. Ex permultis accuratiſſimis obſervationibus Telonis Martis Latitudo inventa eſt 43° 6′ 55″.

—*Of* Toulon *by* F. Ant. Laval. N°. 394 p. 101.

6. On *Wedneſday*, *Feb.* 2. we took our Departure from *Java Head*, allowing it to lie in the Latitude of 6° 45′ South.

Obſervations of Latitude *and* Variation, *taken on Board the* Hartford, *in her Paſſage from* Java Head *to St.* Hellena, A.D. 173½. *Communicated by* Ed. Halley, *LL.D. F.R.S.* N° 424. p. 331.

ib. 7.	By a good Amplitude made	3°	28′	Variat. NWly.	
	Latitude by Account	9	59	South.	
	Merid. Diſt. from *Java Head*		43	West.	
	Longitude from ditto		45		
b. 13.	By a good Azimuth made	4	45	Variat. NWly.	
	Latitude by good Obſervat.	13	43	South.	
	Merid. Diſt. from *Java Head*	3	31	Weſt.	
	Longitude from ditto	3	36		
b. 15.	By a good Amplitude	4	52	Variat. NWly.	
	Latitude *per* Obſervation	15	18	South.	
	Merid. Diſt. from *Java Head*	6	1	Weſt.	
	Longitude from ditto	6	9		
	By a good Azimuth and Amplitude	4	51	Variat. NWly.	*Feb.* 21.
	Latitude *per* Obſervation	18	12	South.	
	Merid. Diſt. from *Java Head*	17	28	Weſt.	
	Longitude from ditto	18	00		
	By a good Amplitude	6	8	Variat. NWly.	*Feb.* 25.
	Latitude *per* Obſervation	19	59	South.	
	Merid. Diſt. from *Java Head*	21	17	Weſt.	
	Longitude from ditto	32	1		
	By a good Azimuth	10	3	Variat. NWly.	*Feb.* 29.
	Latitude *per* Obſervation	21	00	South.	
	Merid. Diſt. from *Java Head*	30	28	Weſt.	
	Longitude from ditto	32	12		
	By a good Amplitude made	15	15	Variat. NWly.	*March* 5.
	Latitude *per* Obſervation	23	16	South.	
	Merid. Diſt. from *Java Head*	37	18	Weſt.	
	Longitude from ditto	38	58		
	By a good Amplitude made	18	2	Variat. NWly.	*March* 8.
	Latitude *per* Obſervation	25	11	South.	
	Merid. Diſt. from *Java Head*	40	30	Weſt.	
	Longitude from ditto	42	33		
	By an Azimuth & Amplitude made	19	00	Variat. NWly.	*Mar.* 10.
	Latitude *per* Obſervation	26	18	South.	
	Meridian Diſtance	42	42	Weſt.	
	Longitude	44	15		

March 13.	By a very good Amplitude	21°	45′	Variat. NWly.
	Latitude *per* Obſervation	27	23	South.
	Meridian Diſtance	44	14	Weſt.
	Longitude from *Java*	46	34	
Mar. 17.	By a good Azimuth made	24	23	Variat. NWly.
	Latitude by Account	30	25	South.
	Merid. Diſt. from *Java Head*	52	29	Weſt.
	Longitude ditto	54	51	
Mar. 19.	By a good Azimuth had	24	50	Variat. NWly.
	Latitude *per* Obſervation	30	27	South.
	Meridian Diſtance	56	40	Weſt.
	Longitude	59	21	
Mar. 22.	By a good Azimuth had	24	15	Variat. NWly.
	Latitude *per* Account	31	23	South.
	Merid. Diſt. from *Java Head*	61	37	Weſt.
	Longitude from ditto	66	3	
Mar. 24.	By a good Amplitude had	23	51	Variat. NWly.
	Latitude *per* Obſervation	32	47	South.
	Meridian Diſtance	63	00	Weſt.
	Longitude	67	44	
April 1.	By a good Amplitude made	20	16	Variat. NWly.
	Latitude by Obſervation	34	58	South.
	Merid. Diſt. from *Java Head*	73	36	Weſt.
	Longitude from ditto	79	44	
Apr. 4.	By a good Azimuth & Amplitude	20	07	Variat. NWly.
	Latitude *per* Obſervation	35	33	South.
	Merid. Diſt. from *Java Head*	74	42	Weſt.
	Longitude from ditto	81	24	
Apr. 6.	By a good Amplitude made	19	7	Variat. NWly.
	Latitude *per* Obſervation	35	41	South.
	Merid. Diſt. from *Java Head*	77	2	Weſt.
	Longitude from ditto	87	12	
Apr. 7.	By a very good Amplitude made	17	30	Variat. NWly.
	Latitude by Obſervation	36	25	South.
	Meridian Diſtance from *Java Head*	77	56	Weſt.
	Longitude from ditto	87	58	
Apr. 10.	By a good Azim. & Amplitude made	16	9	Variat. NWly.
	Latitude *per* Obſervation	38	10	South.
	Merid. Diſt. from *Java Head*	77	24	Weſt.
	Longitude from ditto	87	26	
Apr. 13.	By a good Azim. & Amplit. made	15	40	Variat. NWly.
	Latitude *per* Obſervation	37	58	South.
	Merid. Diſt. from *Java Head*	77	21	Weſt.
	Longitude from ditto	85	15	

By

By a very good Azim. & Amplitudes	15°	45′	Variat. NWly.	*April* 14.
Latitude *per* Obſervation	37	4	South.	
Merid. Diſt. from *Java Head*	76	54	Weſt.	
Longitude from ditto	84	42		

N. B. This Day I judged Cape *Bonne Eſperance* to bear N. by W. from me, Diſtance 2° 34′.

By a very good Azimuth made	16°	14′	Variat. NWly.	*Apr.* 16.
Latitude *per* Obſervation	36	15	South.	
Merid. Diſt. from *Java Head*	77	59	Weſt.	
Ditto from Cape *Bonne Eſperance*	00	30		
Longitude from *Java Head*	85	14		
By a very good Amplitude made	15	45	Variat. NWly.	*Apr.* 18.
Latitude *per* Obſervation	35	33	South.	
Merid. Diſt. from *Java Head*	79	5	Weſt.	
Ditto from Cape *Bonne Eſperance*	1	36		
Longitude from *Java Head*	86	10		
By a very good Azimuth made	14	40	Variat. NWly.	*Apr.* 21.
Latitude *per* Obſervation	32	23	South.	
Merid. Diſt. from *Java Head*	81	9	Weſt.	
Ditto from Cape *Bonne Eſperance*	3	40		
Longitude from *Java Head*	87	9		
By a good Amplitude made	12	39	Variat. NWly.	*Apr.* 24.
Latitude by Obſervation	27	1	South.	
Merid. Diſt. from *Java Head*	84	52	Weſt.	
Ditto from Cape *Bonne Eſperance*	7	23		
Longitude from *Java Head*	89	18		
By a good Azimuth made	11	20	Variation.	*Apr.* 29.
Latitude *per* Obſervation	21	45	South.	
Merid. Diſt. from *Java Head*	89	8	Weſt.	
Ditto from Cape *Bonne Eſperance*	11	41		
Longitude from *Java Head*	92	20		
Latitude *per* Obſervation	16	00	South.	*May* 5.
Merid. Diſt. from *Java Head*	97	43	Weſt.	
Ditto from Cape *Bonne Eſperance*	20	16		
Longitude from *Java Head*	99	53		
By an Ampl. the Night before came in	8	00	NWly.	

At Noon *Barn Point* bore W by N ½. N. Diſtance four Miles.

VI. From *Tunis* I travelled as far Weſtward as *Hydra*, and from thence went to *Toſer*, paſſing from *Tegewſe* through the *Lake of Marks*, or the *Palus Tritonia*, as I take it, to *Gaps*; from *Gaps* I travelled all the Way upon the Coaſt to *Biſerta*; but at the ſame Time took Care to viſit ſuch Places within Land, where I could learn of any Ruins or Curioſities. I made uſe of a ſmall, but very good Mariners Compaſs, and found the Variation at *Cairwan* 10

A Geographical Deſcription and Map of the Kingdom of Tunis, *by the Rev. Mr.* Thomas Shaw, *Chaplain to the* Engliſh *Factory at* Algier. N° 411. p. 177. Fig. 198.

Degr. Weſt; at *Biſerta* ſomething more than 12 Degr. and at *Algier* I find it now to be 30 Degr. 30 Min. I carried along with me likewiſe a Braſs Quadrant of a Foot Radius, and took the Latitudes of *Tunis*, *Cairwan*, *Spetula*, *Gaffſa*, *Toſer*, *Ebillee*, *Gaps*, *Stax*, *Suſa*, *Lowbaria* and *Biſerta*, with all the Exactneſs ſuch an Inſtrument would admit of. As to the Longitude, moſt Mariners whom I have converſed with, agree within 10 or 12 Miles, that the Diſtance between *Algier* and the *Goletta* (or Port of *Tunis*) is 400 Miles. I have made this Voyage 4 Times, and the Reckonings we made aboard, amounted only to 390. I have made therefore the Meridional Diſtance betwixt this Place and Cape *Carthage* 350 Miles: (allowing 48 to a Degree of Longitude) for as this whole Courſe is not upon the ſame Parallel, we may very well allow 40 or 50 Miles for the oblique Sailing; becauſe the Courſe is in 37 Degr. 20 Min. N. Lat. but *Algier* lies in 36 Degr. 48. Min. and the *Goletta* in 36 Degr. 40 Min.

The Kingdom of *Tunis* is bounded to the North and Eaſt with the *Mediterranean Sea*, to the Weſt with the Kingdom of *Algier*, and to the South with that of *Tripoly*. It is 230 Miles in Length from the Iſle of *Gerba*, in Lat. 33° 24′, to Cape *Serra*, in Lat. 37° 16′, and 128 Miles in its greateſt Breadth from *Monaſteer* to *Tibéſa*. *Sbeka*, its utmoſt Boundary to the Weſt, lies in Long. 7° 26′ and *Clybea*, its utmoſt Boundary to the Eaſt, in 10° 47′ from *London*.

Of the Modern Geographers, *Luyts* ſeems to have been the beſt acquainted with its Extent in general, giving it 3° of Longitude, and (above) 4° in Latitude. The *Sanſons* place it above 3° further to the South than it ſhould be, and their Error is greater, in relation to the Longitude. *Moll* places it a few Minutes only too far to the North, but to the South he has extended it beyond the Parallel of *Tripoly*, wherein I find he has been followed by Mr. *De Liſle*, in his Map of *Africa*, 1722. But a long Chain of Mountains which run in the ſame Parallel of Latitude with *Gerba*, are the Limits of *Tunis* and *Tripoly*.

If we take the Antients for our Guides, we ſhall ſtill find further Errors and Diſagreements. For *Ptolemy* makes the Difference of Latitude betwixt *Carthage* and *Gaps*, almoſt the two Extremities of the Kingdom, to be only 1 Degr. and 50 Min. (provided the *Italian* Copy I make uſe of be correct.) The like Diſtance he puts between *Gaps* and *Toſer*, making thereby the latter 110 Miles more to the South; whereas I found it 18 Miles more to the North. Thus again he places *Gaffſa* in Latitude 29° 45′ and *Gaps* in 30° 30′ making *Gaps* a great Way to the North; whereas the Courſe from *Gaffſa* to *Gaps*, is near 80 Miles South-Eaſt: not to ſpeak of his placing *Carthage*, and ſo reſpectively of other Places, too far to the South by near 4° 30′ or 270 Miles. The like Errors may be obſerved as to his Difference of Longitude of particular Places, and as to his Scale of Longitude in General, which he places at leaſt 10° too far to the Eaſt.

The *Antonine* Itinerary will alſo admit of ſeveral Doubts and Contradictions, as *Ricciolus* has already obſerved, *Geogr. p.* 74. and therefore is not to be altogether depended upon; though it muſt ſtill be allowed to be a much better Conductor than *Ptolemy*. Thus the Author of the *Itinerary* makes it to be 216 Miles from *Sufetula*, I preſume by the Way of *Adrumettum*, to *Clypea*, thereby making *Clypea* 111 Miles from *Adrumettum*; whereas in another Place, in his *Maritime Itinerary*, he only makes a Difference of about 44 Miles, or 350 Furlongs. And again he makes the direct Road from *Carthage* through *Laribus* and *Theveſte* to *Cirta*, to be 332 Miles; but the Road by *Hippo Regius*, or *Bona*, which ſhould be further, only 312. So that great Caution is to be obſerved in following that Authority.

Pliny is not ſo particular as either *Ptolemy* or the *Itinerary*. He lays down Things in general, and therefore can give but little Light and Aſſiſtance to a Traveller, in pointing out to him the antient Boundaries, or the particular Cities of this Kingdom. His Alphabetical Collection of Towns, has but little Inſtruction in it, and where he would ſeem to follow ſome Order and Method, as in naming the Towns along the Coaſt of *Byzacium*, he places *Adrumettum* and *Ruſpina* after *Leptis*; thereby inſinuating, as if *Leptis* lay at a greater Diſtance from the leſſer *Syrtis*; the contrrary to which is proved eaſily from *Hirtius* and other Authors. And if with *Cluverius*, *&c.* we ſhould make the *Africa* of *Pliny*, comprehending even the two Provinces of *Zeugitana* and *Byzacium*, to be the Kingdom of *Tunis*, we ſhall meet with great Difficulties in the Geography, eſpecially of *Byzacium*, which is the Southern, and ought to be the greater Part of it. For as *Pliny* makes it only 250 Miles in Circuit, and to extend from *Adrumettum* or *Hercla* North to *Sabrata*, or to *Gaps* only, or *Tacape* South, we ſhall find that this Number of Miles will not be ſufficient to meaſure the Coaſt twice over, and therefore can lay no Claim at all to any Part of the Continent. But how far ſhort ſoever this Calculation may be of the Truth, it ſeems very probable, that the Province of *Hadrumettum*, as deſcribed by *Ptolemy*, how faulty ſoever he may be in Particulars, is the *Byzacium* which we look after, and that it included the *Blaide el Gereed*, or *Country of Dates*, which *Pliny* and the Author of the *Itinerary* ſeem to have known nothing of, or not to have regarded. For *Ptolemy*'s *Uſulitanum*, *Turza*, *Zugara*, Cities ſtill preſerving their old Names, and near upon the ſame Latitude with *Adrumettum*, continue to remain its Boundaries to the North; as *Toſer* and *Gaps*, the *Tiſuro* and *Capi*, or *Tacape* of the Antients, do the South; while *Tæney* and *Gaffſa*, or the antient *Thæne* and *Capſa*, determine the

Midland Continent. And in this Situation, * *Strabo* ſeems to place his *Byzacii*; and at the ſame Time makes the Country of the *Carthaginians* to be only the *Zeugitana* of *Pliny*, contrary to the Opinion of ſome Geographers, who give it a much greater Extent. However the *Zeugitana*, or the greater Part of it at leaſt, is ſtill called *Fregea* or *Frikæa* by the *Arabs*; and as this is without doubt a Corruption of its antient Name, ſo the Tradition of it through ſo many Ages, may perhaps be a ſtronger Argument, that this was the *Africa* properly ſo called of *Pliny*, or the Province of *Africa*, by Way of Eminence, than moſt of the Geographical Reaſons which have hitherto appeared to the Contrary.

The Kingdom of *Tunis* then contains the *Africa propria* of *Pliny*, with the *Byzacium* of *Strabo*, or the Province of *Hadrumettum* of *Ptolemy*, to which we are likewiſe to add ſo much of *Numidia* as lies half a Day's Journey, or ſix Leagues Weſt of *Keff*; for *Keff* or *Sicca Venerea* is now Part of theſe Dominions, and which *Ptolemy* and *Pliny* place in *Numidia*, though it is almoſt in the ſame Meridian with the River *Tuſca*.

Reflections on M. de Liſle's *Compariſon of the Magnitude of* Paris *with* London *and ſeveral other Cities, printed in the Memoirs of the* Royal Academy *of Sciences at* Paris *for the Year* 1725. *By* Peter Davall. N°. 402. p. 432.

VII. *M. de Liſle* in the Account of his Method of making an exact Plan of *Paris*, and comparing it with *London*, and other Cities, firſt ſhews, by what Means he proceeded in determining the true Situation of the ſeveral Places in *Paris*: After which he explains his Manner of drawing a true *Meridian Line* through that City; whereby he was enabled to divide it by *Meridians* and *Parallels*, as is practiſed in a general Map: And then he goes on in the following Words;

“ *I traced the* Parallels *from* 15 *to* 15 *Seconds, and the* Meridians
“ *from* 20 *to* 20. *And; as under the Parallel of* Paris, 15 *Degrees*
“ *of* Latitude *are equivalent to* 20 *of* Longitude, *and the like is true*
“ *of Minutes and Seconds*; *by allowing* 5 *Seconds more to the Inter-*
“ *vals of the* Meridians, *than to thoſe of the* Parallels, *I formed*
“ perfect Squares.”

He ſays, the chief Uſe he intended to make of theſe Squares, was to compare the Magnitude of *Paris* with that of *London*, and gives an Account of what Method he took to procure a juſt Plan of this City, which he reduced to the ſame Scale as that of *Paris*, and proceeds thus:

“ *I traced upon it in like Manner*, Squares *from* 15 *to* 15 *Seconds*
“ *of a* great Circle, *and then I was prepared to compare the Great-*
“ *neſs of the two Cities.*”

“ *The Reſult of this Compariſon is, that* Paris *contains* 63 *of theſe*
“ *Squares, which makes for its Superficies* 3538647 *ſquare* Toiſes:
“ *And that* London *contains only* 60 *of thoſe Squares, or* 3370140
“ *ſquare* Toiſes.”

And

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* Supra Syrtes *Pſyllos* atque *Naſamones* atque *Getularum* aliquos: deinde *Sintas* & *Byzacios* uſque ad Carthaginienſem regionem: ea enim eſt multa. *Strab. Geogr. l.* 2.

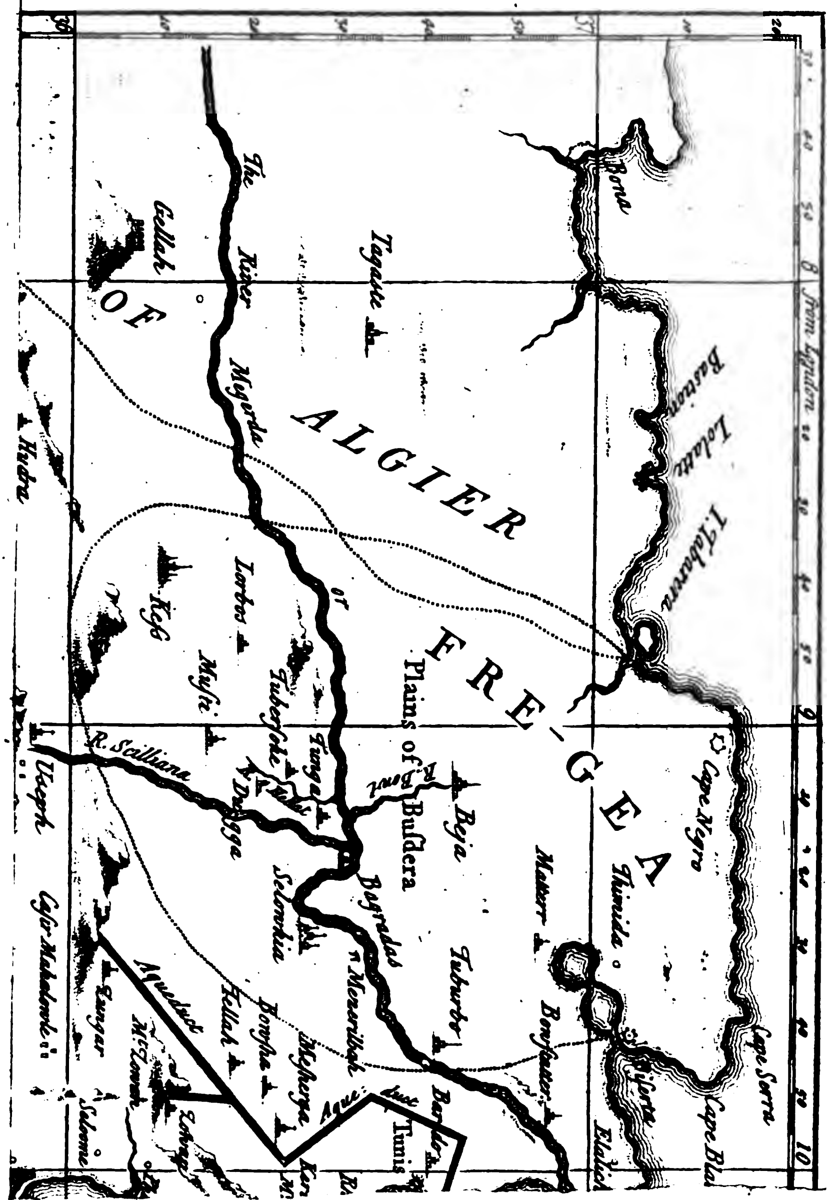

Plate XXVIII.

8' to 7, which

Lines, in a Plan of no larger a Scale than this.

Now

* Supra Syrtes *Pſyllos* atque *Naſamones* atque *Getularum* aliquos: *Byzacios* uſque ad Carthaginienſem regionem: ea enim eſt multa. *Strat*

And from hence he concludes, that *Paris* is *one twentieth* Part greater than *London*, though he ſays he has excluded ſeveral Gardens, contained within *Paris*, out of this Menſuration, which would have made it bear ſtill a greater Proportion to *London*.

Upon reading this Account of *M. de Liſle*'s, it immediately occurred to me, that the Method which he has here taken of comparing the Magnitudes of *Paris* and *London*, from whence he infers that the firſt of theſe Cities is *one twentieth* greater than the latter, is founded on a *falſe* Suppoſition, *viz.* That under the Parallel of *Paris* 20 Degrees of *Longitude* are equal to 15 of *Latitude*, and conſequently that by drawing Meridians from 20 to 20 Seconds, and Parallels from 15 to 15, the Figures formed by their Interſection will be *perfect Squares:* For the *Equator* and its *Parallels* are to each other as the *Sines* of their *reſpective Diſtances* from the *Pole*. Whence, as the *Radius*, or *Sine* of 90 Degrees, is to the *Sine* of the *Diſtance* of any *Parallel* from the *Pole*, or *Coſine* of its Latitude :: ſo is a *Degree* or any other Part of the *Equator*, or of any great Circle, to the like Part of the given Parallel. Therefore taking the mean Latitude of *Paris* at 48°. 51′, the Proportion of the Degrees of a great Circle to thoſe of the *Parallel* of *Paris* will by a Table of Sines be found to be as 1 to .6580326. Whereas according to *M. de Liſle*, that Proportion is only as 20 to 15, or as 1 to .75. The Figures therefore which *M. de Liſle* calls Squares, are not ſuch, but *Rectangles*, whoſe longeſt Side containing 15 Seconds of a great Circle, bears the ſame Proportion to the ſhorteſt, containing 20 Seconds of the Parallel of *Paris*, as .75 does to .658, &c. or nearly as 8 to 7. And the Intervals, which he ought to have allowed to the Meridians, to make perfect Squares of theſe Figures, ought to have been $\frac{15}{658}$ &c. Seconds, or nearly 22″ $\frac{4}{5}$ or 22″. 48‴ of the Parallel of *Paris*.

Now *M. de Liſle* ſays, theſe Figures are perfect Squares, and has computed them as Squares, whoſe Side was 15″ of a great Circle; for he ſays *Paris* contains 63 of theſe Squares, which makes 3538647 ſquare *Toiſes*, which laſt Number being divided by 63, the *Quote* 56169 will be the Number of ſquare *Toiſes* contained in each Square, whoſe ſquare Root gives 237 *Toiſes* for the Side of each Square, which is juſt 15″ of $\frac{1}{240}$. or a Degree of a great Circle.

M. de Liſle hath therefore by this Account made the *ſuperficial Content* of each *Rectangle*, and conſequently of the whole City of *Paris* too great by near one ſeventh. To confirm which beyond Contradiction we have *M. de Liſle*'s own Teſtimony, who in the Plan he himſelf has drawn and publiſhed of *Paris*, and which he refers to in this very Account, has not made Squares of the abovementioned Figures, but has given to their reſpective Sides the Proportion of 8 to 7, which is as near the true one as can well be expreſs'd by Lines, in a Plan of no larger a Scale than this.

Now in the Account we have been conſidering, *M. de Liſle* ſays himſelf, that in his meaſuring of *London* he drew Squares, whoſe Sides contained 15 Seconds of a great Circle, and of theſe he ſays, *London* contains ſixty.

Therefore to compare *Paris* with *London*, we ought for the foregoing Reaſons to make an Abatement out of the 63 Rectangles which *Paris* contains, nearly in the Proportion of 8 to 7; but becauſe that is a little greater than the true one; let us make ſuch Abatement only in the Proportion of 9 to 8, which is pretty conſiderably leſs than the juſt one. By which Abatement the Number of Squares, whoſe Side is 15 Seconds of a great Circle contained in *Paris*, will be reduced from 63 to 56. And conſequently, according to *M. de Liſle*'s own Way of meaſuring, the Magnitude of *London* will be to that of *Paris* as 60 to 56, or as 15 to 14; or *London* will be one fourteenth greater than *Paris*. But to determine what Proportion thoſe two Cities really bear to each other, requires a more exact Menſuration of *London* than any we yet have, which whoever would undertake, I think he cannot follow a better Method than that *M. de Liſle* has taken, and would adviſe him to conſult the Account upon which the foregoing Reflections are made, which he may find in the *Memoires* of the *Royal Academy of Sciences*, for the Year 1725. *pag.* 48.

An Account of Obſervations made on Board the Chatham-Yacht, *Aug.* 30, 31. *and Sept.* 1, 1732, *in purſuance of an Order made by the Lords Commiſſioners of the Admiralty, for the Trial of an Inſtrument for taking* Angles. *By* John Hadley, *Eſq; V. P. R. S.* N° 425. p. 341.

VIII. In *May* 1731, I communicated to the *Society* the Deſcription * of a new Inſtrument for taking Angles, and produced a Specimen of an Inſtrument made accordingly. Several of the Gentlemen to whom it was ſhewn, as well then as at other Times, entertained a favourable Opinion of the Probability of its Uſefulneſs, particularly our worthy Vice-Preſident Dr. *Edmund Halley*, Aſtr. Reg. and the Reverend Mr. *James Bradley*, Aſtr. Pr. S. not only expreſſed their Deſire that Trial ſhould be made of it at Sea, but promiſed the Favour of their Company and Aſſiſtance on that Occaſion.

The Inſtrument produced at the *Society* was made of Wood, and was intended chiefly for taking Altitudes of the Sun, Moon and Stars, from the viſible Horizon, either forwards or backwards; I therefore procured another to be made of Braſs by Mr. *J. Siſſon*, for taking the Diſtance of any kind of Objects. It is ſupported by a ſingle Stem ſcrewed on to it on the under Side, the lower End of which may reſt on the Ground, to eaſe the Obſerver of the Weight of the Inſtrument. This Stem is alſo made to lengthen or ſhorten, by which Means the Inſtrument is brought to the proper Height for any Obſerver's Eye, either ſtanding or ſitting. Inſtead of a Ball and Socket, it has two circular Arches fixed on its Back, by which it is readily ſet to any Poſition which the Situation of the Objects may require.

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* *See above;* Chap. 11. §. 6. *p.* 139.

The Right Honourable the Lords Commiſſioners of the Admiralty having been pleaſed to order the *Chatham-Yatcht* for the Trial of the ſaid Inſtrument, and to give Directions to Mr. *James Young*, Maſter Attendant at *Chatham*, a Gentleman well ſkill'd in Navigation, to be preſent at the Trial, my two Brothers and Self went on Board accordingly *Aug.* 30, being favoured with the Company (beſides the two 'forementioned Gentlemen) of the Reverend Sir *Robert Pye*, Bart. and *Robert Ord*, Eſq; Members of this Society. We met Mr. *Young* at *Sheerneſs* the next Day, who accompanied us down about three Leagues below the *Nore*, near the *Spile-Sand*, and was on Board on *Friday*, *Sept.* 1, when we lay by there, and the ſeveral Altitudes of the Sun were taken as it approached the Meridian from about Ten of the Clock 'till Noon.

The Obſervations were as follow.

	°	′	″
Aug. 30, near Midnight, Mr. *Bradley* obſerved the Diſtance of *Lucida Lyræ* from *Cor Aquilæ* by the Braſs Inſtrument off *Graveſend* in ſtill Water	34	13	30
The ſame repeated was	34	13	15
The Error of the Inſtrument in that Place is 23″ to be ſubtracted.			
	°	′	″
The Diſtance of thoſe Stars, according to Mr. *Flamſteed*, is	34	11	50
Which by the Refraction is reduced to	34	11	10
Aug. 31, about 10^{h} 30′, Mr. *Bradley* obſerved the Diſtance of *Capella* from the *North Pointer* in the *Great Bear's* Back, by the ſame Inſtrument, while we lay at Anchor in the Mouth of the *Medway* near *Sheerneſs*, the Wind blowing hard at N. E.	49	14	00+
Or	49	15	00
Mr. *Bradley* and my ſelf making a ſmall Difference in numbring the Angle mark'd by the Index.			
The Error of the Diviſion of the Inſtrument there is 30″ to be added.			
	°	′	″
The Diſtance of thoſe Stars, according to Mr. *Flamſteed*, is	49	16	00
By the Refraction reduced to	49	14	20

Clouds coming up prevented the repeating this Obſervation, nor had we any Opportunity of making any others of this kind.

Altitudes of the Sun obſerved by Mr. *Bradley*, lying at Anchor in the Mouth of the *Medway*, *Aug.* 31, Afternoon, the Wind at N. E. a freſh Gale, by the Wooden Inſtrument forwards. The Watch by the Mean of the Obſervations appeared to be about 8′ 45″ too ſlow; the viſible Horizon being ſuppoſed 3′ 30″ depreſſed below the true by the Height of the Obſerver's Eye above the Surface of the Water, amounting to about 8 or 9 Feet.

Time by Watch.			True Time.			True Alt. of Sun's upper Limb from the viſible Horizon.			Refraction, add.		Appt. Alt. of Sun's upper Limb from the viſible Horizon.			Alt. of the Sun's upper Limb obſerved.			Errors of Diviſion of the Inſtr. Subſtr.		Obſerved Alt. of the Sun's upper Limb corrected.			Errors of Obſervation.	
h.	′	″	h.	′	″	°	′	″	′	″	°	′	″	°	′	″	′	″	°	′	″	′	″
5	11	50	5	20	35	9	50	31	4	54	9	55	25	9	57	00	2	15	9	54	45	— 0	40
	16	30		25	15		7	00	5	17		12	17		13	30	2	15		11	15	— 1	2
	18	20		27	5	8	49	52	5	27	8	55	19	8	57	30	3	00	8	54	30	— 0	49
	21	20		30	5		21	51	5	44		27	35		30	00	3	00		27	00	— 0	35
	28	5		36	50	7	18	44	6	28	7	25	12	7	27	30	2	00	7	25	30	+ 0	18
	30	35		39	20	6	55	22	6	46		2	8		5	00	2	15		2	45	+ 0	37
	32	25		41	10		38	13	7	3	6	45	16	6	48	00	2	15	6	45	45	+ 0	29
	36	30		45	15		00	00	7	40		7	40		10	00	2	15		7	45	+ 0	5
	38	37		47	22	5	40	11	8	3	5	48	14	5	51	00	2	15	5	48	45	+ 0	31
	40	35		49	20		21	50	8	24		30	14		34	00	2	15		31	45	+ 1	31
	42	34		51	19		3	14	8	50		12	4		15	00	3	00		12	00	— 0	4
	43	50		52	35	4	51	24	9	7		00	31		3	30	3	00		00	30	— 0	1

Altitudes of the Sun, obſerved *Sept.* 1, before Noon, under Sail from *Sheerneſs* towards the *Spile-Sand*, with the Tide of Ebb, the Wind blowing hard at N. E. by the Wooden Inſtrument forward. The ſecond Speculum being removed by ſome Accident from its due Poſition, ſo as to increaſe the Angles obſerved about one Degree three Minutes and a half, as appeared by the firſt Obſervations of the Afternoon of the ſame Day, made with the ſame Inſtrument, in the ſame manner, while we continued lying-by near the *Spile*; and that Degree and three Minutes and a half are added to the Errors of the Diviſions of the Inſtrument in the ſeventh Column. While theſe Obſervations were making, the Yatch ſteered at firſt chiefly E. ſometimes S. E. afterwards ſtood to the N. E. towards the *Swin.* The Time of the Watch was regulated by ſome of the later Obſervations made when we were moſt Eaſtward, and this was probably the Cauſe why the firſt Altitudes, which were taken while we were more Weſterly, fall ſo much ſhort of the Computations, the Difference decreaſing gradually as we advanced towards the Eaſt.

Altitudes obſerved by Mr. *Bradley.*

Time by Watch.			True Time.			True Alt. of the Sun's lower Limb from the viſible Horizon.			Re-fracti-on, add.		Appt. Alt. of the Sun's lower Limb from the viſible Ho-rizon.			Altitude of the Sun's lower Limb obſerved.			Errors of the In-ſtrument ſubſtract.			Obſerved Altitude of the Sun's lower Limb corrected.			Errors of Obſerva-tion.	
h.	′	″	h.	′	″	°	′	″	′	″	°	′	″	°	′	″	°	′	″	°	′	″	′	″
7	9	15	7	18	15	15	5	39	3	15	15	8	54	16	9	00	1	5	45	15	3	15	— 5	39
	11	44		20	44		28	13	3	17		31	24		33	00 —	1	5	45		27	15 —	— 4	9 +
	13	38		22	38		45	23	3	8		48	31		49	00	1	5	45		43	15	— 5	16
	14	53		23	53		56	43	3	5		59	48	17	2	00	1	5	45		56	15	— 3	33
	16	33		25	33	16	11	47	3	2	16	14	49		18	00 —	1	5	45	16	12	15 —	— 2	34 +
	18	4		27	4		25	31	2	59		28	30		32	00	1	5	45		26	15	— 2	15
	23	54		32	54	17	18	00	2	50	17	20	50	18	25	00	1	6	30	17	19	30	— 1	20
	25	38		34	38		33	31	2	47		36	18		43	00 +	1	6	00		37	00 +	+ 0	42 +
	28	25		37	25		58	23	2	43	18	1	6	19	7	00 —	1	6	00	18	1	00 —	— 0	6 +
	30	44		39	44	18	19	4	2	40		21	44		28	00	1	5	00		23	00	+ 1	16
	34	21		43	21		51	10	2	35		53	45	20	00	00	1	4	30		55	30	+ 1	45
	36	24		45	24	19	9	18	2	33	19	11	51		16	00	1	4	30	19	11	30	— 0	21
	38	44		47	44		29	54	2	30		32	24		38	00 —	1	4	30		33	30 —	+ 1	6 — uncertain
	40	30		49	30		45	27	2	28		47	55		52	00 +	1	4	30		47	30 +	— 0	25 +
	42	00		51	00		58	41	2	26	20	1	7	21	4	00 +	1	4	30		59	30 +	— 1	37 —
	45	34		54	34	20	29	51	2	22		32	13		35	00 —	1	4	30	20	30	30	— 1	43

The ſame continued by Mr. *John Hadley.*

Time by Watch.			True Time.			True Alt. of the Sun's lower Limb from the viſible Horizon.			Refraction, add.		Appt. Alt. of the Sun's lower Limb from the viſible Horizon.			Altitude of the Sun's lower Limb obſerved.			Errors of the Inſtrument ſubſtract.			Obſerved Altitude of the Sun's lower Limb corrected.			Errors of Obſervation.		
h.	′	″	h.	′	″	°	′	″	′	″	°	′	″	°	′	″	°	′	″	°	′	″	′	″	
7	52	18	8	1	18	21	28	21	2	15	21	30	36	22	36	00 —	1	4	30	21	31	30 —	+0	54 —	
	54	18		3	18		45	43	2	13		47	56		52	00	1	4	30		47	30	—0	26	
	55	40		4	40		57	25	2	12		59	37	23	4	00	1	4	30		59	30	—0	7	
	58	22		7	22	22	20	34	2	9	22	22	43		30	00	1	5	15	22	24	45	+2	2	uncertain.
8	2	51		11	51		58	46	2	6	23	0	52	24	4	00	1	5	15		58	45	—2	7	
	9	19		18	19	23	53	26	2	0		55	26	25	0	00 +	1	5	30	23	54	30 +	—0	56 —	
	13	10		22	10	24	25	35	1	57	24	27	32		32	00	1	5	30	24	26	30	—1	2	
	14	45		23	45		38	43	1	56		40	39		45	00 +	1	5	30		39	30 +	—1	9 —	
	16	55		25	55		56	24	1	55		58	19	26	3	00	1	5	30		57	30	—0	49	
	19	5		28	5	25	14	34	1	53	25	16	27		22	00 +	1	5	30	25	16	30 +	+0	3 +	
	22	57		31	57		46	14	1	51		48	5		52	00 —	1	5	30		46	30 —	—1	35 +	
	25	5		34	5	26	3	35	1	49	26	5	24	27	10	00 —	1	5	30	26	4	30 —	—0	54 +	
	26	43		35	43		16	50	1	48		18	8		22	00 +	1	5	30		16	30 +	—1	38 —	
	28	20		37	20		29	54	1	47		31	41		35	00	1	5	30		29	30	—2	11	

Altitudes of the Sun, obſerved lying-by near the *Spile*, *Sept.* 1, before Noon, with the Wooden Inſtrument backward, the Wind continuing to blow hard, as before, at N. E. The Inſtrument when uſed for the back Obſervation was ſo adjuſted, as to allow for a Dip of the viſible Horizon of 2½ Minutes; conſequently that Dip being ſuppoſed, as before, 3½ Minutes, there remains only one Minute to be accounted for, in computing the Height of the Sun, which is accordingly ſubtracted in the third Column from the Altitudes found by Computation. The Watch now appeared to be 9′ 30″ too ſlow.

Altitudes obſerved by Mr. *John Hadley*.

Time by Watch.			True Time.			True Alt. of the Sun's upper Limb.			Refraction, add.		Appt. Alt. of the Sun's upper Limb.			Alt. of the Sun's upper Limb obſerved.			Errors in the Diviſion. Subſtr.		Obſerved Altitude of the Sun's upper Limb corrected.			Errors of Obſervation.	
h.	′	″	h.	′	″	°	′	″	′	″	°	′	″	°	′	″	′	″	°	′	″	′	″
9	52	55	10	2	25	36	52	54	1	11	36	54	5	36	46	00	1	00	36	45	00	−9	5
10	2	7		11	37	37	43	37	1	9	37	44	46	37	44	00	1	00	37	43	00	−1	46
	6	0		15	30	38	4	0	1	8	38	5	8	38	4	00	1	00	38	3	00	−2	8
	8	53		18	23		19	0	1	8		20	8		22	00	1	00		21	00	+0	52
	12	25		21	55		36	25	1	7		37	32		41	00	1	30		39	30	+1	58
	16	30		26	0		56	9	[illegible]	6		57	15	39	0	00	1	30		58	30	+1	15
	18	50		28	20	39	7	7	1	6	39	8	13		6	00	0	30	39	5	30	−2	43
	20	40		30	10		15	26	1	6		16	32		14	00	0	30		13	30	−3	2

The ſame continued by Mr. *Bradley*.

Time by Watch.			True Time.			True Alt. of the Sun's upper Limb.			Re-fraction, add.		Appt. Alt. of the Sun's upper Limb.			Alt. of the Sun's upper Limb obſerved.			Errors in the Diviſion Subſtr.		Obſerved Altitude of the Sun's upper Limb corrected.			Errors of Obſervation.	
h.	′	″	h.	′	″	°	′	″	′	″	°	′	″	°	′	″		″	°	′	″	′	″
10	30	18	10	39	48	39	57	28	1	4	39	58	32	40	00	00	2	00	39	58	00	−0	32
	33	23		42	53	40	10	2	1	4	40	11	6		12	00	2	00	40	10	00	−1	6
	35	53		45	23		19	51	1	4		20	55		16	00−	2	00		14	00−	−6	55+
	37	48		47	18		27	13	1	3		28	16		32	00−	2	30		29	30−	+1	14−
	39	22		48	52		33	3	1	3		34	6		36	00	2	30		33	30	−0	36
Continued by Mr. *Henry Hadley*.																							
11	8	5	11	17	35	41	59	36	1	00	42	00	36	42	15	00+	2	30	42	12	30+	+11	54+
	16	20		25	50	42	16	36	1	00		17	36		20	00	2	30		17	30	−00	6
	22	00		31	30		25	48	0	59		26	47		31	00	2	45		28	15	+1	28
	24	20		33	50		29	35	0	59		30	34		34	00	2	34		31	15	+00	41
	28	00		37	30		34	27	0	59		35	26		39	00	2	45		36	15	+00	49
	33	45		43	15		40	32	0	58		41	30		45	00−	3	00		42	00−	+00	30−
	37	45		47	15		43	43	0	58		44	41		48	00+	3	00		45	00+	+00	19+
	40	30		50	00		45	25	0	58		46	23		49	00	3	00		46	00	−00	23
	43	00		52	30		46	34	0	58		47	32		51	00	3	00		48	00	+00	28
	47	00		56	30		47	42	0	58		48	40		52	00	3	00		49	00	+00	20
again.															52	00	3	00		49	00	+	
again.			12	00	00		48	2	0	58		49	00		52	00	3	00		49	00	00	00

Between each of the five laſt of theſe Obſervations the Index was removed ſo as to make them entirely independent of one another; and from their near Agreement among themſelves, and with good Part of the preceding, I conclude the true Height of the Sun's Center above the real Horizon at Noon was exactly enough 42° 33′ his Semidiameter being 16 Min. from which, and the Sun's Declination 4° 1′ the Latitude of the Place will be 51° 28′ which is accordingly uſed in all the Computations.

Altitudes of the Sun obſerved *Sept.* 1, 1732, Afternoon, near the *Buoy of the Spile*, and under Sail Weſtward, by the Wooden Inſtrument forwards, the ſecond Speculum remaining diſplaced as in the Morning.

Altitudes obſerved by Mr. *Bradley*.

Time by Watch.			True Time.			True Alt. of the Sun's lower Limb from the viſible Horizon.			Refraction, add.		Appt. Alt. of the Sun's lower Limb from the viſible Horizon.			Altitude of the Sun's lower Limb obſerved.			Errors of the Inſtrument ſubſtract.			Obſerved Altitude of the Sun's lower Limb corrected.			Errors of Obſervation.		
h.	′	″	h.	′	″	°.	′	″	′	″	°.	′	″	°.	′	″	°.	′	″	°.	′	″	′	″	
12	7	30	12	17	00	42	12	13	1	00	42	13	13	43	20	00	1	6	0	42	14	00	+0	47	
	8	30		18	00		11	9	1	00		12	9		19	00	1	6	0		13	00	+0	51	
	12	00		21	30		7	18	1	00		8	18		13	00	1	6	0		7	00	−1	18	
	19	50		29	20	41	56	00	1	00	41	57	00		1	00	1	6	0	41	55	00	−2	00	

The ſame continued by Mr. *Henry Hadley*.

1	00	00	1	9	30	40	9	21	1	3	40	10	24	41	13	00	1	6	0	40	7	00	−3	24	uncertain
	1	35		11	5		3	20	1	4		4	24		10	00	1	6	0		4	00	−0	24	
	3	2		12	32	39	57	59	1	4	39	59	3		4	00	1	6	0	39	58	00	−1	3	
	4	21		13	51		52	57	1	4		54	1		2	00	1	6	0		56	00	+1	59	
	6	14		15	44		45	31	1	4		46	35	40	52	00	1	6	0		46	00	−0	35	
	7	30		17	00		40	28	1	4		41	32		49	00	1	6	0		43	00	+1	28	
	8	43		18	13		35	37	1	5		36	42		40	00	1	6	0		34	00	−2	42	uncertain
	10	00		19	30		30	28	1	5		31	33		38	00	1	6	0		32	00	+0	27	
	11	29		20	59		24	22	1	6		25	28		34	00	1	6	0		28	00	+2	32	
	14	23		23	53		11	46	1	6		12	52		18	00	1	6	0		12	00	−0	52	

The firſt and ſixth Columns of the preceding Tables of Obſervations are copied from the Minutes as they were ſet down at the Time. The Diviſions of the Wooden Inſtrument being not exact, I found it neceſſary to make a Table to correct them by, which was done partly by meaſuring with Compaſſes, and partly by examining them againſt thoſe of another Inſtrument. The Corrections are every where to be ſubſtracted from the Angles obſerved, and the Errors of a Degree and three Minutes and a half; occaſioned by the miſplacing the ſecond Speculum in all the forward Obſervations of *Sept.* 1, being of the ſame kind, are joined with them, in the ſeventh Column of the Tables of thoſe Obſervations. The laſt Column contains the Differences between the obſerved Altitudes, corrected by the 'forementioned Table, and the Altitudes as they ought to have appeared by the Computations. Among them there are two or three which ſo much exceed any of the reſt, that for that reaſon they ſeem to be rather owing to Miſtakes, in counting the Minutes on the Inſtrument, or the Time by the Watch, than to the Errors of the Obſervations.

The greateſt Part of the Altitudes were taken by a Horizon not clear of Land, and by that Means not always ſo readily diſtinguiſhable. The Obſervers were all Perſons quite unaccuſtomed to the Motion of a Ship at Sea, which in this Caſe was generally very great and quick, the Veſſel we were in being only of about 60 Tuns Burthen, as the Maſter informed us, the ſmallneſs of which made it alſo more liable to be lifted up and let down again by the Waves: And if the Difference of Height occaſioned by that Means was about four or five Feet, as we judged it to be, it muſt neceſſarily ſink and raiſe the viſible Horizon by Turns near one Minute. The Computations of the Sun's Altitudes are all made for the Latitude of 51° 28′, whereas a good Part of them were taken under Sail, and upon different Tacks, the Veſſel ſometimes ſtanding N. E. or N. and at other times South Eaſt, for near a quarter of an Hour at a Time.

Several of theſe Circumſtances may probably have contributed to increaſe the Inconſiſtency of the Obſervations; but as no particular Notice was taken of them at the Time, I content my ſelf with barely mentioning them.

The Principle on which the Contrivance of this Inſtrument depends, was laid down in the *Philoſ. Tranſ.* N°. 420. in one Propoſition, and ſeveral *Corollaries*, the fifth of which contains the Grounds of an Approximation for correcting ſome ſmall Errors which will ariſe if the Plane of the Inſtrument be ſuffered to vary too much from the great Circle paſſing through the two Objects, when the Obſervation is taken. There appears reaſon to think, that there will be very little Occaſion in Practice for that Correction; but it was neceſſary to mention it, in order to explain the Nature of the Inſtrument; and as the manner of deducing that Corollary from the Propoſition may not appear obvious to every Reader, I have here annexed the Demonſtration of it.

Let O B C in the annexed Figure repreſent an infinite Sphere, at whoſe Center R are placed the two Specula inclined to one another in any given Angle, and let their common Section coincide with the Diameter O R C. Let BAN be the Circumference of a great Circle, to the Plane of which the common Section of the Specula O R C is perpendicular, and B R its Radius: Let *b a n* be the Circumference of a Circle parallel to B A N, and at the Diſtance from it B *b:* Draw *b* D the Sine, and *b r* the Sine complement of the Arch B *b:* B D is the verſed Sine of the ſame. Let A be a Point of an Object placed in a Circumference of the great Circle B A N, and N the Point in which its Image is formed by the two ſucceſſive Reflections, as before deſcribed; and let *a* be a Point of another Object placed any where in the Circumference of the Parallel *b a n*, and *n* its Image; and let *a b n* be an Arch of a great Circle paſſing through the Points *a* and *n*. The Point *a* is at the ſame Diſtance from the great Circle BAN, as the Point *b*, *i. e.* at the Diſtance B *b*. Draw A R, A N, R N, *a r*, *a n*, *r n*, *a* R and *n* R. Fig. 199.

By the fourth Corollary the Figures A R N and *a r n* are ſimilar, and conſequently the Line A N is to the Line *a n* as A R or B R is to *a r* or *b r*, *i. e.* as the Radius is to the Sine complement of the Diſtance B *b*. But A N is the Chord of the Arch A H N of the great Circle B A N equal to the Tranſlation of the Point A, or double the Inclination of the Specula, and *a n* is the Chord of the Arch *a b n* of a great Circle, meaſuring the Angle *a* R *n*, by which the Point *a* appears removed by the two Reflections, to an Eye placed in the Center R. Therefore the Tranſlation, or apparent Change of the Place of the Point *a* is meaſured by an Arch of a great Circle, whoſe Chord is to the Chord of the Arch AHN (equal to double the Inclination of the Specula) as the Sine complement of its Diſtance from the great Circle B A N is to the Radius.

From any Point C of the Circumference O B C, draw the Chords C M and C *m*, to the ſame Side of the Point C, and equal to the Chords A N and *a n* reſpectively, draw the Radius R M, and from R and *m* draw R Q and *m* P, both perpendicular to C M, and cutting it in Q and P. R Q is the Sine complement, and C M double the Sine of half the Angle M R C, or A R N, or of the Angle of Inclination of the Specula. The little Arch M *m* will repreſent the Difference of the apparent Tranſlations of the Objects in A and *a*; and if it be very ſmall, may be looked on as a ſtrait Line, and the little mixed Triangle M *m* P as a rectilinear one, which will be ſimilar to RMQ, becauſe R M is perpendicular to M *m* and R Q to C M, and the Angles at Q and P right Angles. The Line C P may be taken as equal to C *m*, and M P as the Difference of the

Lines C M and C *m*. Therefore the little Arch M *m* is to the Line M P nearly as R M to R Q: But C M (*i. e.* A N) was to C *m* (*i. e. a n*) as B R to *b r*, and the Difference M P of C M and C *m* to the Difference B D of B R and *b r* as C M to B R. Therefore M *m*, the Difference of the apparent Tranſlations, is to B D, the verſed Sine of the Diſtance B *b*, or to an Arch equal to it, in the compound Ratio of R M the Radius to R Q the Sine complement of the Angle of Inclination of the Specula, and C M double the Sine of the ſame to B R the Radius, *i. e.* as C M to R Q.

The Obſervation may be corrected by one eaſy Operation in Trigonometry, as will appear from the firſt Part of this Corollary, *viz.* by taking the half of the Angle obſerved, and then finding another Angle, whoſe Sine is to the Sine of that half, as the Sine complement of the Diſtance B *b* is to the Radius: This Angle doubled, will be the true Diſtance of the Objects. But as this Operation, though eaſy, will require the uſe of Figures, I rather choſe the Method of Approximation, becauſe by that the Obſerver, retaining in his Memory the Proportions of the Sines of a few particular Arches to the Radius, may eaſily eſtimate the Correction without Figures, when the Angle is not great, and by a Line of artificial Numbers and Sines, may always determine it with greater Exactneſs than will ever be neceſſary.

When the Angle obſerved is very near 180 Degrees, the Correction may be omitted; for then it will be eaſy to keep the Plane of the Inſtrument ſo near that of the before-mentioned great Circle as not to want any, if the Situation of that Circle be known: If it be not, the Obſerver, when he ſees the two Objects together, may turn the Inſtrument on the Axis of the Teleſcope, 'till he finds that Poſition of it by which he obtains the leaſt Angle; and this (if the Specula are ſet truly perpendicular to the Plane of the Inſtrument) will always happen when the Objects appear to coincide in the Line *g b*, as expreſſed in Fig. 89 *.

In Page 142. a Rule is given for finding to which Hand of the Obſerver the Object ſeen by Reflection ought to lie, but is reſtrained to the particular Form of the Inſtrument there deſcribed. The general Rule is, that when the Index is brought to the beginning of the Scale (*i. e.* to 0° when the Inſtrument is deſigned for Angles under 90°, or to 90° when it is deſigned for Angles from 90° to 180°) if then a Line be imagined to be drawn on it parallel to the Axis of the Teleſcope, or Line of Direction of the Sight, ſo as to point towards the Object ſeen directly; which ever way this Line is carried by the Motion of the Index along the Arch from 0° towards 90° in the firſt Caſe, or from 90° towards 180° in the ſecond, the ſame way the Object ſeen by Reflection ought to lie from that which is ſeen directly.

IX. The

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* Chap. 11. §. 6.

A Method for Rowing Men of War in a Calm Communicated by Mr. Du Quet, Nº. 369. p. 239.

IX. To perfect the Art of Navigation, two Things ſeem principally wanting. An eaſy Method for finding the Longitude at Sea; and a Way to give a Veſſel its Courſe, when there's no Wind ſtirring. I flatter my ſelf to have found the laſt; and hope to make it appear, that a Man of War may make a League an Hour in a Calm, by Means of revolving Oars, which are eaſily apply'd to the Sides of the Ship, without occaſioning any Incumbrance.

They take the Water perpendicularly, and enter far enough not to miſs it: And if the Water ſhould happen to evade the Stroke, the Rowers would not be much incommoded; becauſe they would be ſupported at every Vibration, which is only of three Foot. Beſides, in the Uſe of inclined Oars, more than half the Time is loſt, in raiſing and recovering the Oar, before they give the Stroke; which makes the Veſſel move by jerks, ſo that the People aboard feel (as it were) every Stroke of the Oars when they play; whereas, the revolving Oars always move equally, and ſucceed one another without Loſs of Time; which makes the Veſſel move uniformly, without affecting thoſe who are aboard. It is to be obſerved too, that a Gally built on purpoſe for the Uſe of inclined Oars, would not be ſo proper as another Veſſel for perpendicular Oars; becauſe the Gally has a conſiderable Length and but little Height above the Water.

Having propoſed this Invention to the Court of *France*, I was ſent to *Havre de Grace*, to make a Tryal, which had the Approbation of the Intendant. He made his Report, That the Officers at firſt objected to the Invention; but as for his own part, the more particularly he conſider'd it, the more he was convinced of its Uſefulneſs. I was afterwards ſent to *Marſeilles*, where I made ſeveral Tryals on board a Gally; the Swiftneſs of which was compared with that of another Gally, equipped as uſual. *M. de Chazelles*, a Member of the *Royal Academy of Sciences*, and Engineer of the King's Gallies, had Orders to make his Obſervations, and ſend them to Court: A Copy of which he gave me, ſign'd with his own Hand, and is as follows.

The Report of Monſieur de Chazelles.

An Experiment of the Swiftneſs of a Gally, with perpendicular revolving Oars, invented by Monſieur Du Quet; *compared with that of a common Gally. Made at* Marſeilles, &c.

At 10 h. 3 min. in the Morning, the *Superbe* Gally quitted her Station over-againſt the *Auguſtins*, in order to fall down to the *Chain*.

At 10 h. 11 m. ſhe came to the *Chain*.

At 10 h. 6 min. the *Machine* Gally quitted her Station, at the innermoſt Part of the Port. She had three *Machines* on each ſide.

10. 13. She came to the *Chain.*

10. 19. The two Gallies abreaſt. Both row with their whole Crews.

10. 25. The *Superbe* paſſes; and then rows only with the hinder part of her Crew.

10. 27. The *Machine* Gally paſſes.

10. 28. Both row with their whole Crews.

10. 30. The *Superbe* Gally paſſes; and then rows only with the fore-part of her Crew.

10. 32. The *Machine* Gally paſſes; upon which the *Superbe* Gally claps no more Oars, till ſuch Time as ſhe has acquir'd the ſame Velocity with the *Machine* Gally: And it appeared, that with ſeven or eight Oars leſs than her Complement on each ſide, ſhe kept up with the *Machine* Gally; making about 200 Rowers, which was the Number of the *Machine* Gally's Crew. There was a little Wind a-head, which retarded the *Superbe* ſomething more than it did the *Machine* Gally; becauſe the *Superbe* had her Maſts and Yard-Arms ſtanding, and the other not.

10. 43. Came to the Moorings of the *Iſles.* The Sign given for turning.

10. 47. The *Superbe* was come about. It appeared, that the *Machine* Gally was conſiderably quicker in turning than the *Superbe.*

10. 30. They came again into Port.

By this it appears, that the *Machine* Gally has a conſiderable Advantage over the common one, in quitting her Station, and acquiring her firſt Motion: For, in ſeven Minutes, ſhe ran the whole Length of the Port; having quitted her Station by means of her Oars, without towing her ſelf by her Moorings; which is what another Gally would not have effected, but very ſlowly. And the *Superbe* Gally, after ſhe had moved from her Station, was 8 Minutes in going a leſs Diſtance than the Length of the Port. But if we conſider the Experiment made without the Harbour, it ſeems to prove the common Gally to have the Advantage over the *Machine* Gally, though the Number of Hands be equal. For, with 8 Oars leſs than her Complement on each ſide, ſhe kept up with the *Machine* Gally, notwithſtanding the greater Reſiſtance of the Wind againſt her Maſts. However, if we conſider, that the Crew of the *Superbe* was a great deal better than that of the *Machine* Gally; that the *Superbe* is acknowledged to be one of the beſt Sailors the King has; whereas that which had the *Machines,* is an old decayed Gally, and reckoned a very bad Sailor; beſides that the Crew of the *Superbe* are much better acquainted with the common Oar, than the others are with the new way of rowing; and that in the common Gally there's no Improvement to be made, either with reſpect to the Proportion of the Oars, their Length, the Breadth of the Pallets,

lets, the Height of the Point of Reſt, *&c.* or with reſpect to the Conſtruction of the Veſſel; whereas in the *Machine*, there are ſeveral Things to be improved and altered in the Oars, the Hand-ſpikes, and in diſpoſing the Men to the beſt Advantage. Theſe Things, I ſay, conſidered, it ſeems reaſonable to believe, that a Veſſel with the *Machines* might go faſter than one with the common Oars; becauſe the Loſs of Time is avoided, which happens in the ordinary way of rowing.

This Experiment, however defective it be, for the Reaſons above, will prove, That the Velocity is greater in this way of rowing than in the other, when the Circumſtances on both ſides are equal. For, by my Journal, I find, that the *Patronne*, in Company with fourteen other Gallies, left the Port of *Marſeilles* at 50 min. paſt three; and rowing all in a Calm, came to the *Iſles* at 4 h. 23 min.; which made 33 m. in going from the *Chain* to the *Iſles*. But the *Machine* Gally made the ſame way, with 200 Men, in 30 min. having left the *Chain* at 10 h. 13 m. and arrived at the *Iſles* at 10 h. 43 min. altho' there was ſome Wind a-head. Signed CHAZELLES.

This Experiment ſhews, that the *Impetus* does not depend upon the Number of Oars, but the Number of Men. A Veſſel charged with revolving Oars, will go as faſt in a Calm with 100 Men, as it would do if towed by a Gally of 200 Men; becauſe there will be one Gally leſs to draw along.

Another Memoire of Monſieur Chazelles, *concerning the Uſefulneſs of perpendicular revolving Oars, invented by M.* Du Quet.

The Experiment made of the New *Machine*, although defective by reaſon of the Difference there was with reſpect both to the Crew and the Veſſels, does yet leave room to expect a conſiderable Advantage from this Invention, in giving the Ship way: For though the common Gally ſhould keep up with the *Machine* Gally at their firſt ſetting out, with equal Number of Hands; 'tis evident, the *Machine* Gally will get the better at long Run, when the others Crew are ſo fatigued, as to be obliged to row by turns. For here the Men will hold a longer Time, their Action not being ſo great, nor ſo violent. Beſides, having only 200 Men employed, and being equally manned with the other Gally, freſh Hands may be ſupplyed, and ſo they will continue to go at the ſame Rate: For in caſe of Need, the Marines may be employed in this Service; which they will perform with as little Reluctance, or Trouble, as they work at the Capſtane.

The Reaſon of this Increaſe of Velocity appears plain, if we conſider the Difference between the common way of rowing, and that by perpendicular Oars: The laſt is done by an uninterrupted Application of Force, in the ſame Direction; the other acts by Jerks. And of the three Parts of Action that are employed, in order to give the Strokes; one in raiſing the Oar out of the Water, the ſecond in advancing the Hands forwards, and the third in preſſing againſt the Water; only the laſt turns to Account: And that ſtill loſes ſomething of its Efficacy; for the Crew, by their falling back all together, make the Veſſel plunge, and render its Motion oblique, which contributes very much to its Decay.

Theſe are not the only Defects of the common Oars; for, in order to augment their Force, the Number is to be increaſed, and conſequently, the Veſſel muſt have a greater Length; by which means, it is rendered weaker and leſs able to reſiſt the Force of the Sea. Beſides, the Veſſel muſt be low-built, and uncovered, (and ſo more expoſed to the beating in of the Waves) by reaſon they are obliged to proportion the Length of the Oar to the Strength and Size of the Men. And though the Crew ſhould be under ſome Covert, as they are in a Galeaſs; an Opening muſt be left for the Oars to play, by which the Waves may beat in.

Both theſe Inconveniencies are avoided, by the perpendicular Oars; becauſe the Addition of Force may be obtained, by only applying more Hands to the *Machine*; ſo that with two or three *Machines* on a Side, there will be more or leſs Force, in proportion to the Number of Men employed, and the Length of the Veſſel may be leſſened at Diſcretion. And to guard againſt the Sea, another Deck may be made, ſhut cloſe on all Sides, even where the *Axis* of the *Machine* paſſes through.

The chief Objections againſt this Invention, ſeem to me ſufficiently obviated by M. *Du Quet's Memoire:* But though the whole of what is objected ſhould indeed prove, that a Veſſel made for ſailing, as the common Gally, would be ſo incumbered with the *Machines*, as to make the Uſe of Sails impracticable; yet if it ſtill holds true, that ſhe will move faſter; as appears, both by Reaſon and Fact; it muſt be allowed, that a Veſſel might be ſo commodiouſly conſtructed, to carry theſe *Machines*, as to go as faſt as a Gally in a Calm, and better endure the Weather when under Sail.

Such a Veſſel would have ſeveral Advantages above a Gally, both in Sailing, and in Fight; not to mention the Conveniencies of lodging the Crew. She may put off to Sea any where, and thereby avoid the Dangers attending the Coaſt-Winds, which Gallies find to be a-head as ſoon as they have doubled certain Capes; and ſo they find themſelves between two Winds, which there would be no Danger of, farther out at Sea. With reſpect to Fight, ſhe may mount Cannon fore and aft, and on each ſide; and even Mortar-pieces. In Time of Battel, ſhe would be of wonderful Uſe; for ſhe would take and maintain her Poſt without Aſſiſtance, either at the Head, or the Rear of the Enemy's Line, and there make uſe of her Bombs: Beſides the Advantages of towing off other Veſſels from their Danger in a Calm, and of boarding, or making off from the Enemy. And this holds in Ships of any Rate; provided the Length of the

Oars, the Breadth of the Pallets, and the Strength of the Handſpikes be proportionable. And the moving Force will always be in proportion to the Strength and Number of the Men employed, and not to the Number of *Machines*, as in the common Oars, which too are impracticable in Ships above the fourth Rate, by Reaſon of their great Length, which will be diſproportionate to the ordinary Bulk of a Man.

By this means the Crew will be free from the Fatigue of towing, and the Veſſel will move incomparably faſter than if it was towed; becauſe the Chaloups which tow, are ſubject to the Inconveniencies of the common Way of towing, by loſing two thirds of the Time; and beſides, they can't act all together: And the Veſſel that is towed, pulling them back after the Oar has made its Stroke, they have ſo much of the Space to regain by the next Stroke. Beſides, the Cable by which they tow, ſinking into the Water by its own Gravity, the Reſiſtance the Water makes to its Return, is to be over-balanced; all which Circumſtances together conſiderably diminiſh the towing Force.

Beſides, this Invention is not ſuch as is deſtructive to Mankind, and becomes uſeleſs to the Nation that firſt puts it in Practice, when generally known; on the contrary, it may be greatly advantageous to the Inventors at the Beginning, and every where ſerviceable on many Occaſions, when it is put in Practice by thoſe who uſe the Sea. Signed CHAZELLES.

M. de Chazelles might have added, that the Chaloups that tow, are in cloſe Fight liable to be ſunk by the Enemy's Cannon, and are expoſed to the Waves by their having ſo little Height above Water.

The chief Advantage, and which includes all the reſt, is, that let a Veſſel crowd as much Sail as poſſible, the perpendicular Oars are always capable of increaſing her Swiftneſs, becauſe the Rowers have only a Motion of 3 Foot to make one Way, and as much the contrary Way, in order to make the Oars deſcribe 54 Foot Space in the Water, and that Motion of 6 Foot might be performed in two Seconds of Time, if the Oars met with no Reſiſtance; conſequently the Veſſel muſt run 54 Foot in two Seconds, that is, about 6 Leagues an Hour, before thoſe revolving Oars be unſerviceable, for then the Veſſel would go as faſt as the Oars could poſſibly move with a Diameter of 18 Foot; and if it was neceſſary to make them move faſter, it is only lengthning out their Diameter, and they would move ſo much the faſter, without obliging the Rowers to increaſe their own Motion.

Mr. *Arnoult* was ordered to examine the new Oars, and he made his Report to the Court, that the Officers of the Gallies found, that they interfered with the Uſe of the Sails in a Gally, but might be of Uſe in other Veſſels and Bomb-ketches; in Conſequence of which, I was ſent to *Toulon* to make the Experiment on Board a Bomb-ketch.

At the Time when the Experiment was made, *M. de Vauvre*, and the Officers of the *Marine* were at Sea, and only ſome Officers of the *Port* were preſent, who ſent a Verbal Proceſs to the Court, without acquainting me with it, or offering any Objection, although I had very much preſſed them to it, in order to obviate the Prejudices might be conceived againſt this Novelty.

At my Return to *Paris*, *M. de Salabery*, ſurpriz'd at my knowing nothing of that Account. gave it me to anſwer, which I did Paragraph by Paragraph: The whole was given to a general Officer then at Court to examine, and make a Report of it, the Reſult of which was, That this Invention ought to be put in Practice.

An Account of a new Machine, called the Marine Surveyor, *contrived for the Menſuration of the Way of a Ship in the Sea, more correctly than by the* Log. *By Mr.* Henry de Saumarez. N°. 391. p. 411.

X. 1. The *Primum Mobile*, or Soul of this Machine, is in the Form of the Letter Y, and is made in Iron, or any other Metal: At each End of the Lines, which conſtitute the Angle, or upper Part of that Letter, are two Pallets not much unlike the Figure of the Log; one of which falls in the ſame Proportion as the other riſes. The falling or pendent Pallet meeting a Reſiſtance from the Water, as the Ship moves, has, by that Means, a circular Motion under Water, which is faſter or ſlower, according as the Veſſel moves. This Motion is communicated to a Dial within the Ship (which is fixed either in the Maſter's Cabbin, or any other proper Place) by means of a Rope (of any convenient Length) faſten'd to the Tail of the Y, and carried to the Dial. The Motion being thus communicated to this Dial, which has a Bell in it, it ſtrikes exactly the geometrical Paces, Miles, or Leagues, which the Ship has run. Thus is the Ship's Diſtance attained; and with equal Eaſe may the Forces of Tides and Currents be diſcovered by this Inſtrument.

Fig. 200.

AKCL and BHDI are the Pallets, which are worked from the Legs DE and CE into the Form they appear, to a Breadth of about $4\frac{1}{2}$ Inches. The Length of the Pallets (BD and AC) are 8 Inches. The Branches or Legs, DE and CE, are each 15 Inches and a half long, and 2 in Circumference, the Diameter of which is about $\frac{2}{3}$ of an Inch; and the Angle CED, which is contained between them, is 45 Degrees. The Shank EF is of the ſame Thickneſs or Circumference with CE and DE, and is 27 Inches long. At the Point F there is a Ring, where one End of the Rope FG is hooked to the Machine, the other End G being fixt to the Dial within the Ship or Veſſel. This Rope may be about 5 Fathoms, more or leſs, according as the Dial is fixed high or low, in reſpect to the Surface of the Water.

In the Figure this Machine has but two Branches; however, it may be formed of three, if not four, and adjuſted to the ſame Standard or Meaſure: But as three or four Branches would be more ſubject to entangle themſelves in Sea-Weeds, and thereby prevent the regular Motion of the Inſtrument, if not in ſome Meaſure impede the Ship's Way, I cannot but recommend their being made only of two Branches, in the Manner I have laid down; for, in my own Experiment at Sea, I have obſerved thoſe made in this Form have been ſo far from being choak'd by Weeds, that if they encountered any at any Time, they have always cleared themſelves of them, without the Trouble of hauling the Engine into the Ship to do it. This Inſtrument may be regulated ſeveral Ways; as firſt, by opening or cloſing the Angle C E D; ſecondly, by lengthening or ſhortening the Branches, or turning or bending more or leſs the Pallets A K C L and BHDI; and ſo in this Manner the Machine is brought to what Standard or Meaſure you pleaſe, to make the hydraulical Revolution to anſwer either to a geometrical Pace of 5 Feet, or to 10, 12, 14 Feet, *&c.* The Machines of this Kind, which I have tried at Sea in all Sorts of Weather, did weigh ſome 4, others 5, and others 6 Pounds; the Weight of them not at all affecting the peculiar Property of the Inſtrument, or hindering the Regulation thereof according to the Methods I have laid down. Theſe Machines may be made of Tin as well as Iron, and ſo light as not to weigh above two or three Pounds, which may ſerve for any Boat, Wherry, Barge, *&c.* without any Hindrance to their Rowing or Sailing. The Manner of fixing them to a Ship, or Boat, is repreſented in *Fig.* 201. Fig. 201.

I come now to the Explanation of the three ſeveral Dials, and one of which may be uſed with this Machine. The firſt Dial had 3 Indexes, one of which mark'd 10 Revolutions of the Engine, each Revolution 10 Feet; ſo that of conſequence the whole Round of the Circle was 100 Feet. As five of theſe Revolutions make 50 Feet, which I reckon to be (or at leaſt ſhould be) the Diſtance marked between each Knot on the Log-Line now in Uſe at Sea; by holding the Half-minute Glaſs in one's Hand (which is always uſed with the Log-Line) one may, by Inſpection, ſee how many Times 50 Feet ſhe runs in half a Minute, and of courſe how many Miles in an Hour, without the Trouble of employing 4 or 5 Hands, as there generally is, in heaving the Log. My ſecond Index on this Dial marked 100 Revolutions, which makes 1000 Feet, as the third Index did 1000 Revolutions, which is equal to 10,000 Feet; and then a little Bell ſtruck, ſignifying when the Ship had ſailed that Diſtance, which may be alſo fitted to ſtrike to any other Meaſure. My ſecond Dial had the Circle on its Plate divided into twelve Parts, ſo that as the Index paſt each Diviſion, the Ship had run one Mile, and conſequently 12 Miles, when it had meaſured the Circumference. On one Side of this Dial, I had fixed another Plate, which was graduated in ſuch Manner, that by the Half-minute Glaſs I could alſo, by Inſpection, tell what the Veſſel run in that Space of Time, *&c.* On my third Dial I had three Circles; the firſt was ſo divided, as to ſhew when the Ship had run 60 Leagues; the ſecond was ſo contrived, as to ſhew when the Ship had run the ſame Diſtance in Miles;

and on the third was marked 120 Knots; ſo that computing each Knot at 50 Feet, the Circumference was 6000 Feet, which I take to be the Standard of an *Engliſh* Maritime Mile, or the $\frac{1}{60}$ Part of a Degree upon the Equator; in running which Length, my Inſtrument has juſt 600 Revolutions; to which Diſtance a little Bell ſtrikes to give Notice, to the Man at the Helm, of the Diſtance ſailed in that Time. Beſides the ſeveral Circles on this Dial (graduated as I have mentioned) I had alſo two Plates on each Side, having two Circles; one divided into 100 Leagues and the other into 300 Miles; ſo that, without hearing the Bell ſtrike to every Mile or League, one might at any Time ſee by them, what Number of Miles or Leagues the Ship had run, from the Time ſhe had left her Port. As to the Materials within the Dial, there is little more than common Clock-work.

As by this Machine I undertake to correct the Errors of the Log, I flatter my ſelf that a Compariſon between that Inſtrument, and my Invention, will not be unacceptable to the Curious.

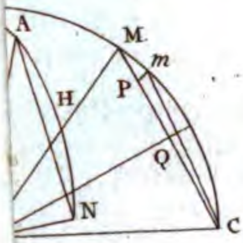
A Comparative Diſcourſe between the Log *and my Inſtrument, which I chuſe to call the* Marine-Surveyor

'The firſt Error I chuſe to touch on, in relation to the Log, is in the half and quarter Minute Glaſſes; I think I may well affirm, that they are ſeldom or never true, in regard it rarely happens that we can find two to finiſh their Courſe in the ſame Space of Time; yet, if they did run their Sand out equally, it is no Demonſtration of their Truth, ſince two, that are falſe, may do the ſame, as well as two that are true. But, admitting they were never ſo truly made, they are notwithſtanding ſubject to Error, ſince it is but too well known, that dry and wet Weather have a great Influence on them. Should the Half-minute Glaſs lack but two Seconds, or be two Seconds too long, it makes an Error of ſome Miles in 24 Hours. If the *Log* be hove by Quarter-minute Glaſſes, in like manner defective (which is the general Practice, when the Ship has great Way) in doubling the Knots, the Error is alſo doubled. Beſides, when the Ship runs after the Rate of 8 or 9 Miles an Hour (and the Line is left to run off of the Reel) it rarely happens but ſome Fathoms are out, before the Line can be ſtopped; though this may be ſmall in the Courſe of 24 Hours, and therefore diſregarded; yet in a long Voyage it will make a great Addition to the many Errors in the Diſtance (which we gain by the *Log*) which, added to thoſe of our Judgment, occaſions ſo many that keep Journals at Sea, to be a Shore, when they have reckoned themſelves, 50, 60, or more Leagues from the Land; and others to be as many Leagues from their Port, at the Time when they have expected to make it.

'In the *Marine-Surveyor* it is not ſo; for this Inſtrument requires no Glaſſes of any Kind: Let the Ship run faſt or ſlow, it is the ſame, for it works in Proportion, and the Bell ſtrikes to every Mile accordingly. To evidence the Truth of this, I take Leave to

e 446.

9.



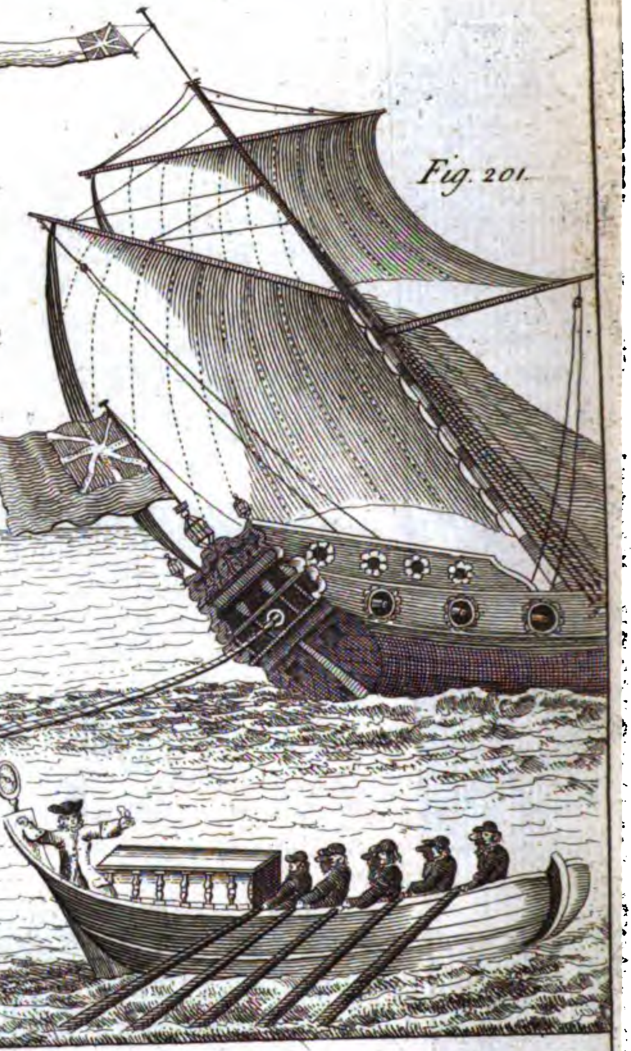
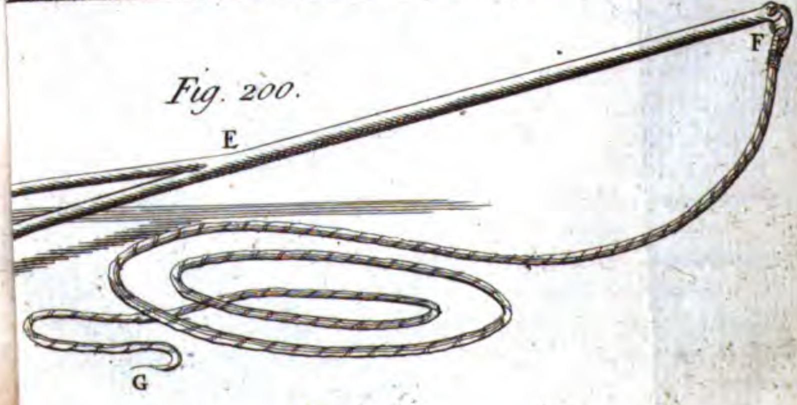


Fig. 201.

Fig. 200.



‘ mention an Inſtance, *viz.* When I was making my Experiments on the Canal, in St. *James*'s Park, Dr. *Deſaguliers* and ſeveral other Mathematicians, at Times, were with me, and we meaſured out a certain Diſtance there; upon which I fitted my Machine to ſtrike to that Diſtance, and accordingly it did ſo. We then altered the Motion of the Boat, and rowed much faſter to the Mark than we had done before; however, the Bell ſtruck, when we came up to it, to the greateſt Exactneſs: And ſuch is the Property of this Inſtrument, that it may be fitted to ſtrike to Miles, Leagues, &c. as ſhall be thought proper. This Machine is made of Materials ſo durable that one of them ſhall laſt 50 or 60 Years; and ſuch is the Price, that they will prove as cheap or cheaper to the Government, than the *Log*, which is attended with an Expence of ſo many Lines, Glaſſes, &c. As for the making a Trial of this Inſtrument, it may be as fully done in the Channel, as in an *Eaſt-India* Voyage; for if it anſwers to 20, 30, or 40 Leagues, the Reaſon holds good for as many Thouſand.

2. ‘ The chief Property of the *Log* is to have it ſwim upright, or perpendicular to the Plane of the Horizon. This is too often wanting in *Logs*, becauſe but few Seamen examine whether it is ſo or no, and generally take it upon Truſt, being ſatisfied, if it weigh a little more at the Stern than the Head. What erroneous Reckonings flow from hence is but too evident; for if the *Log* does not ſwim upright, it will not hold Water, neither remain ſteady in the Place where it is heaved, ſince the leaſt Check of the Hand, in veering the Line, will make it come up ſeveral Feet. This repeated, the Errors become Fathoms, and perhaps Knots, which, how inſignificant ſoever they may ſeem, are Miles and Parts of Miles, and amount to much in a long Voyage.

‘ In anſwer to this, the *Marine-Surveyor* is of ſuch a Property, that there is no Neceſſity to take Care about its ſwimming; and it is a conſtant Truth, peculiar to this Inſtrument, that be the Ship's Motion on the Water what it will, whether ſhe runs one Mile faſter or ſlower than another, yet all ſhe runs, is exactly marked on the ſaid Inſtrument, as appears plainly from ſome Tables of Experiments made by me in the River *Thames*, for obtaining the gradual Increaſe and Decreaſe of both Ebb and Flood.

3. ‘ The ſtretching and ſhrinking of the *Log-Line*, is another great Error in the Uſe of the *Log*; for when a new Line is firſt uſed, let it be ever ſo well ſtretched upon Deck, and meaſured as true as poſſible, it ſhrinks after wetting conſiderably; and therefore if we rely on the Line run out for the Ship's Diſtance, we ought to meaſure and alter the Knots on it every Hour before we uſe it; but I am well aſſured that this is ſeldom done oftner than once a Week, and ſometimes not above once or twice in a Voyage. What great Dependance then is there on a Reckoning kept by

'the *Log*? Since in this Caſe the Line will ſhrink ſo, as to add Miles 'to the other Miſtakes of every 24 Hours. Again, when the Line 'is meaſured to its greateſt Degree of Shrinking, it is generally left 'there; and when, by much Uſe, it comes to ſtretch again, it is 'ſeldom or never mended, although it will ſtretch beyond what it 'firſt ſhrunk. In ſhort, ſuch are the Errors incident to the *Log*, 'that I don't wonder at our Neighbours the *Dutch* for preferring 'their *Chips* or an irregular Pulſe to it; which conjectural Reckoning of theirs is obtained after the following Manner. They fix 'two Marks on the Side of the Ship at a certain Diſtance, when an 'experienced Perſon, ſtanding at the foremoſt Mark, throws a 'Chip over-board, and counts the ſeveral Beats of his Pulſe, during 'the Chip's Paſſage from one Mark to the other; and from thence 'it is they compute the Number of Miles that the Ship runs in an 'Hour.

'As for the *Marine-Surveyor*, it is not hove with a Line, but is 'tow'd a Stern by a Rope; and let that Rope ſtretch or ſhrink (be 'long or ſhort) it is all one, for the Inſtrument will have the ſame 'true Revolutions. Should it be objected, that it holds Water, I 'affirm, from my own Experiments of it, that the *Log* hauled in 'from 5 or 6 Knots, is much heavier upon the Hand; and that the 'faſter the Ship runs, the leſs Water this Inſtrument of mine holds, 'becauſe it gives Way to the Water and turns quicker; nay, I can 'venture to ſay, that it is ſo far from being any conſiderable Impediment to the Ship's Way that ſhe does not loſe one Mile in an 'hundred by it. But ſhould this Inſtrument be introduced into the 'Navy, in caſe of chaſing an Enemy, or the like, it may be taken 'in at any Time, and let down again at Pleaſure.

4. 'I appeal to all Seamen, if in a moderate Gale, when the Ship 'runs 5 or 6 Knots, two different Perſons (every way qualified) 'were to heave the *Log* immediately after one another, whether they 'would exactly agree. Surely no. Since 'tis but Chance if they do 'ſo, and is what may not happen in a hundred Trials. I therefore 'affirm the *Log* to be very erroneous on this Account, and that the 'Error frequently increaſes with the Wind; for in a ſtiff Gale, when 'a Ship has run about 8 or 9 Knots before the Wind, it has been 'known that two expert Seamen have hove the *Log* in this Manner, 'and on their comparing Notes, they have found a Knot Difference; 'ſometimes it has been more, and at others leſs, which muſt certainly make a ſtrange Confuſion in the Reckoning. Under this 'Head I take leave to obſerve, that when the *Log* is hove, it is 'ſometimes in ſo ſtrong a Gale, that the Ship runs 9 Knots; but 'before it is hove again, there may be ſuch a Decreaſe of the Wind, 'that for half of the Hour ſhe may not run above 5 Knots. Her 'true Diſtance ſailed then, is the Mean between the Extremes of 9 'and 5; but this has been ſo far from being conſidered by ſome

'*Chalkers of the Log-board*, that it is but too well known, the Extremes have been put for the Mean, and the contrary. Were there 'Truth in the *Log*, two Ships in Company would nearly have the 'ſame Account; but it is otherwiſe; for we too often find many 'Leagues Difference in Reckonings, even on board the ſame Ship. 'In a Word, ſuch Errors have been found in the *Log* by ſome of my 'Acquaintance, that when they have ſailed between a Meridian and 'a Parallel, the whole Difference on the *Log-board* has not proved '*Difference of Latitude* enough to agree with their *Obſervation*, al'though each Day they had a good obſerved Latitude, and no Cur'rents.

'In the *Marine-Surveyor* we are ſo aſſured of the Ship's Diſtance, 'that all Ships ſhall all agree which are in Company, as to their 'Reckonings, ſave that ſome Allowance be made for Difference of 'Judgment in the ſeveral Perſons who keep Journals.

There are ſeveral other Caſes equally, if not more momentous than what I offer here, wherein the *Marine-Surveyor* will be found to have the Preference of the *Log*.

The following are the Subſtance of two Affidavits, taken under the Seal of the Royal Court at *Guernſey*, by ſome expert Seamen, who have had Trial of my Inſtrument, *viz.*

'Know all Men by these Presents, that on the 30th of '*November* 1720, there perſonally appeared before *William Le Mar'chant*, Eſq; (Judge Delegate in the Iſland of *Guernſey*, &c.) Meſ'ſieurs *Jean Andros*, and *Eleazar Le Marchant* (Jurats of the Royal 'Court of the ſaid Iſland.)

'*William Abier*, aged about 40 Years, who commanded ſeveral 'Privateers in the late War, (and particularly that called *La Chaſſe*, 'of about 150 Tun, 16 Guns, and 140 Men) and is now Maſter 'of the Ship called the *Eagle*, of which Veſſel he is the only Pro'prietor, who voluntarily makes Oath, that on *Sunday* the 9th of '*October* 1720, he parted from *Southampton* with ſeveral Gentlemen 'Paſſengers on board for *Guernſey*; that he had fixed at the Stern 'of his Ship a new Invention called the *Marine-Surveyor*, project'ed, to the beſt of his Knowledge, by Mr. *Henry de Saumarez*, a 'Gentleman of the Iſland of *Guernſey*, for correcting the *Log*, &c. 'That after they had left the *Needles*, they had a ſtiff Gale of Wind, 'attended with a rolling Sea, notwithſtanding which, the Machine 'worked as regularly as if it had been ſmooth Water, the little Bell 'of it ſtriking to every Mile the Ship run with great Exactneſs. 'And this Deponent further declares, that having thoroughly viewed 'and examined the Experiment of this new Invention, he finds it 'to be not only practicable, but preferable to the common Methods 'uſed at Sea for attaining the Ship's Diſtance ſailed; that therefore, 'for the publick Good, he doth atteſt the Truth of the above'mentioned

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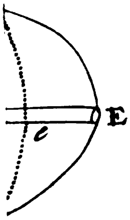


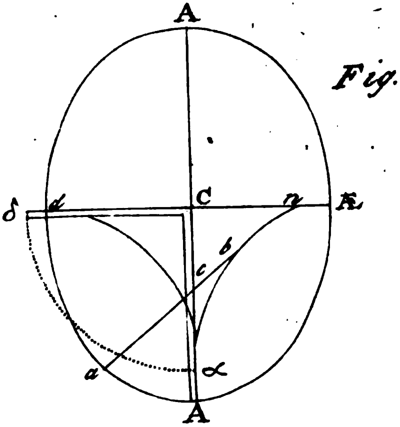
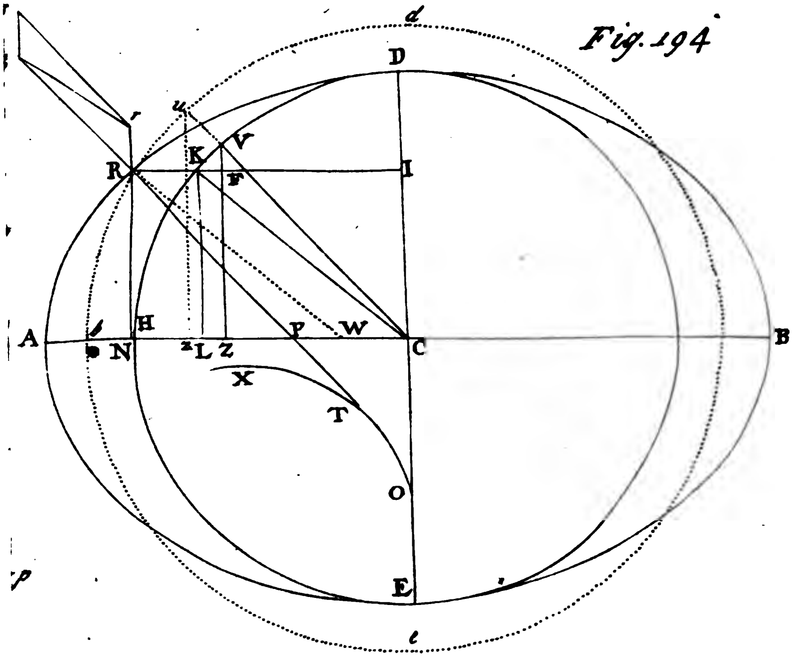


Fig. 192

Fig. 194



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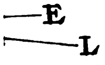
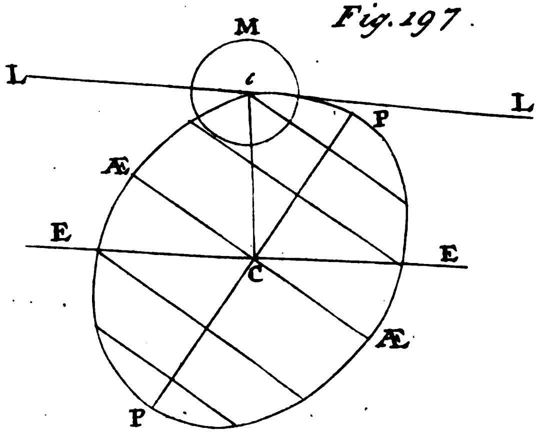


Fig. 197



'the Royal Court of *Guernſey* is hereunto affixed by us the under-'written,

'Signed by the Judge Delegate and Jurats, as above-'mentioned.

Here you have ſome Proof of the Uſefulneſs of this new Invention and that from Seamen of long Standing and Practice: But, notwithſtanding theſe Teſtimonials, I was yet determined to have it tried further: Accordingly I made a Preſent of one of my Machines to a Friend of mine, Captain *John Thoumes*, who beſides his Knowledge in the Theory and Practice of Navigation, was the better qualified to make Trial of it, in regard he had ſometimes accompanied me in my Experiment on the Canal in St. *James*'s *Park*, and in the River *Thames*. As he was then going a Voyage, I intreated him to act impartially with me, and to loſe no opportunity in letting me know how far, and with what Certainty, my Invention might be depended on. Agreeable to my Requeſt, he wrote twice to me on this Occaſion: His firſt Letter was dated at *Nantes* the 20th of *October* 1724, and the following is an Extract of it, *viz.*

'According to my Promiſe, I am to acquaint you, that I have 'had as favourable an Opportunity as I could have wiſhed for, to 'try your *Marine-Surveyor*; for ſome Part of my Voyage being 'from St. *George*'s Channel to the Bay of *Biſcay*, I paſſed cloſe to 'the Land's End of *England*, with a moderate Gale of Wind at 'North, our Courſe S. by E. When I had the Land's End Eaſt of 'me about 3 Miles, I began to reckon, and the next Morning, 'when *Uſhant* bore Weſt, about 5 Miles Diſtance, the *Surveyor* had 'made juſt 37 Leagues. Theſe two noted Headlands, which are 'very near under the ſame Meridian, differ in Latitude about 33 or '34 Leagues. As for the Tides, we croſs'd them, having in this Run 'two Floods and two Ebbs; and as the Wind blew croſs the Chan'nel, one Tide was no more influenced by it than the other, nor 'could the Current be any Impediment to the Trial. Now as to 'our having 3 or 4 Leagues more than the true Diſtance, the Rea'ſon is very plain, ſince it cannot be expected but that a Ship before 'the Wind will deviate from her true Courſe, ſometimes one Way, 'ſometimes another, in her *Yaws* and *Sheers*. Of this all Seamen 'are ſenſible. What I would remark from hence is, that the *Sur'veyor* meaſures all the little Traverſes exactly; 'tis therefore the Bu'ſineſs of the Navigator to allow for this, when he works the Ship's 'Run. But I cannot help obſerving here, that a good Effect is 'produced from theſe little Traverſes being ſo meaſured; for ſhould 'we be running boldly on the Land in a dark Night, it forewarns 'us to look out in time, by marking ſomewhat more than the true 'Diſtance ſailed upon a ſtreight Line.

'Many are the Advantages which accrue to Navigation by this 'Invention, which I ſhall not take upon me to enumerate: In ſhort,

‘ the Sailors are in love with it, and when at the Helm, they value ‘ themſelves on chalking more Miles than thoſe who went before ‘ them. For my own Part, I am ſo pleaſed with it, that I have ‘ done with the *Log*. One excellent Quality I obſerve in it, which ‘ I cannot omit mentioning, *viz.* That in plying to Windward ‘ along Shore in a dark Night, our uſual Way, by the *Log*, is to ‘ ſtand two or three Hours out, and ſo many in; and here we may ‘ be a-ſhore before we are aware, becauſe in running out we may ‘ not have had ſo much Wind as in running in; or we may have ‘ reef'd Topſails, ſhortened Sail, hankered in the Wind, or have ‘ met with many other Impediments, which, by being drouſy in the ‘ Night, a Man may ſometimes not take Notice of; but it is other‘ wiſe with the *Surveyor*; for if the Ship is hindered in her Way, it ‘ will not mark more Miles than ſhe has run.

‘ I have ſhewed it to ſome curious Perſons at *Nantes*, who are ‘ greatly delighted with it. They wanted to ſee the Movement ‘ within, but I ſhall never grant that to a Stranger. I have been ‘ offered fifty Piſtoles for it, and might have had more, would I ‘ have parted with it; but I value the worthy Donor of it too much, ‘ to do any ſuch Thing.

P. S. ‘ When I ſaid my Courſe from the *Lands-End* to *Uſhant* ‘ was S. *b.* E. it muſt be underſtood that I did not go on the Outſide, ‘ but paſſed within, between *Uſhant* and the Main: For in the other ‘ Caſe, to paſs to the Weſtward, the Courſe had been about S. *b.* W. ‘ to go clear of all.

The ſecond Letter, which I received from Captain *Thoumes*, in relation to my Inſtrument, was dated at *Guernſey* the ſecond of *Sept.* 1725; and what follows is the Subſtance of it, ſo far as it relates to the *Marine-Surveyor*, viz.

‘ I am now fully confirmed of the Uſefulneſs of your *Marine*‘ *Surveyor*, having tried it, this laſt Voyage to *Marſeilles* and *Toulon*, ‘ ſufficiently to perſuade me, that it is greatly preferable to the *Log*.

‘ Having in two former Voyages, in the Bay of *Biſcay* been ‘ apprized, that the Ship's Diſtance ſailed, as obtained by the *Ma*‘ *rine-Surveyor*, was really true, yet I was obliged every 24 ‘ Hours to ſhorten the Diſtance by a certain Proportion, that I gueſt ‘ to be near one ſeventh Part of the Whole; which, from the Bear‘ ings of Headlands, &c. I found conſtantly ſo. However, to be ‘ better ſatisfied of this Allowance, I wanted a long Run, near, or ‘ upon a Meridian, with good Obſervations, which could not be ‘ had in the Bay or our Channels; therefore, when I ſailed for the ‘ *Mediterranean*, which was in *January* laſt, I continued to make ‘ the ſame Allowance, and cautioned my Mate to make it alſo. It ‘ happened, that for the firſt eight Days, we had hard Gales of ‘ ſoutherly Winds, attended with violent Squalls of Rain, and a ‘ diſtracted Sea, inſomuch that we tryed under a double reefed Main

‘ ſail, great Part of the Time, and drove to the Weſtward, with‘ out the Benefit of celeſtial Obſervations; yet all the While the ‘ *Marine-Surveyor* ſtruck the Miles of our Drift, which are to be ‘ ſeen upon our Journals for every Hour; and ſo far did I depend ‘ on it, that I did not order the *Log* to be once hove.

‘ After the bad Weather, the Wind changed with the New Moon, ‘ to N. N. E. and N. E. with a briſk Gale, which gave us a fair Run ‘ for five Days, near 50 Leagues every 24 Hours. We had daily ‘ Obſervations, and our Courſe was near South. Here it was, that ‘ I found the one ſeventh of the Ship's Diſtance was to be deducted ‘ from the whole, and that it was for *Yaws* and *Sheers*, which the ‘ *Marine-Surveyor* marks exactly. After this Allowance was made, ‘ ſo well did my Reckoning agree with my Obſervation, that when ‘ there was 2 or 3 Miles difference, I rather imputed it to the Want ‘ of Exactneſs in my obſerving, or a Fault in the Quadrant, than to ‘ the *Marine-Surveyor*, in regard my Mate alſo found it to agree to ‘ a ſurprizing Exactneſs.

‘ Three Weeks after our Departure, I had the Misfortune to loſe ‘ the Fork of the Machine, and therefore was afterwards without the ‘ Help of the *Surveyor*, till our Arrival at *Toulon*; which Place being one of the chief Nurſeries for Navigators that ſerve the *French* ‘ King, I was the more concerned for my Loſs; but I in ſome meaſure repaired it, by ordering a Smith to make two ſuch Forks, of ‘ nearly the ſame Dimenſions and Turns in the Fins, as I could remember the other had, which ſerved there ſo well, as to gain the ‘ Admiration of all who ſaw me try it. My Merchant was ſo taken ‘ with it, that he deſired me to ſhew it to a Friend of his, a noted ‘ Profeſſor of the Mathematicks in the College of Jeſuits there. He ‘ was all Surprize at the regular Motion of the Machine under Water, and more that it ſhould ſo nicely determine the Diſtance ſailed ‘ of any Ship or Boat. I ſhould ſwell my Letter to too great a Bulk, ‘ ſhould I repeat the Converſation I had with this Jeſuit, who importuned me much to ſee the Inſide of the Clock-work, offering ‘ me what I pleaſed for a Sight of it. In a Word, I was deaf to ‘ him, and many other Gentlemen of the Town, who crouded to ‘ me every Day on the ſame Account, and who were all greatly ‘ pleaſed with the Invention.

‘ The Machine made by my Directions at *Toulon*, I uſed in my ‘ Way home, and found it to anſwer very well in the Ocean; from ‘ whence ariſes this Remark, which ſufficiently ſhews the Uſefulneſs ‘ of your Invention, *viz.* That even rough ones, made by a meer ‘ Cobler of a Smith, and turned by the Directions of a ſhort Memory, which I dare not truſt in many Things, are capable of anſwering the End for which you invented them.

‘ It muſt be noted, that though I allow one ſeventh of the Ship's ‘ Diſtance for her Deviation from her Courſe, yet ſome Ships are ſo

‘ built, that they will ſteer much truer, and others worſe than ours
‘ did; and in this Caſe the *Marine-Surveyor* ſhews its Worth; for if
‘ two Ships are in Company, the one ſteering well, the other ill, the
‘ Latter ſhall have more Miles than the Former on comparing their
‘ Run, although they ſet out from the ſame Port, and never part
‘ Company.

I ſhould be wanting to my ſelf, if I did not produce the moſt convincing Proof of the Uſefulneſs of this new Invention; I ſhall therefore add to the foregoing Teſtimonials, ſome other Certificates, whereby it will further appear, that the *Marine-Surveyor* has the Preference of the *Log*, *viz.*

‘ We the underwritten Maſters of Ships, *&c.* do certify all whom
‘ it may concern, That this 21ſt of *October* 1725, we accompanied
‘ Mr. *Henry de Saumarez* on board the *Richard* Yatch, in order to
‘ make an Experiment of an Inſtrument invented by him, called
‘ the *Marine-Surveyor*; and as by it he propoſed to aſcertain the
‘ Way of a Ship in the Sea, much more correctly than by any
‘ Thing hitherto invented for that Purpoſe, we tried it between *London*
‘ and *Graveſend* with the *Log* (which we hove ſeveral Times)
‘ to which it appears to us to have the Preference, for by its conſtant
‘ and regular Motion, the Ship's Diſtance ſailed muſt be more exactly
‘ attained than by the *Log*; which being hove but once in an
‘ Hour or two, cannot be ſo correct, in regard the Wind may increaſe
‘ or leſſen ſoon after the *Log* is hove, in ſuch Manner, that it
‘ entirely depends on him who chalks the *Log-board* to allow for it.
‘ As therefore very conſiderable Errors muſt ariſe from thence, if a
‘ proper Allowance is not made for an Increaſe and Decreaſe of
‘ Wind; and as the *Marine-Surveyor* is not ſubject to this, but
‘ keeps a regular Motion, according as the Wind is more or leſs:
‘ We are therefore of Opinion, that this new Invention is not only
‘ an ingenious Contrivance in its Kind, but is exactly calculated for
‘ the Ends propoſed. As witneſs our Hands this 21ſt of *October*
‘ 1725.

‘ Signed in the Original {
Michael Hales,
Benjamin Hutchinſon,
Joſiah Hales,
Peter Perchard,
Robert Gamble.

‘ Theſe are to certify all whom it may concern, that I *John Harris*,
‘ who have uſed the Sea for thirty Years paſt, and who was lately
‘ Mate of the *William* and *Thomas*, bound from *London* to *Canſo*
‘ in *America*, was preſent, when Mr. *Henry de Saumarez* came on
‘ board our Veſſel and fixed an Inſtrument at the Stern of her, called
‘ the *Marine-Surveyor*, invented by him for aſcertaining the Way
‘ of a Ship in the Sea, much more correctly than by the *Log*, or any
‘ Method

‘ Method hitherto in Uſe for that Purpoſe: And as he deſired us to
‘ try it with the *Log*, and to make an impartial Report whether we
‘ found it preferable to the *Log* or not; I do hereby, in Juſtice to
‘ that Gentleman, certify, That we kept our Reckoning both by
‘ the *Log* and this Inſtrument, and do find it much preferable to the
‘ *Log*, or any Thing that has yet appeared to me for attaining the
‘ Ship's Diſtance ſailed; the Truth of which I am ready to teſtify
‘ on Oath, if called on to do it. In witneſs whereof, I have here-
‘ unto ſet my Hand this 15th of *November*, 1725.

Signed in the Preſence of
Robert Gamble,
Eliſha Dobree,
John Harris.

It may perhaps be aſked, how I came to produce a Certificate from the Mate, and not from the Captain of the *William* aud *Thomas?* To which I anſwer, that the Mate left the Ship at *Plymouth*, and came to Town, ſo that I had an Opportunity of obtaining his Opinion of it, without the Captain's, who ſoon after his Arrival in *England*, made the beſt of his Way to the Iſland of *Guernſey:* However, as I had deſired him to try my Inſtrument with the *Log*, and impartially report to me, whether he found it preferable, or not to that Method of obtaining the Ship's Diſtance ſailed, he favoured me with a Letter from thence: His Name is *Thomas Picot*, and his Letter bears Date the 16th of *November* 1725; it is in *French*, and the Subſtance of it in *Engliſh*, is as follows, *viz.*

‘ That he had made uſe of the *Marine-Surveyor* in his Voyage to
‘ *Canſo* in *America*, and had been more than ordinarily careful therein,
‘ in order to make a juſt Report of it; that he had tried it upon a *Me-*
‘ *ridian* with good Obſervations, and found it to anſwer his Expecta-
‘ tion, and to be preferable to the *Log*, particularly in rough and
‘ ſtormy Weather; that it had been much admired by ſeveral Maſ-
‘ ters of Ships, and particularly by Captain St. *Loe*, of his Majeſty's
‘ Ship the *Ludlow-Caſtle*, who expreſſed a great Liking to it. He
‘ concludes his Letter with wiſhing I had an Opportunity to peruſe
‘ his Journals, whereby it would fully appear how much my Inven-
‘ tion is preferable to the *Log*.

Being informed, that Captain *Henry Daniell* had come over as a Paſſenger from *Canſo* in *America*, to *England*, in the aforeſaid Veſſel *William* and *Thomas*; and being willing to obviate every Objection that might be brought againſt the *Marine-Surveyor*, I applied my ſelf to that Gentleman for his Opinion of it, who was pleaſed to ſend me the following Certificate.

> ‘THESE *are to certify all whom it may concern, that I* Henry Daniell, *who have been at Sea upwards of twelve Years, firſt as a Voluntier, and afterwards as a Midſhipman, did lately come over as a Paſſenger in the* William *and* Thomas, *from* Canſo *to* Plymouth, *in which Veſſel there was an Inſtrument fixed at the Stern of her, called the* Marine-Surveyor, *invented by Mr.* Henry de Saumarez, *for aſcertaining the Way of a Ship in the Sea; and as that Gentleman has applied to me for my Opinion of it, I do hereby certify, that we found it much more correct than the* Log; *and that in a Gale of Wind, our Reckoning by it agreed with our Obſervation, which the Reckoning by the* Log *ſeldom did. And I muſt, in Juſtice to that Gentleman, ſay, that we kept our Reckoning both by his Inſtrument and the* Log, *and found it much preferable thereto, or to any other Method for obtaining the Ship's Diſtance. In witneſs whereof, I have hereunto ſet my Hand this 4th of* December 1725.
>
> Henry Daniell.

Continued by the ſame. N°. 408. p. 47. Fig. 202.

2. F repreſents my Boat on the Canal in St. *James*'s *Park*, through the Rudder of which a ſmall Spindle paſſes (in an Iron Pipe) of which H G is the Length. To the Point G are faſtened the four Iron Fins, or Flyers, A, B, C and D, in the Form of a Square, the Bars D B and A C, to which they are fixed, lying in a horizontal Poſition. Theſe Flyers are ſo contrived as to have full Play in any Motion of the Boat. To the Point H, which is the upper Part of the Pipe and Spindle, is fixed the Dial E: Now the Boat being put in Motion, the Flyers move accordingly, which proportionally affecting the Spindle, the Motion is thereby communicated to the Dial, which may be fitted to ſtrike the Miles or Leagues that the Veſſel runs.

Fig. 203.

But to deſcribe the firſt Movement of this Machine more exactly, *Fig.* 203 repreſents it unfixed. The Croſs, or Bars DB and AC, as I ſaid before, lie flat, or in a horizontal Poſition; the Arbor or Spindle, which is perpendicular thereto, ſcrews into the Point G, and paſſes through an Iron Pipe to the Dial, in manner aforeſaid. The Flyers A, B, C and D being fitted to move in any Motion of the Boat, the Bars are accordingly affected. This Inſtrument is ſo contrived, that two of the Flyers on one Side ſhall always reſiſt the Water in the Motion of the Veſſel, whilſt the other two give way in their Turning. The reſiſting Flyers in this Figure are A and B, and D and C will be the ſame when they come into their Poſition; for they reſiſt and give Way alternately ſo long as the Motion continues, which is always circular; and ſo truly does it revolve, that be the Motion ſwift or ſlow, in any meaſured Diſtance, the Number of Revolutions will be equal.

This is the Machine which I firſt tried on the Canal in S. *James*'s *Park*, which I choſe the rather to do, in regard I found it to anſwer very well in all my Experiments. And I am yet of Opinion, that it would be an uſeful Inſtrument to determine the Strength of the Tides on our Sea Coaſt, which if marked in our Charts, might prove advantageous to our Commerce. But conſidering, that though this Projection might be ſerviceable in Barges, Pleaſure-Boats, or other Veſſels, in fair and moderate Gales of Wind, yet it might prove uſeleſs in boiſterous and ſtormy Weather, and in long Voyages, when it might be choaked with Weeds; I therefore fixed to my other Invention the Fork, which is contrived in ſuch a Manner, that I will even yet be ſo bold as to affirm, it ſhall determine the Ship's Way in a Storm, or when ſhe is ſcudding before the Wind, when the *Log* is incapable of it. As the Canal would not allow me to try, with any Certainty, my Iron Forks there, I was obliged to have ſome made of lighter Materials, which ſeemed to anſwer ſomewhat near the Truth, and made me ſo ſanguine as to believe, that they would have an equal Number of Revolutions in the ſame Diſtance, even though the Motion of the Boat was ſwift or ſlow between Mark and Mark. I muſt here do my worthy Friend Dr. *Deſaguliers* (who frequently honoured me with his Company in the Experiments of the aforeſaid Invention) the Juſtice to own, that he diſſented from me in this Particular, in regard he ſaid the Forks muſt have different Poſitions, according to the Velocity of the Veſſel to which they were fixed, and conſequently could not have an equal Number of Revolutions in ſwift and ſlow Motion.

Whilſt I was conſidering where to carry on my Experiments to prove the Verity of my Inſtrument, and to anſwer this Objection, I had the Honour to be introduced to the late ingenious *Samuel Molyneux*, Eſq;. As he was ever ready to encourage all laudable Deſigns, and particularly ſuch as were calculated for Publick Good, he ſoon became my Patron: And as he was then one of the Lords Commiſſioners of the Admiralty, and my Machine fell within his Province, he expreſſed a Deſire to ſee an Experiment of it on the River *Thames*: Accordingly I ſhewed to him, and ſeveral of the principal Officers and Commiſſioners of his Majeſty's *Navy*, the Nature and Uſe of it between *London* and *Woolwich*, when he ſeemed to be of the ſame Opinion with Dr. *Deſaguliers*, *viz.* Whether in a certain Diſtance, and in different Motions of the Veſſel, the Inſtrument could revolve equally. Hereupon he adviſed me to take a Trip over to *Holland*, and to try my *Machine* with the *Log*, in my Paſſage; as alſo throughly to examine the Truth of his Objection on the long Canals in that Country, where there was little or no Tide or Current.

Accordingly I had Orders to embark on board the *William* and *Mary* Yatch. My Machine being fixed at the Stern of this Veſſel, we kept her Run both by it and the *Log*. On the niceſt Calculation, in our Paſſage over, the Difference between us was 2 Miles and 2640 Feet: At this I was in no wiſe ſurpriſed; for as I knew the *Log* to be very erroneous, and I undertook to correct the Errors of it by my Inſtrument (in the Truth of which I might then be too forward, as too many are on ſuch Occaſions) I was aſſured we could not agree; and therefore I charged the Difference accordingly.

Among the conſiderable Company on board the Yatch, we had a curious Gentleman, Captain *Lynſlager*, Commander of one of the *Dutch* Men of War, who ſeemed not a little pleaſed with my Contrivance; and no ſooner did he land in *Holland*, but he ſpoke of it to ſome Gentlemen of the higheſt Rank there, whoſe Curioſity induced them to deſire to ſee an Experiment of this Invention: Accordingly I was ſent for to the *Hague*, and on the Canal there, before Baron *Hop*, Baron *Waſſenaar*, Admiral *Somelſdyk*, Mr. *s'Graveſand* (Profeſſor of the Mathematicks in the Univerſity of *Leyden*) Captain *Lynſlager*, *&c.* we run a certain Diſtance in ſwift and ſlow Motion, in order to ſee if the Inſtrument would have an equal Number of Revolutions therein. In running up, it revolved 2300 Times, and in coming down 2060. Here then was enough to convince me, that Dr. *Deſaguliers*, and Mr. *Molyneux*, had judged truly of the Fork, and more eſpecially ſince the learned Mr. *s'Graveſand* joined in Opinion with them; who notwithſtanding encouraged me, by telling me my Labour was not in vain, for that the Inſtrument might ſtill be of good Service, by making Tables to rectify the different Revolutions.

An Opinion ſtrongly indulged is rarely parted with; the Truth of which I find in my ſelf; for although Dr. *Deſaguliers*, Mr. *Molyneux*, and Mr. *s'Graveſand* did jointly agree as to this Invention, yet ſtill did I entertain ſome ſlender Thoughts, that it muſt anſwer the Purpoſe, in the Manner I had propoſed. For when I conſidered, that I had two Fathom of Rope out on the *Dutch* Canal, which was but 5 or 6 Feet Deep, and that the Fork of my Machine weighed about three Pounds, or three and a half, and was two Feet and a half in Length, I thought it not unreaſonable to ſuppoſe, that its Weight, in the ſloweſt Motion of the Veſſel, might occaſion it to ſtrike Ground, and conſequently impede its Motion, and leſſen the Motion, and leſſen the Number of Revolutions as above. Of this I had been fully ſatisfied whilſt in *Holland*; but fearing to loſe my Paſſage in the Yatch, on Board of which I had embarked by Order of the Lords Commiſſioners of the Admiralty, I was obliged to haſten over.

Not long after I came to *England* died my worthy Patron Mr. *Molyneux*, in whom all Men of Learning and Ingenuity loſt a Friend; and as there was now but little Hope of my going over to *Holland* in the Manner I had done before, I was notwithſtanding reſolved to take that Journey at my own Expence; and accordingly did ſo, where I no ſooner began my Experiments, but I was convinced of the Truth of the Objections of the three learned Gentlemen aforementioned, which is plainly made appear from the following Figure, wherein the Poſition of the Fork, in five different Motions of the Veſſel, is repreſented. *See* Fig. 204. Fig. 204.

This needs no Explanation, for it plainly appears, that the Pallets will be more or leſs affected by the Reſiſtance of the Water, according to the Poſition they are in; and therefore the Revolutions in a ſwift or ſlow Motion, in the ſame Diſtance, cannot be equal.

Being now fully perſuaded, that the Fork would not revolve equally in the ſame Diſtance, and in different Motions of the Veſſel, I now began to repair this Defect by calculating ſome Tables, which render it ſtill a very uſeful Inſtrument. On what Foundation I formed theſe Tables, there will be no need for me to mention, ſince I ſhall go on to ſhew what further Improvements I have made of this Inſtrument, and that it is now every Way uſeful without them. And this, I think, I cannot better do, than by entering here the Extract of a Letter to Dr. *Deſaguliers* from a learned Mathematician of *Holland*, whoſe Company I was honoured with ſeveral Times, whilſt I was making my Experiments on that Side, *viz.*

'Mr. *De Saumarez* having deſired me to acquaint you of the Succeſs of the Experiments, which I have ſeen him make of this 'Machine, for the meaſuring the Way of a Ship in the Sea, it is 'with Pleaſure I undertake it, ſince I am fully perſuaded you will 'not be wanting to contribute all in your Power to promote an invention ſo uſeful and advantageous as this is.

'The firſt Experiment that I attended was with an iron Fork, ſuch 'as the Gentleman himſelf hath deſcribed in the *Philoſophical Tranſactions*, when the Number of Revolutions were more in the ſwift 'than in the ſlow Motion of the Boat, whereon we tried this inſtrument. This I take to be owing to the different inclinations of the 'Machine; which were more Horizontal, according as the Motion 'of the Boat was more ſwift; from whence we concluded, that it 'would be neceſſary to help this by ſome Tables calculated for the 'Purpoſe: Since which, Mr. *De Saumarez* hath accordingly formed ſuch Tables; but as I was not preſent at the Experiments whereon they are founded, I leave you to the Gentleman himſelf to give 'you an Account thereof.

'I have alſo made another Experiment with Mr. *De Saumarez*, 'upon a new Correction of his Machine, which he will better explain 'to you, when you ſee him, than I can deſcribe. Here he has contrived the firſt Movement of his Machine to lie Horizontal under 'the Water; and ſuch was our Succeſs in this Experiment, that I 'make no more doubt of the Uſefulneſs of this invention, which I

'look

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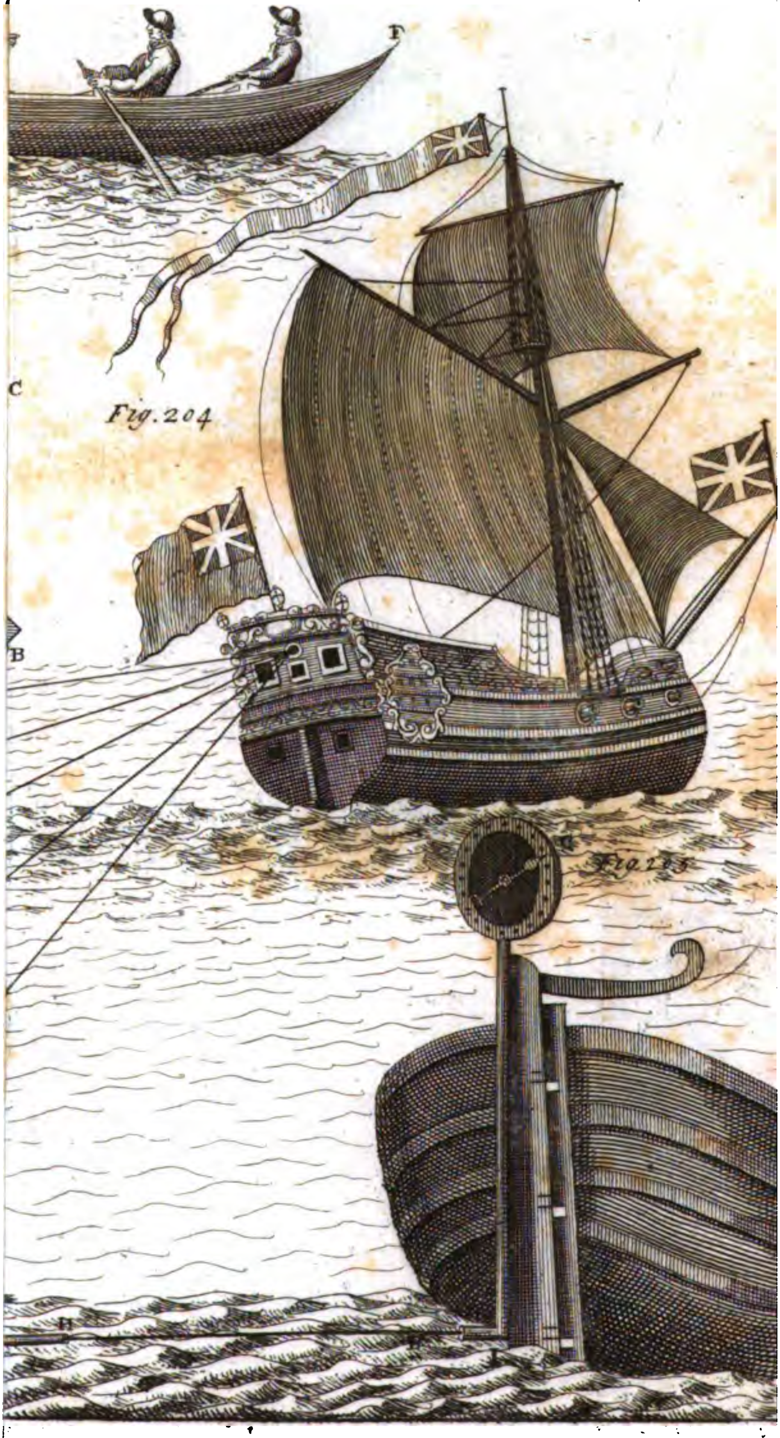
Fig. 202



cold Weather was ſo far ſet in, that it would not allow me to make more Experiments than I did on that Side, I hope ſoon to make it appear, that the Revolutions are exactly equal in this new Improvement of the Fork.

P. S. I have been lately endeavouring to make a further Improvement in Navigation, whereby I propoſe to make a Ship work far better to Windward, than it is poſſible for the moſt Weatherly one to do at preſent; as alſo to make them tack and ware in much leſs Room than is generally done on ſuch Occaſions. The Advantages ariſing from ſuch a Projection, if it proves practicable, muſt be conſiderable; For 1. The Ship which is in Danger of a Lee-ſhore will hereby be enabled to weather the Point ſhe may want, and not be forced, in ſtormy Weather, to anchor in the very Breach of the Shore, and even in the Jaws of Deſtruction. Of this we have had too many melancholy Inſtances, where ſeveral Lives and Fortunes have been loſt; Diſaſters of which kind, it is humbly conceived, may, in a great Meaſure, be prevented by this Invention. 2. Hence we need not fear to get the Weathergage of an Enemy; for by plying to Windward much faſter than he can, and by tacking and waring in much leſs Compaſs, I can either leave him, or continue to engage him, as ſhall appear moſt convenient: At leaſt I can ſo ſpend the Day, as to be able to ſecure my ſelf under the Covert of the Night; or if I chance to be near the Land, I may hereby be enabled to gain a ſafe Harbour. 3. By this Invention the wild Steerage which is too frequently made in ſome Ships, will be prevented, which all Mariners muſt allow to be of Service, eſpecially in chaſing, or being chaſed by an Enemy; as well as in their keeping the Reckoning of the Ship's Way, *&c.*

The Deſcription of a new Quadrant *for taking* Altitudes *without a Horizon, either at Sea or Land. Invented by Mr.* John Elton. N°. 423. p. 273. Fig. 206.

XI. This Inſtrument contains four principal Parts, *viz.* a Frame, an Index, a Label, and a Shield; and theſe conſiſt of ſeveral Parts. The Frame has two Parts, one a graduated Arch of 30 Degrees, each Degree being ſubdivided into ſix equal Parts; the other a Chord of an Arch of 60°, divided into two equal Parts (at the Extremities and in the Middle of which are Holes or Stops for the Label) together making 90° or a Quadrant. The Index turns upon the Center of the Frame the whole Compaſs of the Arch, and has three Parts; *viz.* a *Nonius* Plate, an Eye-Vane, and a Tube. The *Nonius* Plate moves with the Index, and ſubdivides each of the ſmall Diviſions of the Arch into ten equal Parts or Minutes. The Eye-Vane is to look through in forward Obſervations. The Tube is to ſhew, when the Index is horizontal. The Label moves upon the Center of the Frame the whole Compaſs of the Chord of the Arch of 60°, having three fixed Stations thereon, at 30°, 60°, and 90°, and contains two principal Parts; *viz.* a Lens, and a Lanthorn. The Lens is to form the Sun's Image upon the Shield. The Lanthorn is neceſſary in Nocturnal Obſervations. The Shield is fixed in the Center of the Frame, and has three Parts; *viz.* an Azimuth Tube, a Horizontal Tube, and an Axis, or in backward Obſervations a Ray-Plate. The Hole in the Shield is to receive the Sun's Image. The Azimuth Tube is to direct the Plane of the Inſtrument perpendicular. The Horizontal Tube is to ſhew when the Label is level. The Axis is to cut the Object in forward Obſervations.

Rule for either backward or forward Obſervations.

If the Altitude does not exceed 30°, the Label muſt be placed at the Station on the Radius or longeſt Limb of the Quadrant; if the Altitude is between 30° and 60°, at the middle Station; and if the Altitude exceed 60° at the uppermoſt Station.

To take the Sun's Altitude by a backward Obſervation.

This is done without uſing the Sight-Vane or Horizontal Tube on the Shield. Hold the Quadrant with both Hands in ſuch a manner as is apteſt for keeping it ſteady, the Back of the Arch being turned toward the Sun. When the Bubble of the Azimuth Tube is brought under the Hole in the Shield, cauſe the Sun's Image to fall on the Hole in the Shield, ſo that it may reſt in the Center of the Sun's Image; the Inſtant the Azimuth Tube and Sun's Image are thus regulated, ſee if the Bubble in the Horizontal Tube on the Index (which 'till then is diſregarded) leaves the open End of the Tube, or ſtops any where clear of the Ends of the Tube: If theſe happen at the ſame Juncture, the Altitude is then truly taken; but if the Bubble had remained in the encloſed End of the Tube, when the Azimuth Bubble and Sun's Image were regulated, the Index muſt have been ſlid up; and if tarried in the open End, moved down, until the Horizontal Bubble on the Index quit the open End of the Tube, or ſtop between the Ends, as was before obſerved; and then is the Quadrant ſet. In continuing the Obſervation for a Meridian Altitude, the Quadrant being ſet, as the Sun riſes, the Horizontal Bubble on the Index will not quit the open End of the Tube, or ſtop between the Ends, but hang there, or leave it after the Azimuth Bubble and Sun's Image have been regulated, which will require the Index to be continually moved down in order to keep the Quadrant ſet. When the Sun is up, or on the Meridian, the Quadrant will remain ſet for ſome Time; and on the Sun's falling, the Horizontal Bubble will have a reverſe Tendency inclining or running wholly to the encloſed End of the Tube.

To take the Altitude of the Sun or Stars by a forward Obſervation.

In this Method, the Lens and Tube on the Index are diſregarded. Hold the Quadrant vertical, and looking through the Eye-Vane, direct the Axis or upper Edge of the Shield to the Sun or Star; if the Axis cut the Sun or Star at the ſame Inſtant that the Bubble in the Horizontal Tube on the Shield quit the open End, the Altitude is then truly taken, and the Quadrant ſet. But if it ſhould leave the open End of the Tube before the Axis or upper Edge of the Shield cut the Sun or Star, then the Eye-Vane (or which is the ſame, the Index) muſt be ſlid down; and if it remain at the open

End, or quit it when the Axis is above the Sun or Star, moved up until the Quadrant is ſet. In continuing the Obſervation for a Meridian Altitude, as the Sun or Star riſes, the Bubble in the Horizontal Tube will always quit the open End of the Tube before the Axis cut the Object; ſo that to keep the Quadrant ſet, the Eye-Vane muſt on every ſuch Alteration be conſtantly moved down; while the Sun or Star is on the Meridian, the Quadrant will remain ſet; and when the Sun or Star falls, the Bubble will act contrary to what it did in the riſing, reſting wholly in the open End of the Tube.

To take the Sun's Altitude with the Horizon.

Turn the Back of the Arch towards the Sun, and cauſe the Sun's Image to fall on the Hole in the Shield, at the ſame time looking through the Eye-Vane, cut the Horizon with the Axis.

N. B. In taking the Altitude of the Stars, a ſmall Light muſt be fixed in the Lanthorn; the leſs the better. It will be beſt in forward Obſervations of the Sun, to take the Altitude of the upper Limb, allowing for the Semidiameter; and when the Sun is very clear, take his Altitude by a backward Obſervation, the forward Method being chiefly intended for Nocturnal Obſervations, and when the Sun is too much obſcured to give any Shade or Image.

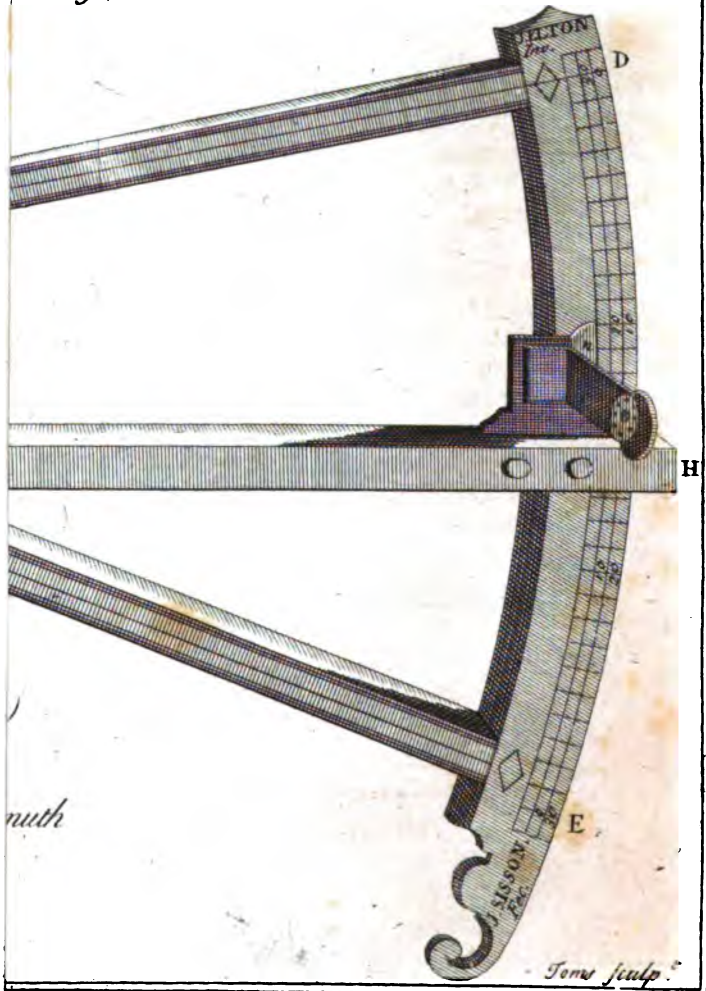
There was at the ſame Time laid before the *Society*, ‘ An Extract made by Mr. *Elton* of Obſervations of the Latitude from the Journal of Captain *Walter Hoxton*, Commander of the Ship *Baltimore* from the River of *Thames* to *Maryland* on the Continent of *America*, by *Davis*'s (or the common) Quadrant with the Horizon, and by Mr. *Elton*'s (a new invented Quadrant) without the Horizon, *A. D.* 1730.”

From this Extract it is obſervable, that in moderate Weather the Difference of the Obſervations, made by the two Sorts of Quadrants, was commonly no more than 1'; with ſtrong Gales and a large Sea 5'; in fair Weather; in hard Squalls; the Sea running high, 6'; in eaſy Gales 9'; in fair Weather and a large Swell 16'; once in ſmooth Water 16'; and the greateſt Difference of all was, with freſh Gales, 21': And this Difference was conſtantly found to give the Latitude more Northerly by Mr. *Elton*'s Quadrant than by *Davis*'s; as in this laſt mentioned Inſtance the Latitude appears to be 35° 39' N. by *Davis*'s, when Mr. *Elton*'s makes it 36° N. There is a Note added by Captain *Hoxton* at the End of this Journal; *viz. That the Difference at different Times between* Davis'*s and* Elton'*s Quadrants is occaſioned by ſhifting the Shade-Vane of* Davis'*s.*

To this Journal were annexed ſome ‘ Obſervations of the Latitude by the fixed Stars in the foreſaid Voyage by Mr. *Elton*'s Quadrant, without uſing the Horizon.”

Theſe Obſervations are generally taken from two Stars, and the Latitude calculated from each Obſervation; and ſo they are found to

Fig. 206



Toms sculp.

CHAP. VIII.

ARCHITECTURE, SHIP-BUILDING.

The Method of making the beſt Mortar at Madraſs, *by the Hon.* Iſaac Pyke, *Eſq; Governour of St.* Helena. N°. 422. p. 231.

I. TAKE 15 Buſhels of freſh Pit-ſand, well ſifted; add thereto 15 Buſhels of Stone Lime: Let it be moiſtened or ſlacked with Water in the common manner, and ſo laid 2 or 3 days together. Then diſſolve 20 Pound of *Jaggery*, which is coarſe Sugar (or thick Molaſſes) in Water, and ſprinkling this Liquor over the Mortar, beat it up together till all be well mixed and incorporated, and then let it lie by in a Heap. Then boil a peck of *Gramm* (which is a ſort of Grain like a *Tare*, or between that and a Pea) to a Jelly, and ſtrain it off through a coarſe Canvaſs, and preſerve the Liquor that comes from it. Take alſo a Peck of *Myrobalans*, and boil them likewiſe to a Jelly, preſerving that Water alſo as the other; and if you have a Veſſel large enough, you may put theſe 3 Waters together; that is, the *Jaggery-Water*, the *Gram-Water* and the *Myrobalans*. The *Indians* uſually put a ſmall quantity of fine Lime therein, to keep their Labourers from drinking of it. The Mortar beat up, and when too dry, ſprinkled with this Liquor, proves extraordinary good for laying Brick or Stone therewith, keeping ſome of the Liquor always at Hand for the Workman to wet his Bricks therewith; and if this Liquor proves too thick, dilute it with freſh Water. Obſerve alſo, that the Mortar here is not only to be well beaten and mixed together, but alſo laid very well, and every Brick, or Piece of Brick, fluſhed in with the Mortar, and every Cranny filled up, yet not in thick Joints, like the common *Engliſh* Mortar; and alſo over every Courſe of Bricks, ſome to be throwed on very thin: And where the Work hath ſtood, though but for a Breakfaſt or Dining-time, before you begin again wet it well with this Liquor, with a Ladle, and then lay on your freſh Mortar; for this Mortar, notwithſtanding its being thus wetted, dries much ſooner than one not uſed to it would conceive, but eſpecially in hot Weather. For ſome very ſtrong Work, the ſame Mortar above is improved as follows. Take coarſe Tow and twiſt it looſely into Bands as thick as a Mans Finger (in *England* Ox-hair is uſed in ſtead of this Tow) then cut it into Pieces of about an Inch long, and untwiſt it ſo as to lie looſe; then ſtrew it lightly over the other Mortar, which is at the ſame time to be kept turning over, and ſo this Stuff to be beat into it, keeping Labourers continually beating it in a Trough, and mixing it till it be well incorporated with all the Parts of the Mortar.

And whereas it will be ſubject to dry very faſt, it muſt be frequently ſoftned with ſome of the aforeſaid Liquor of *Jaggery*, *Gram*, and *Myrobalans*, and ſome freſh Water; and when it is ſo moiſtened and beat, it will mix well, and with this they build (though it be not uſual to build common Houſe-Walls thus) when the Work is intended to be very ſtrong; as for inſtance, *Madraſs* Church Steeple, that was building when I was laſt there, and alſo for ſome Ornaments, as Columns, good arched Work, or Imagery ſet up in Gardens, it is thus made. Though for common Buildings about *Madraſs*, where the rainy Seaſon holds not above 3 Months in the Year, and ſometimes leſs, they uſually lay all the common Brick-work in a loamy Clay, and plaiſter it over on both ſides with this Mortar, which is yet farther to be improved. Thus far for Building Mortar. Having your Mortar thus prepared, as is before deſcribed, you muſt ſeparate ſome of it, and to every ½ Buſhel, you are to take the white of 5 or 6 Eggs, and 4 Ounces of *Ghee* (or ordinary unſalted Butter) and a Pint of Butter-milk, beaten all well together: Mix a little of your Mortar with this, until all your *Ghee*, Whites of Eggs, and Butter-milk be ſoaked up; then ſoften the reſt well with plain freſh Water and ſo mix all together, and let it be ground, a Trowel full at a Time, on a Stone, with a Stone Roller, in the ſame manner that Chocolate is uſually made, or ground in *England*; and let it ſtand by in a Trough for uſe. And when you uſe it, in caſe it be too dry, moiſten it with ſome Water, or the before-mentioned Liquor. This is the ſecond Coat of Plaiſtering.

Note, When your firſt Coat of Plaiſtering is laid on, let it be well rubbed on with a hardening Trowel, or with a ſmooth Brick, and ſtrewed with a gritty Sand, moiſtened, as occaſion requires, with Water, or the before-mentioned Liquor, and then well hardened on again, which, when half dry, take the laſt mentioned Compoſition for your fine Plaiſtering; and when it is almoſt dry, lay on your whitening Varniſh; but if your Work ſhould be quite dry, then your *Chinam* Liquor muſt be waſhed over the Work with a Bruſh. The beſt ſort of whitening Varniſh is thus made. Take one Gallon of *Toddy*, a Pint of Butter-milk, and ſo much fine *Chinam*, or Lime, as ſhall be proper to colour it; add thereunto ſome of the *Chinam* Liquor before-mentioned, waſh it gently over therewith; and when it is quite dried in, do the ſame again. And a Plaiſter thus made is more durable than ſome ſoft Stone, and holds the Weather better in *India*, than any of the Bricks they make there. In ſome of the fine *Chinam* that is to endure the Weather, and where it is likely to be ſubject to much Rain, they put [*] *Gingerly Oil* inſtead of *Ghee*; and alſo in ſome they boil the Bark of the *Mango-Tree*, and other Barks of aſtringent Natures, and *Aloes*, which grow here in great Plenty by the Sea-ſhore; but to all of the fine *Chinam*, that is for outſide Plaiſtering,

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[*] Oleum Seſami.

tering, they put Butter-milk, which is here called *Toyre.* And for inſide Work they uſe Glue made very thin and weak, in ſtead of Size, for White-waſhing, and ſometimes they add a little Gum to it. *N. B.* Whereas ſundry Ingredients here mentioned are not to be had in *England*, it may not be amiſs to ſubſtitute ſomething more plentiful here, which I imagine to be of the ſame Nature. As to all the aſtringent Barks, I take Oaken Bark to be as good as any. Inſtead of *Aloes*, either Turpentine, or the Bark and Branches of the *Sloe-tree.* Though *Turpentine* be not ſo ſtrong, yet, if uſed in greater Quantity may ſerve to the ſame purpoſe. But there is a ſort of *Aloes Hepatica*, often very cheap. Inſtead of *Myrobalans*, ſome Juice of * *Aloes*; alſo inſtead of *Jaggery*, coarſe Sugar, or *Molaſſes*, will do; inſtead of *Toddy*, which is a ſort of *Palm-wine*, the Liquor from the *Birch-tree* comes near to it.

Note, That in *China*, and ſome other Parts, they temper their Mortar with Blood of any ſort of Cattle, but the Ingredients beforementioned are ſaid to be as binding, and do full as well, and does not make the Mortar of ſo dark a Colour as Blood will do. The Plaiſtering above deſcribed, is thought in *India*, vaſtly to exceed any ſort of *Stucco-work*, or Plaiſter of *Paris*; and I have ſeen a Room done with this ſort of Terras-Mortar that has fully come up to the beſt ſort of Wainſcot Work, in Smoothneſs and in Beauty.

Of the manner of bending Planks in His Majeſty's Yards by a Sand-heat, invented by Captain Cumberland. *By* Robert Cay, *Eſq*; Nº 371. p. 75.

II. The Place where the Planks lie to be ſoftned in the Stove, is between two Brick Walls; of ſuch a length, height and diſtance from each other, as ſuffice to admit the largeſt, or to hold a good number of the ſmaller Sort: The bottom is of thick Iron Plates, ſupported by ſtrong Bars; under the middle of which, are two Fire-places, whoſe Flews carry the Flame towards the Ends.

The Planks are laid in Sand; the loweſt about ſix or eight Inches above the Iron Plates, they are well covered with the Sand, and Boards laid over all, to keep in the Heat. The Sand is moiſtened with warm Water, (for which purpoſe they have a Cauldron adjoining to the Stove) and if the Timber be large, and intended to be very much bent, ſo that it muſt lie long in the Stove, they water the Sand again, once in 8 or 10 Hours. When 'tis judged to be ſoft enough, the Sand is removed; and the Workmen carry away their reſpective Planks, to the ſeveral Places, where they are to be uſed; and having firſt nailed a thin Board upon the out-ſide, to preſerve the Plank from Bruiſes, they fix one part in its proper Place, and bring to the others, by any power they can moſt conveniently apply. This Work ſeems to be performed with wonderful Eaſe; notwithſtanding ſome we ſaw were ſo knotty, that the Builders aſſured us, they could not have brought them to that Curvature by the former Methods. Thoſe we ſaw put in between others, very exactly ly fitted the Spaces they had been cut for; and the Workmen told us, they had made no Allowance either for the ſwelling, or ſhrinking of the Wood.

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* *Sloes.*

This Method excells that of burning the Planks over an open Fire in ſeveral Reſpects: particularly, that no part of the Wood is deſtroyed, but remains of the ſame Dimenſions; at leaſt very nearly; a Plank of the Breadth of 16 Inches being ſaid not to alter above $\frac{1}{20}$ part of an Inch. The Edges of the Plank are preſerved; and conſequently the Work muſt be much firmer, and the Calking laſt longer. The extraordinary ſoftneſs of the Wood, while 'tis warm, makes it eaſily bend to any Figure neceſſary in Ship-building, which it holds very well, if they have occaſion to take it off again after it is cold: Whereas the Plank bent by burning, would ſtart when looſened; and could only be fixed to the Timbers by ſuch a Force, as was able to overcome the Reſiſtance occaſioned by the Spring of the Plank. It likewiſe adapts it ſelf very readily to the Surface of the Timbers, if they happen to be uneven.

They ſhewed us the Gun-Deck-Clamps in a Ship of the Second Rate; which are very large Planks, bent and twiſted in ſo peculiar a Manner, that they never could by any other Method, bend them into that Form, but uſed to cut them into Shape. The whole Operation is performed with much leſs trouble to the Carpenters, as well as at leſs Expence; as 'tis evident, from the deep Tincture the Sand receives, that a conſiderable Quantity of Sap comes out of the Oak, while it's in the Stove: And a large Plank was obſerved by the Workmen, or Officers of the Yard, to weigh ſome Pounds leſs, when it was taken out.

A Plank 5 Inches thick requires 5 or 6 Hours to make it fit for bending; and the Time requiſite for others, ſeems to be in a Duplicate Proportion to their Thickneſs.

Fig. 207. *Fig.* 207. Repreſents a Plank, in the Buttocks of a Second Rate Ship, whoſe length from A to C, is three Feet, and thickneſs (A F) 4$\frac{1}{4}$ Inches, the End C, of this Plank was bent 12 or 13 Inches from the ſtreight Line A B.

Fig. 208, 209. *Fig.* 208, and 209, are Sections of the Stove. A. A. the Fire-places. B. B. the Aſh-holes. C. C. the Flews under the Iron Bottom. D. D. the two Chimneys. E. the place for the Planks and Sand. F. F. the two Brick Walls. G. G. two inclined Planes, for the Men to ſtand on, *&c.* when they put in, or take out Planks, or water the Sand. h. h. The bottom of the Stove, made of Iron. i. i. The Grates to lay the Fewel on.

CHAP.

age 468.

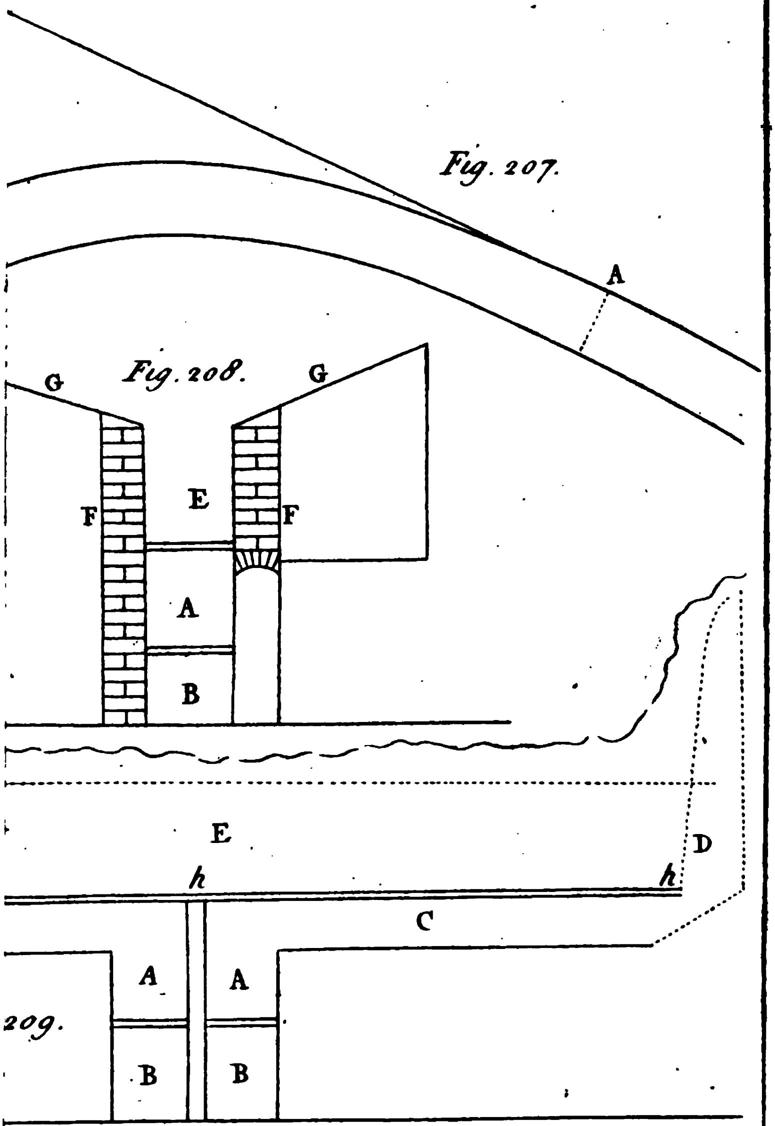
Fig. 207.

Fig. 208.

209.

CHAP. IX.

PAINTING.

MR. *Le Blon* endeavouring to fix the true Harmony of Colouring in Painting, found that all viſible Objects may be repreſented by the three primitive Colours, Red, Yellow, and Blue; for out of them, all others, even Black itſelf, may be compounded. We are beholden to the great Sir *Iſaac Newton* for the Diſcovery of the *Difference of Colours contained in the Rays of the Sun; and that the Union of them all produces a White, which is Light itſelf.*

An Account of Mr. James Chriſtopher LeBlon's *Principles of* Printing, *in Imitation of* Painting, *and of* Weaving, Tapeſtry, *in the ſame manner as* Brocades. *By* C. Mortimer, *M. D. S. R. Secret.* N°. 419. p. 101.

For Diſtinction ſake Mr. *Le Blon* calls thoſe Colours which are comprehended in the Rays of the Sun, Impalpable Colours, and thoſe uſed in Painting, Material Colours. In the Material Colours, a Mixture of all three produces a Black or Darkneſs, contrary to what is obſerved in the Impalpable, which I ſaid juſt now produce White. Mr. *Le Blon* takes this Phænomenon to be owing to the Body or Subſtance of which theſe three material Colours conſiſt, and to the Particles of them being Opake, and not Tranſparent; for they only reflect certain Rays of Light, that ſtrike on their Surfaces; and therefore when ſmall Particles of different Colours are placed cloſe together, if they are ſo ſmall that each of them cannot be ſeen ſeparately by the Eye, we do not diſcern the Colour of each particular Atom, but only the blended reflected Rays, proceeding from the adjoining Particles: Thus Yellow and Red produce an Orange, Yellow and Blue a Green, *&c.* which ſeems to be confirmed by placing two Pieces of Silk near together; *viz.* Yellow and Blue: When by intermixing of their reflected Rays, the Yellow will appear of a light Green, and the Blue of a dark Green; which deſerves the farther Conſideration of the Curious.

He hath reduced the *Harmony of Colouring in Painting to certain infallible Rules, built on this Foundation:* Whereas, according to the common Practice of Painters, their colouring is the Effect of meer Chance or Gueſs-work at firſt, but improved by Experience; all Painters uſually declaring that there can be no certain Rules given for mixing Colours. Mr. *Le Blon* publiſhed, ſome Years ago, an ingenious Book on this Subject, intituled, *Coloritto*; or, *the Harmony of Colouring in Painting.*

By theſe Rules he light on the Manner of Printing any Object in its natural Colours, by the Means of three Plates, and the three primitive Colours; an Art attempted and ſought after ever ſince the Invention of Printing, but in vain, and thought impoſſible, till he put it in practice about 15 Years ago. The Plates are engraved chiefly after the *Mezzo Tinto* Manner; only the darker Shades, and ſometimes the Out-Lines, where they are to appear very ſharp, are done with a common Graver. Each Plate is not compleatly engraved, but only contrived to take ſuch a Portion of the Colour as is neceſſary with the other two Plates, to make the Picture compleat.

This Art of Printing conſiſts in ſix Articles.

1. To produce any Object with three Colours, and three Plates. 2. To make the Drawings on each of the three Plates, ſo that they may exactly tally. 3. To engrave the three Plates, ſo as that they cannot fail to agree. 4. To engrave the three Plates in an uncommon Way, ſo as that they may produce 3000 and more good Prints. 5. To find the three true primitive material Colours, and to prepare them, ſo as that they may be imprimable, durable, and beautiful. 6. To print the three Plates, ſo as that they may agree perfectly in the Impreſſion.

The firſt of which is the moſt conſiderable, comprehending the Theoretical Part of the Invention; and the other five are ſubſervient to bring it into mechanical Practice, and of ſuch Importance, that if any one of them be wanting, nothing can be executed with Succeſs or Exactneſs. Sometimes more than the three Plates may be employed, *viz.* when Beauty, Cheapneſs, and Expedition require it.

Weaving Tapeſtry.

The Obſervation of the compounded Colours reflected from two Pieces of Silk, of different Colours, placed near together, firſt gave him the Thought of what the Effect of weaving Threads of different Colours would be, when all the Threads were ſo fine, as not to be diſtinguiſhed at a ſmall Diſtance one from another.

By the ſame Principles of producing any viſible Object with a ſmall Number of Colours, he arrived at the Skill of producing in the Loom all that the Art of Painting requires. An Art likewiſe often attempted, but as often abandoned, and declared impoſſible till now, as well as the other of Printing in Colours. And 'tis probable, many Improvements may from hence be made in ſeveral Trades, eſpecially in combing of Wool, where the mixing of ſeveral Colours may be of great Uſe; but he hath not yet had Time to apply it to any thing elſe beſides Painting, Printing and Weaving.

The Colours uſed in Weaving being only ſuperficial, and ſo differing from both the impalpable and the material Colours, and not being to be ſo cloſely joined or incorporated together as thoſe, will not of themſelves produce a White or Black, but only a Light *Cinnamon:* Wherefore, in Weaving he hath been obliged to make uſe of white and black Threads, beſides Red, Yellow, and Blue; and though he found he was able to imitate any Picture with theſe five Colours, yet for Cheapneſs and Expedition, and to add a Brightneſs where it was required, he found it more convenient to make uſe of ſeveral intermediate Degrees of Colours.

There are two Ways in Uſe at *Bruſſels*, and at the *Goblins* in *Paris*, for making Tapeſtry after the common Manner: One they call the flat Way, and the other the upright. In the flat Way, they have the Warp ſtretched in a Frame length-wiſe of the Piece: It is made of white Worſted, and the Pattern lies cloſe under it; ſo that the Workman can ſee the Figures through the Warp: He is provided with Bobbins of various Colours of Silk or Worſted, as the Piece requires: Then he takes up with his Fingers one Thread after another, as they anſwer to any Colour, in the Painting beneath; and with the other hand paſſes the Bobbin with the ſame Colour, and ſtrikes the Threads cloſe with an Ivory Comb. Some of theſe Frames are made like a Loom, with a Warp paſſed through the Leiſhes and Tredles for the Feet, with which they open the Threads of the Warp, to paſs a common Shuttle through them, when it is neceſſary to make a long Throw, as is required in Grounds, Pillars, and tall Uprights.

In the upright Way the Warp runs from Top to Bottom of the Piece; the Pattern is placed upright, and cloſe behind it, and the Out-lines are drawn in Charcoal upon the Foreſide of the Warp. The Work-man is placed with his Back to the Light, by which means he can ſee the Pattern better; then he takes up the Threads one by one, and paſſes the Bobbin, as in the other Way, and ſtrikes it cloſe with the Comb: All which is near as tedious as Needle-work itſelf; which is the Reaſon why fine Tapeſtry comes to ſuch high Prices, ſo that none but Princes care to buy it; and what can be had at a moderate Price is always coarſe, and of a low Taſte: For Workmen who have any good Notion of Painting, and are capable of adjuſting the Colours, are not to be had, but for exceſſive Wages; which much enhances the Price likewiſe: But in Mr. *Le Blon*'s new Way of weaving Tapeſtry in the Loom with a Draw-boy, Tapeſtry may be performed almoſt as expeditious as fine Brocades: For when the Loom is once ſet and mounted, any common Draught-Weaver, though not acquainted with Drawing nor Painting; nay, hardly knowing what Figure he is about, exactly produceth what the Painter hath repreſented in the original Pattern, And thus a Piece of Tapeſtry may be woven in a Month or two, which, in the common Way of Working, would take up ſeveral Years: And what in the common Way coſts a thouſand Pounds: may, by this means, be afforded finer and better for a hundred. Therefore, it is likely, this woven Tapeſtry may become a currant Merchandize; and that many thouſand induſtrious Families may be well employed about it.

The main Secret of this Art conſiſts in drawing the Patterns, from which any common Draught-Weaver can mount the Loom; and when that is done, the Piece may be made of any Size, by only widening the Reeds and the Warp; and a Reverſe may be made with the ſame Eaſe; which is done by the Boy's pulling the Laſhes up again in the ſame Order in which he pulled them down before; by which Contrivance the Tapeſtry may be ſuited to any Room, whether the Light comes in on the right Hand, or on the left.

The Patterns are painted upon Paper, whereon are printed Squares from Copper-Plates, and theſe ſubdivided by as many Lines as anſwer to the Threads of the Warp, which run length-wiſe of the Piece; then they try how many Threads of the Shoot anſwer in Breadth to every Subdiviſion of the Squares. Every Thread of the Warp goes through a ſmall Braſs Ring called a *Male*, or through a Loop in the Leiſh, and hath a ſmall long Weight or Lingoe hung below, to counter-balance the Packthreads, which going from the Top of the Rings or Loops, are paſſed over the Pullies in the Table directly over the Loom, and are continued nearly in a Horizontal Poſition on one ſide of the Loom, to a convenient Diſtance; where they are all ſpread on a Croſs-piece faſtened to two Staples: Theſe are called the Tail of the Mounture; and from each of theſe Packthreads, juſt by the ſide of the Loom, are faſtened other Packthreads called Simples, which deſcend to the Ground; ſo that by pulling theſe ſimple Chords, you raiſe any of the Threads of the Warp at Pleaſure; wherefore they faſten a Loop or Potlart to as many of theſe ſimple Chords as there are Threads of the Warp to be pulled up at every Shoot, or every Throw of the Shuttle; by which means the Shoot ſhews itſelf on the right Side, where the Warp is pulled up: And in ordering this, they are guided by the Pattern, on which they count the Diſtances of the Subdiviſions, which contain the ſame Colours in the ſame Line, and can be ſhot at once: Then they faſten Potlarts to the ſeveral ſimple Chords, that draw up the Rings, through which thoſe Threads of the Warp run, which are to lie behind this Colour; they tie all theſe Loops together, and faſten a Piece of Worſted or Silk to the Knot, of the ſame Colour that the Workman is to throw; and the Boy, when he pulls each Loop, names the Colour, that the Weaver may take the proper Shuttle, and ſo on for every Colour to be thrown.

The End of the Firſt Part.